Bank Deregulation and Stock Price Crash Risk

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Abstract

This paper examines the relation between bank branch deregulation and corporate borrowers' stock price crash risk. Using a large sample of U.S. public firms over the period 1962–2001, we provide robust evidence that intrastate branch reform reduces firms' stock price crash risk. Further analysis shows that the negative relation between bank branch deregulation and crash risk is more pronounced among firms that are more dependent on external finance and lending relationships as well as firms that have weaker corporate governance and greater financial constraints. Our findings are consistent with the notion that bank branch reform improves bank monitoring efficiency, thereby reducing borrowing firms' bad news formation and bad news hoarding and hence their stock price crash risk. Overall, our results suggest that, as a reform 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; bad news hoarding; bad news formation.

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1. 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 shows that bank branch deregulation has significantly changed regional banking market structures and promoted economic growth (Jayaratne and Strahan, 1996, 1998; Berger et al., 1999; Kroszner and Strahan, 1999; Black and Strahan, 2002).[1] Meanwhile, a recent and growing strand of literature has documented significant effects of branching reform on corporate borrowers' behavior, such as firm financing and investment decisions (Zarutskie, 2006; Rice and Strahan, 2010), entrepreneurship (Black and Strahan, 2002; Ceterolli and Strahan, 2006; Kerr and Nanda, 2009), and innovation (Chava et al., 2013; Cornaggia et al., 2015; Hombert and Matray, 2017). However, much less is known about whether and how such reform in the banking sector (the loan market) may generate a spillover effect in the equity market, affecting the downside risk of corporate borrowers' stock value. This study attempts to fill this literature void by investigating the impact of bank branch deregulation on firms' stock price crash risk.

Bank deregulation may affect corporate borrowers' stock price crash risk in two opposite ways. On the one hand, intrastate branching deregulation may reduce the likelihood of stock price crashes by facilitating banks to efficiently monitor borrowing firms and constrain them from withholding bad news, a major driver of firm-specific crash risk.[2] As a result of the branching

[1] Evidence suggests that the banking system becomes more integrated after bank deregulation, which stabilizes economic growth (Morgan et al., 2004). Moreover, bank branch reform mitigates income inequality by boosting incomes in the lower part of the income distribution (Beck et al., 2010).

[2] Previous research shows that managers with privileged access to private information have incentives to withhold bad news or opportunistically manage the timing of disclosing such news (Jin and Myers, 2006; Kim et al., 2011a, 2011b; Hong et al., 2017). Although managers can accumulate adverse news for an extended period, they will eventually reach a tipping point, beyond which the cost of hoarding bad news exceeds the benefit of doing so. At this point the previously hidden adverse information will be made public, leading to a stock price crash (Kim et al., 2011a, 2011b).

reform between the 1970s and 1990s, the banking industry consolidated through the acquisitions of many small banks, which were incorporated as branches into larger and more complex banking organizations, thus providing an important selection mechanism to remove less efficient banks (Jayaratne and Strahan 1996, 1998; Strahan, 2003). Post-deregulation banks are under pressure to improve loan monitoring and screening in the face of fierce competition and/or the threat of takeover. Indeed, Jayaratne and Strahan (1996, p. 641) conclude that, following branching reforms, "banks do not necessarily lend more, but they appear to lend better". Furthermore, bank deregulation leads to greater bank efficiency and better monitoring using borrowers' hard information by large banks. Unlike their smaller counterparts, larger banks have wider networks, better diversification, and enjoy a comparative advantage in collecting and processing quantitative information at lower transaction costs (Stein, 2002; Petersen and Rajan, 2002; Berger et al., 2005; Liberti and Petersen, 2019). Moreover, to the extent that intrastate deregulation enhances banks' market power and thus their screening and monitoring capacity, it will improve borrowing firms' performance (Delis et al., 2017), reducing these firms' incentive to withhold adverse information and lowering the likelihood of bad news formation (e.g., Chang et al., 2017). The above arguments predict a negative relationship between bank deregulation and corporate stock price crash risk.

On the other hand, the competing view predicts that bank branch deregulation may increase corporate borrowers' crash risk. Branch reform does not only increase the average size and hierarchy of banks but also intensifies banking competition (Black and Strahan, 2002), thus shifting the nature of lending from relationship-based to arm's length (Petersen and Rajan, 1994). Compared to large banks with diversified loan portfolios, small local banks in the relationship-based system have a more concentrated exposure to a sector or a region, hence stronger incentives to collect and verify soft, private information (Berger et al., 2017a; Berger et al., 2017b). Those banks can collect this type of information through frequent personal interactions and observations with borrowing firms, which helps mitigate informational frictions between them and these borrowers (Petersen and Rajan, 2002; Agarwal and Hauswald, 2010; Li et al., 2019). However, the advantage of banks in collecting and processing soft, private information decreases after branching reforms. As mentioned above, the consolidation activities in the banking system post-deregulation saw the emergence of large, hierarchical banking organizations, which damaged lending relationships and bank monitoring based on this form of lending (Hombert and Matray, 2017). To the extent that bank deregulation impairs banks' ability to acquire borrowing firms' soft, private information, it may encourage these firms' bad news withholding and increase their stock price crash risk.

To test the above opposing views about the impact of bank deregulation on corporate borrowers' future stock price crash risk, we exploit the staggered passage of intrastate branch reform by various U.S. states between the 1970s and 1990s as a quasi-natural experiment. We perform our empirical analysis using a difference-in-differences (DID) model. Using a large sample of U.S. public firms from 1962 to 2001, we find a negative relation between bank branch deregulation and firm-specific stock price crash risk. The impact of bank deregulation is not only statistically significant but also economically meaningful. Our estimates suggest that intrastate deregulation reduces corporate borrowers' stock price crash risk, as proxied by conditional negative skewness (*NCSKEW*) and the natural log of down-to-up volatility (*DUVOL*) of firm-specific weekly returns, by 14% and 12.7% of their sample means, respectively. These results are consistent with the first hypothesis that bank branch reform improves bank monitoring and hence reduces firm-level stock price crash risk.

We perform several robustness tests to ascertain the validity of our quasi-natural experiment and strengthen our statistical inference. Importantly, we find that there is no evidence of pre-treatment trends or reverse causality and that the significant decrease in stock price crash risk is only observed post-deregulation. Our results continue to hold in propensity score matching (PSM) analysis and a host of robustness checks with various fixed effects and additional controls, thus further alleviating the concerns about possible confounding effects and omitted variables driving our results. Taken together, these analyses provide strong support for a causal interpretation of a negative effect of bank branch deregulation on firms' stock price crash risk.

We then conduct various cross-sectional analyses to provide evidence on the possible mechanisms driving the relation between bank deregulation and stock price crash risk. To this end, we first exploit the variations in borrowing firms' degree of dependence on external finance and lending relationships. Our tests are motivated by the extant literature deeming bank branch deregulation as an exogenous shock to credit supply (e.g., Black and Strahan, 2002; Amore et al., 2013) and lending relationships (e.g., Hombert and Matray, 2017). If intrastate deregulation indeed affects the stock price crash risk of borrowing firms through the monitoring mechanism, this effect should be more noticeable among firms with greater dependence on external finance, particularly bank loans, because such firms are more susceptible to intensive bank monitoring. In the same vein, to the extent that branching deregulation improves bank monitoring and reduces crash risk via the increased use of corporate borrowers' hard information, in place of soft information, the impact of such deregulation should be more conspicuous for firms that were previously monitored with soft information, that is, those firms dependent on relationship lending. Consistent with these expectations, we find that the mitigating effect of bank deregulation on stock price crash risk is more pronounced for firms with greater reliance on external finance and lending relationships.

We next investigate whether and how the impact of bank deregulation on stock price crash risk is affected by borrowing firms' corporate governance and financial constraints. If bank branch reform reduces borrowers' crash risk through enhanced bank monitoring, we would expect this effect to be more pronounced for firms with weaker governance and greater financial constraints, in which the agency problem tends to be more severe. Weakly governed firms typically have less accountability and transparency (Bhojraj and Sengupta, 2003; Bae et al., 2006) while financially constrained firms have greater incentives to withhold adverse information to enhance their access to external capital (Hutton et al., 2009; Li and Zhan, 2019). Those firms tend to face a high likelihood of stock price crashes and will benefit most from more efficient bank monitoring post-deregulation. We find evidence consistent with this prediction.

Finally, we attempt to provide more direct evidence on the channels through which bank deregulation affects firms' stock price crash risk. In particular, we study the impact of bank branch reform on firm-level measures of bad news hoarding and bad news formation proposed by recent research (e.g., Kim and Zhang, 2014, 2016; Li and Zhan, 2019). We find that bank deregulation leads to higher accounting conservatism and a lower likelihood of financial restatement, consistent with bank deregulation reducing stock price crash risk by constraining borrowing firms' bad-news-hoarding behavior. Meanwhile, there is evidence that intrastate bank deregulation improves future firm profitability, earnings surprise, and investment efficiency, consistent with bank deregulation improving firms' fundamentals and reducing their likelihood of bad news formation. Taken together, these findings provide empirical support to both channels: bad news hoarding and bad news formation.

This paper contributes to the literature in several ways. First, it adds to the literature on the economic consequences of bank deregulation by documenting the real effects of branching reforms on firm-specific stock return distributions. Prior studies show how bank branch deregulation affects borrowing firms' various corporate decisions (e.g., Black and Strahan, 2002; Ceterolli and Strahan, 2006; Zarutskie, 2006; Rice and Strahan, 2010; Chava et al., 2013; Cornaggia et al., 2015; Hombert and Matray, 2017; Bai et al., 2018). However, those studies largely exploit bank branch reform as a regulatory shock to bank competition and credit supply. Although Jayaratne and Strahan (1996, 1997) argue that intrastate branching deregulation fundamentally altered the nature of bank monitoring, 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 exogenous changes to bank monitoring. At a broader level, our analysis shows a structural change in the banking industry through intrastate deregulation may introduce positive externalities and spillover effects across capital markets, as it allows bank monitoring in the loan market to contribute to shareholder wealth protection in the equity market.

Second, our study adds to the growing literature on stock price crash risk. Recent research has documented several firm-specific factors affecting crash risk, such as financial reporting quality (Hutton et al., 2009; Kim et al., 2016; Kim and Zhang, 2016; Ertugrul et al., 2017; Kim et al., 2019), tax avoidance (Kim et al., 2011b), and innovation (Jia, 2018). This literature has also revealed several other determinants of crash risk associated with managerial bad-news-hoarding activities or the likelihood of bad news formation, including equity-based executive compensation (Kim et al., 2011a; Xu et al., 2014), religiosity (Callen and Fang, 2015), stock liquidity (Chang et al., 2017), CEO age (Andreou et al., 2016), employee welfare plans (Ben-Nasr and Ghouma, 2018), product market competition (Li and Zhan, 2019), top executive gender (Li and Zeng, 2019), powerful CEOs (Al Mamun et al., 2020), among others. However, one major challenge facing this stream of research is that the determinants of stock price crash risk are largely endogenously linked with unobserved firm and/or managerial characteristics, making statistical inference difficult. By exploiting a quasi-natural experiment based on the staggered passage of bank branch deregulation, we can arguably establish the causal effect of important regulatory changes in the banking system on corporate crash risk.[3] More broadly, our study also adds to the current limited understanding of how the market structure of the financial industry affects firms' disclosure incentives.

In a related study, Kim et al. (2019) examine the role of banks in mitigating firms' stock price crash risk. Our paper differs from this study in two important aspects. First, Kim et al. (2019) focus on *interstate* branch deregulation, whereas our study investigates the role of *intrastate* branch deregulation in lowering corporate borrowers' crash risk. Following the former reform, states gradually lifted branching restrictions for bank holding companies to expand beyond state boundaries. However, previous studies show that, compared to intrastate branching reform, interstate deregulation typically has a more limited impact on the structure of the banking sector as well as the costs of intermediation and hence the quality of loan monitoring and screening (Amel and Liang, 1992; Calem, 1994; McLaughlin, 1995; Jayaratne and Strahan, 1996; Strahan, 2003). By focusing on the passage of intrastate deregulation, we can better disentangle the effect of an exogenous shock to bank monitoring from any other systematic change in banks' ability to diversify geographically.[4] More importantly, while both Kim et al. (2019) and our study find a negative link between bank deregulation and crash risk, Kim et al. (2019) mainly attribute this result to increased bank monitoring reducing bad news withholding. We find that the decrease in

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[3] Recent research has started to investigate new drivers of crash risk using quasi-natural experiments for identification purposes (e.g., Ali et al., 2019; Li and Zhan, 2019; Balachandran et al., 2020; Deng et al., 2020).

[4] Chava et al. (2013) show that interstate and intrastate deregulation may have contrasting effects on the local market power of banks and a potential opposite impact on corporate policies and outcomes. As an additional robustness check, we follow Chava et al. (2013) and run a horse-race regression by simultaneously controlling for both forms of bank deregulation. Our analysis documents a negative and significant relation between intrastate deregulation and firms' stock price crash risk but an insignificant relation between interstate deregulation and crash risk. This is consistent with the former type of reform playing a more profound role in improving bank intermediation efficiency than the latter (Jayaratne and Strahan, 1996; Beck et al., 2010).

crash risk is driven by both bad news hoarding and bad news formation. In addition, we document evidence of the bank monitoring mechanism. In one of our cross-sectional tests, we show that the effect of bank branch reform on crash risk is more pronounced for firms with weaker corporate governance, for which bank monitoring is more important. Overall, our study provides an arguably more complete picture of the association between bank deregulation and stock price crash risk.

In another contemporaneous paper, Jiang et al. (2020) examine the impact of *interstate* deregulation on corporate risk. Beside the fact that we examine the impact of *intrastate* deregulation, our study differs from this paper in two important ways. First, we focus on firm-level stock price crash risk, while Jiang et al. (2020) study firm risk, measured as 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). Although managers can mask firm risk levels by hiding information about the volatility of underlying earnings from outside investors, thereby reducing corporate earnings volatility, such behavior may exacerbate stock price crash risk. Second, in terms of the channels explaining the results, Jiang et al. (2020) argue that interstate deregulation reduces firm risk via intensified competition among banks and the relaxation of financing constraints. We find that intrastate branching deregulation mitigates firms' stock price crash risk through improved bank monitoring efficiency (Jayaratne and Strahan, 1996). Our result also holds after controlling for various measures of firm risk, thus ruling out the concern that our inference is driven by firm risk and risk-taking behavior.

The rest of the paper proceeds as follows. Section 2 briefly introduces the background of intrastate branching reforms and develops the hypotheses. Section 3 discusses data and research design. Section 4 presents empirical results from the main analysis. We discuss results from additional analyses in Section 5 and verify the channels in Section 6. Section 7 concludes.

2. Institutional background and hypothesis development

2.1. Bank branch deregulation

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 U.S. 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, with some banks allowed to run unit offices. For example, prior to 1987, Texas, a typical regulated state, had a substantial number (hundreds) of banks but a limited number of branches, while California, a deregulated state, had a handful of banks but numerous branches.

Up to the 1970s, only 12 states had allowed unrestricted statewide branching. The other 38 states progressively relaxed their branching restrictions between the 1970s and 1994, before the passage of the Interstate Banking and Branching Efficiency Act (IBBEA). Two forms of branching restrictions were lifted in the 1970s through the 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]

2.2. Hypothesis development

We argue that lifting restrictions on bank branching may affect corporate borrowers' stock price crash risk through enhanced bank monitoring. We develop two competing hypotheses. On the one hand, bank branch deregulation improves bank monitoring efficiency, allowing banks to curb borrowing firms' bad-news-hoarding behavior as well as reducing their likelihood of bad news formation, hence lowering these firms' stock price crash risk. There are two reasons why bank monitoring is enhanced as the result of bank branch reform, namely the consolidation in the banking sector and a shift in the nature of lending and monitoring. First, Jayaratne and Strahan (1997) suggest that branching restrictions retarded the "natural" evolution of the banking industry by preventing better-run banks from establishing branches.[5] 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; 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 reducing loan losses (Kroszner and Strahan, 1997, 1999; Dick and Lehnert, 2010). Jayaratne and Strahan (1996) conclude that, post-deregulation, there is a significant improvement in bank screening and monitoring.

Second, the improved bank monitoring following branching reforms can also be explained by the emergence of large, hierarchical banks and a shift in the nature of lending, from a relationship-based system to one based on arm's length. Diamond (1984) argues that large, better-

[5] Economides et al. (1996) show that states with many small, poorly capitalized banks supported the 1927 McFadden Act, which gave them the primary authority over national banks' ability to branch.

diversified banks have greater incentives and capabilities to monitor borrowers. Importantly, compared to their smaller counterparts, such banks are better equipped to collect and process borrowers' hard information, which is more standardized and verifiable (Stein, 2002; Berger et al., 2005; Liberti and Petersen, 2019). Thus, large banks can make decisions based on borrowers' quantitative information and monitor them at relatively lower transaction costs than small banks. Petersen and Rajan (2002) find that by relying on hard information, loan officers do not have to make regular visits to the borrowing firms but can still effectively process these firms' financial histories, credit reports, and scoring methods. Chen and Vashishtha (2017) show that bank mergers that create more complex and hierarchical organizations lead to increased disclosures by corporate borrowers. Overall, by efficiently acquiring and processing borrowing firms' hard, quantitative information, post-deregulation banks can enhance their screening and monitoring capacity.

Enhanced bank monitoring moderates borrowing firms' crash risk through two channels. First, since banks typically have strong incentives to detect corporate borrowers' accounting irregularities in a timely manner (Fama, 1985; Diamond, 1991), greater bank monitoring efficiency will further help constrain managerial bad-news-hoarding activities and reduce firms' crash risk. In addition, a better screening and monitoring capacity or effort by banks with stronger market power (Chan et al., 1986; Caminal and Matutes, 2002) following the consolidation in the banking sector can help enhance the performance of the borrowing firms (Delis et al., 2017). To the extent that bank monitoring improves firm fundamentals and performance, it reduces those firms' likelihood of bad news formation and lowers their crash risk (Chang et al., 2017; Li and Zhan, 2019). Overall, the above arguments allow us to develop the following hypothesis:

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

On the other hand, branch deregulation may exacerbate borrowing firms' bad news withholding and crash risk by weakening banks' ability to access these firms' soft, private information. Prior to bank branch deregulation, the banking system was primarily relationship-based, featuring interpersonal linkages between small banks and borrowers. In this system, small local banks typically had more concentrated portfolios in a sector or a region so that they had strong incentives to analyze more soft, private information about borrowers (Berger et al., 2017a; Berger et al., 2017b). Having close lending relationships with borrowers also gave banks an informational advantage (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 contacts with borrowing firms. Unlike hard information, such soft, qualitative information is difficult to verify and can barely be communicated in numbers (Petersen and Rajan, 1994). In a similar vein, Liberti and Petersen (2019) suggest that lending relationships play a useful role in eliciting private information, given that a loan officer can use his/her discretion to more accurately evaluate a long-term borrower's creditworthiness.

However, bank deregulation encourages banking competition among numerous small local banks and a handful of large, diversified banks (Black and Strahan, 2002; Stiroh and Strahan, 2003), consequently damaging lending relationships and transforming the banking industry from a relationship-based to an arm's-length system (Hombert and Matray, 2017). To the extent that an arm's-length system restricts large, complex banking organizations' ability to collect, process, and transmit borrowers' soft, private information (Skrastins and Vig, 2019), bank branch reform may provide borrowing firms with more opportunities to accumulate negative information, thus exacerbating these firms' stock price crash risk.[6] Overall, in light of the above arguments, we formulate the competing hypothesis as follows:

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

3. Data and methodology

3.1. 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 intrastate branching deregulation. Following prior studies (e.g., Hutton et al., 2009; Kim et al., 2011a, 2011b; Kim et al., 2016; Chang et al., 2017), we exclude financial firms, firms with year-end share prices below $1, those with fewer than 26 weeks of stock return data in the fiscal year, firm-year observations with negative total assets and book values of equity, and those 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).

3.2. Measuring bank branch deregulation

Consistent with Jayaratne and Strahan (1996), we set the date of bank branch reform based on the year in which a state permitted branching via M&As through the holding company structure

[6] Intrastate deregulation may have negative and unintended consequences for the corporate sector, leading to weakening firm outcomes. Chava et al. (2013) document a decline in the supply of credit for young and private firms, thus restricting their innovative activities post-deregulation. Hombert and Matray (2017) also report that the number of innovators decreases following bank branch reform. These changes in firm fundamentals may have also resulted in stronger incentives for firms to withhold adverse information.

or *de novo* branching. Our main test variable, the bank branch deregulation indicator (*BRANCH*), is a dummy variable that equals one if the state in which a firm is headquartered has implemented intrastate branching deregulation and zero otherwise. As mentioned, Table 1 shows the timeline of bank branch deregulation events across states.[7]

3.3. 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 effects from weekends and Mondays (Wang et al., 1997). 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

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

[7] 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 banks headquartered in those states were heavily affected by laws that provided a tax incentive for credit card banks to operate. 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.

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 the natural logarithm of "down-to-up volatility" (*DUVOL*), which is calculated as follows:

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

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. Compared to *NCSKEW*, this alternative measure of crash risk may be less influenced by a handful of extreme returns as it does not involve the third moments (Chen et al., 2001).

3.4. Control variables

Following prior literature (e.g., Chen et al., 2001; Jin and Myers, 2006; 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 defined 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 stocks with a higher intensity of differences in 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 measured 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 defined 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 ($ACCM_t$) is defined as the absolute value of discretionary accruals, which are the residuals estimated from the modified Jones model (Dechow et al., 1995). As reviewed above, financial reporting opacity is positively associated with future stock price crash risk (Hutton et al., 2009). 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% percentiles of their distributions.

4. Empirical results

4.1. Descriptive statistics

Table 2 presents the summary statistics of all variables used in our main regressions. Regarding 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; Chang et al., 2017), and thus are not discussed herein to preserve space.

[Insert Table 2 about here]

4.2. Baseline specification and results

Our baseline regression model captures the relationship between bank branch deregulation and firm-specific stock price crash risk. The specification 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 defined in year t. The independent variable of interest is $BRANCH_t$, the bank branch deregulation indicator. Since our quasi-natural experiment exploits the staggered introduction of bank deregulation across states, the specification we use is a generalized DID model. The effect of bank deregulation on stock price crash risk is estimated as the difference in the changes in firms' stock price crash risk before and after deregulation, between the treatment and control groups. The treatment group consists of firms headquartered in states that implemented the bank branch reform while the control group includes firms headquartered in states that had not experienced such reform. In our baseline regressions, we control for year and state fixed effects and cluster standard errors at the state level, that is, the level at which *BRANCH* is defined. Including state fixed effects helps address the concern that time-invariant omitted variables that generate variation in a state's stance toward openness to bank branching might be correlated with the stock price crash risk of firms headquartered in the state (treatment firms).

Panel A of 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 indicator, $BRANCH_t$, 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 coefficients on $BRANCH_t$ remain significant and negative for both crash risk measures (*t-stat* = –3.05 in Column (3) and –3.34 in Column (4)). This finding suggests that intrastate branching deregulation reduces firms' future stock price crash risk, consistent with the first hypothesis (*H1a*) that improved bank monitoring post-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 following a common approach used in prior literature on bank deregulation (e.g., Chava et al., 2013) and stock price crash risk (e.g., DeFond et al., 2015; Deng et al., 2020). The coefficients on $BRANCH_t$ in Columns (3) and (4) of Panel A Table 3 indicate that, holding other factors unchanged, $NCSKEW_{t+1}$ ($DUVOL_{t+1}$) decreases by about 0.028 (0.015) post-deregulation. Given that the sample mean values of $NCSKEW_{t+1}$ and $DUVOL_{t+1}$ are –0.200 and –0.118, respectively, one interpretation of our results is that bank branch deregulation leads to a 14% (12.7%) reduction in stock price crash risk.[8] Overall, the negative association between intrastate deregulation and stock price crash risk is not only statistically significant but also economically meaningful.[9]

[8] Since *DUVOL* is the natural log of down-to-up volatility, an alternative interpretation of our results is that bank deregulation leads to a 1.5% drop in the down-to-up volatility, which implies a modest economic impact. However, the interpretation presented in the text is preferred as it takes into account how *DUVOL* is constructed and interpreted as a crash risk measure in its entirety and allows for a direct comparison with existing research (e.g., Deng et al., 2020).
[9] Economic impact can also be estimated by accessing the documented effect relative to the interquartile (IQ) spreads of the test variable of interest or crash risk measures. Since our independent variable (*BRANCH*) is an indicator, we are unable to apply the former approach as previous studies (Hutton et al., 2009; Callen and Fang, 2015). Using the latter approach (i.e., based on the IQ spreads of the crash risk measures), bank deregulation is associated with a modest economic effect of 3.3% to 3.7%. However, this approach seems less commonly used in existing research and may pose a challenge in terms of comparing the economic impact across studies.

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. Consistent with Hutton et al. (2009), *$ACCM_t$* is significantly and 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; Callen and Fang, 2015).

Although we have included state fixed effects to control for time-constant unobserved state-level factors that may be associated with (state-level) bank branch reform, a concern remains that our inference may still be affected by industry and firm heterogeneity. Hence, we perform additional regressions where we separately include industry and firm fixed effects to account for time-invariant industry- and firm-level heterogeneity. Panel B of Table 3 reports the results from those fixed-effects regressions. The coefficients on *BRANCH* continue to be significant and negative, which is consistent with our baseline findings and suggests that our main statistical inference is unlikely to be affected by heterogeneity bias.

[Insert Table 3 about here]

4.3. Addressing endogeneity concerns and robustness checks

4.3.1. Pre-treatment trends analysis

Our identification is based on the idea that the staggered deregulation of bank branching restrictions represents an exogenous shock to bank monitoring efficiency, thus affecting firms' stock price crash risk. However, one concern with this strategy is that, although we have controlled for state fixed effects in the main specification, there may still remain omitted (time-varying) state-level factors that could potentially trigger the deregulation in different states. There might also be a reverse causality problem if firms' stock price crash risk systematically differs across states and such variation affects the timing of bank deregulation in those states. Following Bertrand and Mullainathan (2003) and Cornaggia et al. (2015), we address these concerns by investigating the dynamic trends of stock price crash risk surrounding the deregulation events. If pre-treatment trends and reverse causality indeed exist, we should observe significant changes in stock price crash risk prior to such events.

We employ two specifications to test whether firms headquartered in states that passed bank deregulation laws (i.e., the treatment group) and those that had not yet passed them (i.e., the control group) follow parallel pre-treatment trends. First, as Cornaggia et al. (2015), we construct four dummy variables indicating four periods around a bank deregulation event: $Before^{2+}$, $Before^{1}$, $After^{1}$, and $After^{2+}$. $Before^{2+}$ takes one for the period more than one year before deregulation; $Before^{1}$ takes one for the one year prior to deregulation; $After^{1}$ takes one for the one year post-deregulation; and $After^{2+}$ takes one for the period more than one year after 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, as well as 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.

Next, we follow Hombert and Matray (2017) and estimate a more stringent specification that includes four indicator variables: $Before^{5+}$, $Before^{1,4}$, $After^{1,4}$, and $After^{5+}$. $Before^{5+}$ takes one for the period more than four 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 the period more than four years after deregulation. The estimation results in Columns (3) and (4) of Table 4 show a similar pattern to those in Columns (1) and (2). Again, the coefficients on the 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. Taken together, the results across different models show no evidence of pre-treatment trends and reverse causality in firms' stock price crash risk.

[Insert Table 4 about here]

Following Beck et al. (2010), we graphically examine the dynamic impact of bank branch deregulation on stock price crash risk and present the 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 track 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. The coefficients on the deregulation dummy variables are insignificant for all years before deregulation, again suggesting no pre-treatment trends in crash risk. The mitigating impact of bank 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 in Table 4 and Figure 1 show no evidence of pre-existing trends in firms' stock price crash risk. Collectively, these results help validate the important assumption about parallel trends and mitigate the concern about reverse causality.

[Insert Figure 1 about here]

4.3.2. Propensity score matching analysis

Next, to balance the observed covariate differences between the treatment and control groups, we repeat our DID estimation using a propensity-score-matched sample (DeFond et al., 2014). As in Dehejia and Wahba (2002), we perform one-to-one matching, without replacement, to the nearest neighborhood, based on industry, state, year, and all control variables specified in our baseline model (Eq. (4)).[10] We identify 6,533 pairs of pre- and post- deregulation firm-years in the treatment and control groups. Panel A of Table 5 compares the characteristics of firms in both groups. The results show that all the univariate differences in the firm characteristics are statistically insignificant, suggesting that any difference in crash risk between the treatment and control groups should be due to bank deregulation, rather than 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 firms are significantly higher than those for control firms, consistent with our main result. In Panel C of Table 5, we re-estimate Eq. (4) using the propensity-score-matched sample. The regression results are in line with our baseline finding that bank branch deregulation exerts a mitigating effect on stock price crash risk. Overall, the results from the PSM analysis lend further support to our main finding.

[Insert Table 5 about here]

[10] 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 those reported in Table 5.

4.3.3. Robustness tests

To establish the robustness of our main finding, we perform a battery of robustness checks. To economize on space, we report and discuss the results from these tests in a self-contained internet appendix. In summary, we show that our finding continues to hold in (1) regression analysis with additional firm- and state-level variables, including several (state-level) economic, legal, and institutional factors (see Section 1 and Table IA-1 of the internet appendix); (2) additional tests controlling for unobserved shocks and placebo effects (see Section 2, Tables IA-2 and IA-3 and Figure IA-1); (3) alternative PSM analysis with treatment and control firms matched on the degree of external finance dependance (see Section 3 and Table IA-4); and (4) various regression analysis using alternative samples, different sample periods, shorter test windows (i.e., windows of three, five, and ten years before and after deregulation events), alternative crash risk and deregulation measures, longer forecasting periods, and alternative data on headquarters locations to calculate the test variable (see Section 5 and Tables IA-6 and IA-7). In addition, using a Hazard model to investigate factors affecting the timing of bank deregulation, we find that intrastate branching reform is plausibly exogenous as the timing of the deregulation events is uncorrelated with stock price crash risk (see Section 4 and Table IA-5).

5. Cross-sectional tests

Thus far we have shown a robust negative effect of bank deregulation on firms' stock price crash risk. In this section, we attempt to provide further evidence on the possible economic mechanism driving this effect, namely enhanced bank monitoring. To this end, we study how the association between intrastate deregulation and crash risk varies with the degree of external finance dependence, lending relationships, corporate governance, and financial constraints.

5.1. The role of external finance dependence

To the extent that lifting intrastate branching restrictions significantly changed the structure of the banking industry, improved bank monitoring (Jayaratne and Strahan, 1996, 1998), as well as increased credit supply (e.g., Black and Strahan, 2002; Amore et al., 2013), firms that are more dependent on external finance, especially those becoming more reliant on bank loans, should experience more intensive bank monitoring. To test this argument, we partition the whole sample into two groups based on the degree of external finance dependence. We expect to observe a more pronounced impact of bank branch deregulation on stock price crash risk for firms with a higher degree of external finance dependence.

We measure the degree of external finance dependence using three proxies: the external finance dependence ratio (*EXDEP*), net change in debt capital (*NCD*), and bank loan ratio (*BANKLOAN*). Following Rajan and Zingales (1998), external finance dependence is defined as the amount of desired investment that cannot be financed through internal sources; we calculate it 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 debt capital as long-term debt issuance minus long-term debt reduction, scaled by total assets. Our third, and most direct measure, the bank loan ratio, is the amount of cumulative bank loan scaled by total assets.[11] In our regression analysis, we construct three indicators based on those measures to reflect firms highly dependent on external finance, namely those with an above-median external finance dependence ratio, net change in debt capital, and bank loan ratio.

[11] Our bank loan data is from the Loan Pricing Corporation DealScan database, which contains comprehensive historical information on (syndicated) loan pricing and contracts details. However, because the data is less available before 1988, the number of observations used is smaller than in the main analysis.

Since the three variables capture firms' demand for external finance, particularly bank loans, they also reflect their sensitivity to bank monitoring post-deregulation.[12]

In Table 6, we run triple-difference regressions where we interact the bank branch deregulation variable (*BRANCH*) with the indicators capturing the degree of external finance dependence, as defined above. The results across all models show that the coefficients on the interaction terms of interest are generally significant and negative, suggesting that the mitigating effect of bank deregulation on stock price crash risk is stronger for firms with greater dependence on external finance, particularly bank loans. To the extent that these firms are subject to more effective bank monitoring, our results provide additional support for the monitoring mechanism.

[Insert Table 6 about here]

5.2. The role of lending relationship dependence

We next investigate how the association between borrowing firms' stock price crash risk and bank deregulation varies with these firms' degree of dependence on lending relationships. As discussed earlier, intrastate branching deregulation transformed the banking industry, from a relationship-based to an arm's length-oriented system, thus fundamentally altering the type of bank monitoring, from monitoring based on soft, qualitative information to one based on hard, quantitative information (e.g., Petersen and Rajan, 1992; Hombert and Matray, 2017). If branch reform allows banks to enhance their monitoring efficiency by collecting and processing borrowing firms' hard information rather than soft information, then the impact of such reform on borrowers' crash risk should be more pronounced for firms that were previously monitored with soft information, that is, those firms dependent on lending relationships.

[12] As an additional robustness check, we define measures of external finance dependence at the industry level following Hombert and Matray (2017). The results remain qualitatively the same.

To test the above prediction, we follow Hombert and Matray (2017) and obtain data from the National Survey of Small Business Finances (1987 and 1998). We employ three industry-level (two-digit SIC) proxies for lending relationship dependence, namely, the average distance between firms and their main lenders in 1987, the average increase in the distance between banks and their borrowers between 1987 and 1998, and the average length of the relationship between banks and borrowers in 1987. A greater distance between banks and borrowing firms or an increase in such distance indicate that their interaction becomes more impersonal, with bank monitoring more dependent on firms' hard information (Petersen and Rajan, 2002). In our regression analysis, we construct three indicator variables to 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, *AVDIS* (*GROWDIS*), or if the average length of the relationship is above the sample median, *AVLENGTH*.

In Table 7, we perform triple-difference regressions, where we regress each crash risk measure on bank branch deregulation (*BRANCH*) and its interaction with each of the three indicators proxying for lending relationship dependence. The coefficients on the interaction terms of interest are significant and negative, suggesting a more pronounced effect of bank deregulation for firms with stronger relationship lending. Consistent with our expectation, following intrastate branching deregulation, corporate borrowers more dependent on lending relationships are subject to more effective bank monitoring based on hard information and thus experience lower crash risk. These results are again consistent with the bank monitoring mechanism.

[Insert Table 7 about here]

5.3. The role of corporate governance

Our previous discussion and analysis suggest that the mitigating effect of bank deregulation on crash risk is mainly attributable to improved bank monitoring efficiency. To provide additional and more direct evidence on this mechanism, we conduct a subsample analysis conditioning on the strength of corporate governance. We expect the documented effect of bank branch reform on crash risk to be more pronounced among borrowers with weaker corporate governance, for which the role of bank monitoring is more important. In weakly governed firms, there is less accountability on the part of managers for not releasing timely and high-quality information (Bhojraj and Sengupta, 2003; Bae et al., 2006). Such firms will, therefore, benefit most from greater bank monitoring efficiency post-deregulation.

To test this prediction, we follow previous research and measure corporate governance using two proxies: institutional ownership (An and Zhang, 2013) and the G-index (Gompers et al., 2003). Given that institutional ownership and the G-index are not available until 1985 and 1990, respectively, in this analysis we use a smaller sample with those states that deregulated in the 1990s. Specifically, we define a dummy variable *INSTOWN* as one for firms with below-median institutional ownership and zero otherwise. In addition, we create a dummy variable *GINDEX* that takes value one for firms with an above-median G-index and zero otherwise. A higher value of *GINDEX* indicates a lower quality of corporate governance.

We run triple-difference regressions conditional on the two governance measures defined above and report the results in Table 8. The interaction terms between both governance measures and *BRANCH* load significantly and negatively, suggesting that the impact of bank deregulation on crash risk is more pronounced for firms with weaker corporate governance. This finding lends additional support to the bank monitoring mechanism.[13]

[Insert Table 8 about here]

5.4. The role of financial constraints

We next investigate the impact of financial constraints on the relationship between bank deregulation and crash risk. Prior research suggests that financially constrained firms face a higher cost of financing when accessing capital markets (Korajczyk and Levy, 2003). Thus, constrained firms may have greater incentives to hide negative information and/or to use earnings management to raise external capital at a more favorable cost than their unconstrained counterparts (e.g., Hutton et al., 2009; Li and Zhan, 2019). To the extent that intrastate deregulation improves bank monitoring efficiency, we expect it to affect financially constrained firms by a larger extent than financially unconstrained firms. Put differently, the impact of bank deregulation on stock price crash risk should be stronger for constrained firms than their unconstrained counterparts.

To test this conjecture, we perform triple-difference regressions using three conventional proxies for financial constraints: the WW index (Whited and Wu, 2006), the KZ index (Kaplan and Zingales, 1997), and credit quality of issued bonds (Li and Zhang, 2010). The latter is also a measure of firm access to public debt markets.[14] We categorize firms as financially constrained if their WW and KZ indices are above their sample medians or if their bonds are unrated. The results reported in Table 9 show that the coefficients on the interaction terms between the above financial constraint proxies and *BRANCH* are negative and generally significant. These results are consistent

[13] In further analysis, we find that the number of covenants used by banks significantly increases after intrastate deregulation, consistent with the bank monitoring mechanism. However, we refrain from reporting the results from this analysis as the sample size is relatively small given limited data availability.
[14] While firms with rated debt can avoid stringent bank monitoring by relying more on public debt, this option is not available for firms with unrated debt. This also suggests the impact of bank monitoring on the latter firms is stronger.

with the argument that financial constraints exacerbate firms' incentives to withhold negative information such that bank deregulation plays a more important role in restraining constrained firms' bad-news-hoarding behavior and stock price crash risk. We interpret these results as further evidence of the bank monitoring mechanism.

[Insert Table 9 about here]

6. Channel tests

In this section, we attempt to provide more direct evidence on the possible channels driving our main finding, that is, more stringent bank monitoring post-deregulation reduces stock price crash risk by constraining borrowing firms' bad-news-hoarding behavior and reducing these firms' likelihood of bad news formation.[15]

6.1. Bad-news-hoarding channel

Our central argument is that intrastate branching deregulation may reduce the likelihood of stock price crashes by facilitating banks to efficiently monitor borrowing firms and constrain them from withholding bad news. To provide direct evidence on this economic channel, we study the impact of bank deregulation on two information hoarding measures commonly used in previous research. The first is firm-level conditional conservatism in financial reporting (*CSCORE*), which reflects firms' tendency to delay the recognition of (unverifiable) good news as gains while accelerating the recognition of bad news as losses (Basu, 1997). Recent research argues that a high degree of accounting conservatism is significantly and negatively associated with managerial bad news withholding (Kim and Zhang, 2016). We calculate firm-year conditional conservatism following Khan and Watts (2009) and Kim and Zhang (2016). The second proxy for information

[15] We would like to thank two reviewers for encouraging us to investigate these two economic channels.

hoarding is the likelihood of financial restatement (e.g., Cheng and Farber, 2008; Hribar et al., 2014; Lobo and Zhao, 2013). Kim and Zhang (2014) argue that financial statement restatement is a valid measure of financial reporting opacity as it depends less on the validity of the model(s) used to derive conventional measures of earnings quality. They further find a significant and positive relation between financial statement restatement and stock price crash risk. In our regression analysis, we follow their approach and define financial restatement (*REST*) as a dummy variable that equals one for restatement firms and zero otherwise.

We first regress those measures of bad news withholding on bank deregulation and some firm controls as in Li and Zhan (2019). The results are presented in Columns (1) and (4) of Table 10. Consistent with our conjecture, we find that bank deregulation leads to higher conditional conservatism and a lower likelihood of financial restatement, consistent with bank deregulation reducing stock price crash risk by constraining borrowing firms' bad-new-hoarding behavior.

Next, we perform a mediation analysis to estimate the magnitude of the economic impact via this channel. Based on the results regarding conditional conservatism in Columns (1) to (3), the products of the paths' coefficients are –0.015 (0.052 × –0.298) and –0.007 (0.052 × –0.135) for *NSKEW* and *DUVOL*, respectively, representing 54% and 46% of the total deregulation effect captured by the coefficients on *BRANCH* in Table 3 (–0.028 and –0.015, respectively). In Columns (4) to (6), using the results for financial restatement, the products of the paths' coefficients are –0.0009 (–0.010 × 0.085) and –0.0004 (–0.010 × 0.036) for *NSKEW* and *DUVOL*, respectively, which account for between 2.6% and 3% of the total deregulation effect. Overall, while the economic effect via financial restatement is somewhat modest, the impact through accounting conservatism is relatively large. Taken together, these results suggest that a substantial fraction of the total effect of bank deregulation on crash risk is via the bad-news-hoarding channel.

[Insert Table 10 about here]

6.2. Bad-news-formation channel

As argued previously, the likelihood of stock price crashes depends not only on the extent of bad news hoarding but also on bad news formation (Chang et al., 2017). Li and Zhan (2019) investigate stock crashes for S&P 500 firms in 2009 and find that the typical reasons to trigger those events include accounting restatements and disappointing firm performance. If bank deregulation improves firm profitability and other performance measures, such as investment efficiency, firms will experience stronger fundamentals and therefore a lower likelihood of bad news formation. Such firms will have less incentive to conceal adverse information, hence lower stock price crash risk. In short, we predict that crash risk decreases post-reform because key firm fundamentals and performance measures improve as the result of more efficient bank monitoring.

To test the bad-news-formation channel, we first examine the direct impact of bank deregulation on future profitability, earnings surprise, and investment efficiency. We measure profitability as return on assets (*ROA*) and earnings surprise (*SURPRISE*) as the difference between actual earnings and the last individual analyst forecast, divided by year-end stock price. We define investment inefficiency (*INEFF_INV*) as the difference between a firm's current investment and the industry-level average of investment.[16] Table 11 reports the results from regressing these performance measures on *BRANCH* and some firm-level controls. As expected, in Columns (1), (4), and (7), we find that intrastate bank deregulation improves future firm profitability, earnings surprise, and investment efficiency.

[16] The results are robust to an alternative measure of investment inefficiency, defined as the difference between a firm's current investment and the average of its investment during the past three years.

Next, we carry out mediation analysis to estimate the economic impact of bank branch reform through the bad-news-formation channel. The products of the paths' coefficients for *NCSKEW* are –0.002 (0.006 × –0.269), –0.001 (0.005 × –0.234), and –0.003 (–0.024 × 0.131), with the mediating variable being profits, earnings surprise, and investment inefficiency, respectively. These effects account for approximately 7%, 4%, and 11% of the total effect of bank deregulation on *NCSKEW* (–0.028), respectively. For the second measure, *DUVOL*, the products of the paths' coefficients are –0.001 (0.006 × –0.141), –0.001 (0.005 × –0.136), and –0.001 (–0.024 × 0.058), which account for approximately 7% of the total effect of deregulation (–0.015). Taken together, while these effects are economically more modest than the effects estimated via bad news hoarding, it is evident that the deregulation impact also occurs through the bad-news-formation channel.

To provide further evidence on bad news formation, we follow the approach used by Li and Zhan (2019) and examine whether stock price crashes are likely to take place when bad news is triggered by weakening firm performance. A firm's stock price is most likely to crash when the firm inflates its current profits, before revealing its disappointing performance in the future. Inflated contemporaneous profits may signal more information withheld or a greater likelihood of a bubble formed, which may lead to future stock price crashes (Li and Zhan, 2019). We expect the role of bank monitoring following bank branch reform to be more pertinent for firms with larger increases in their profits. On the other hand, declining future performance may indicate the stock price bubble is about to burst, leading to an imminent stock price crash. We thus expect the role of bank monitoring in reducing the crash risk of firms with declining future earnings (i.e., those with negative changes in profits) to be more important. Overall, we predict that the interaction terms between bank branch deregulation and the current (future) changes in earnings negatively (positively) impact crash risk. In Panel B Table 11, we document evidence consistent with these predictions.[17]

Overall, the results in Table 11 suggest that future stock crashes can be triggered by inflated profits, followed by weakening firm performance. Meanwhile bank deregulation helps improve firms' fundamentals and constrains bad news formation, leading to lower future stock price crash risk, consistent with the bad-news-formation channel.

[Insert Table 11 about here]

7. Conclusion

In this study, we use the staggered passage of intrastate deregulation by U.S. states as a quasi-natural experiment to investigate the impact of bank branch reform on corporate borrowers' stock price crash risk. We empirically test two competing views. The first view predicts that, because the reform encouraged consolidation in the banking sector, eliminated inefficient small banks, and improved bank intermediation, post-deregulation banks were able to monitor borrowing firms more effectively, leading to lower stock price crash risk among such firms. The alternative view argues that the branching reform damaged lending relationships and impeded banks' ability to collect and process soft, private information about their corporate borrowers, potentially enabling managers to conceal negative information and thus increasing stock price crash risk.

Our empirical evidence shows that lifting the restrictions on intrastate bank branching leads to a lower level of firm-specific stock price crash risk, consistent with the former view. This finding holds strongly in tests addressing potential endogeneity concerns and in a battery of additional

[17] In addition, the significant and positive (negative) stand-alone coefficients on current (future) profits support the argument that crash risk is triggered by inflated earnings, followed by weakening firm performance.

robustness checks. Taken together, the results support the prediction that after branching reforms, banks monitor borrowing firms more effectively, which, in turn, lowers their crash risk.

In our cross-sectional analyses, we provide further evidence on the bank monitoring mechanism by showing that the negative relation between bank branch reform and firms' stock price crash risk is more pronounced for firms more susceptible to bank monitoring post-deregulation, namely, those with greater reliance on external finance and lending relationships. Moreover, consistent with this economic mechanism, we find that the effect of bank deregulation on crash risk is stronger for firms with weaker corporate governance and greater financial constraints, which benefit more from effective bank monitoring. Finally, we document direct evidence that intrastate deregulation mitigates firms' crash risk by reducing managerial bad news withholding as well as firms' bad news formation.

Overall, our study helps link two separate strands of research, namely the established literature on banking, particularly bank deregulation, and recent corporate research on firm-level stock price crash risk. The former literature provides robust evidence that liberalization in the banking system, in the form of branching reform, is beneficial to economic growth (Levine, 2005). Our paper adds to this strand of research by documenting novel evidence that intrastate bank deregulation also reduces firms' stock price crash risk. This finding corroborates the view that an important driver of the growth effects of branching reforms is improvements in bank intermediation and monitoring (Jayaratne and Strahan, 1996). Finally, our study also provides a relevant policy implication. We show that deregulatory reform in the banking system may generate positive externalities for the corporate sector and investors as well as beneficial spillover effects across capital markets that contribute to shareholder wealth protection.

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Figure 1. The Impact of Bank 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 lead and lag indicators of bank 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 a state is deregulated in year t and zero otherwise. $D_{i,t-4}$ is set to one for the period up to four years prior to bank deregulation and zero otherwise. $D_{i,t-3}$ is set to one for the period up to three years prior to bank deregulation and zero otherwise. $D_{i,t+8}$ is set to one for the period eight years after bank deregulation and zero otherwise. We report the estimated coefficients as well as their 95% confidence intervals (in the dashed lines represent), adjusted for state-level clustering.

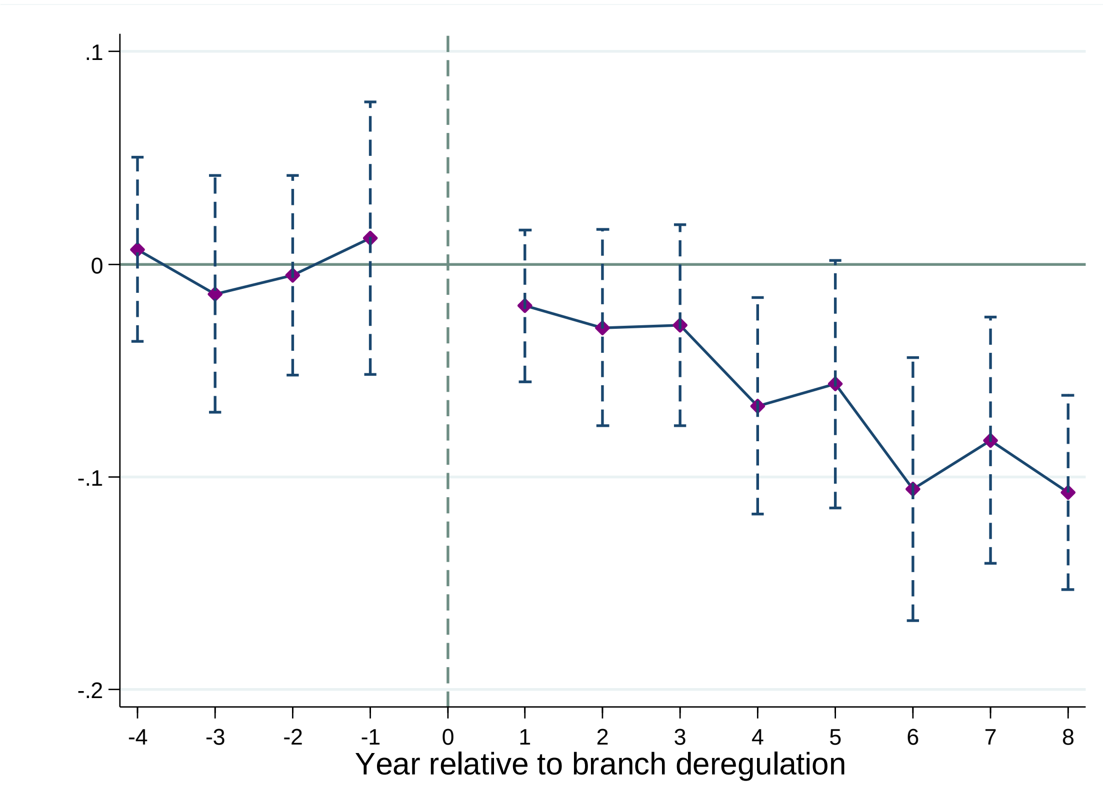

Table 1. State-level Branching Deregulation – Timeline

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
Main independent variable						
$BRANCH_t$	79,231	0.679	0.467	0.000	1.000	1.000
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 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.

Panel A. Baseline regression results

	(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

Panel B. Additional combinations of fixed effects				
	(1)	(2)	(3)	(4)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.025***	-0.014***	-0.027**	-0.014**
	(-2.71)	(-3.07)	(-2.25)	(-2.36)
$DTURN_t$	0.011***	0.006***	0.009***	0.004***
	(3.64)	(3.87)	(3.92)	(3.84)
$SIGMA_t$	0.444**	-0.020	-0.412*	-0.299***
	(2.06)	(-0.20)	(-1.97)	(-3.09)
RET_t	0.028**	0.004	-0.010	-0.009
	(2.01)	(0.63)	(-0.79)	(-1.57)
$SIZE_t$	0.073***	0.034***	0.174***	0.087***
	(35.59)	(35.22)	(29.13)	(30.90)
MB_t	0.008***	0.004***	0.006***	0.003***
	(6.71)	(6.92)	(4.27)	(4.73)
LEV_t	-0.026	-0.021***	0.076**	0.029**
	(-1.61)	(-2.64)	(2.65)	(2.11)
ROA_t	0.301***	0.158***	0.189***	0.093***
	(12.68)	(13.97)	(7.79)	(7.63)
$NCSKEW_t$	0.036***	0.018***	-0.077***	-0.034***
	(8.18)	(8.83)	(-15.23)	(-13.52)
$ACCM_t$	0.109***	0.050***	0.004	0.002
	(3.01)	(2.87)	(0.11)	(0.09)
Constant	-0.480***	-0.257***	-0.679***	-0.358***
	(-3.51)	(-3.50)	(-5.79)	(-5.54)
Firm FE	No	No	Yes	Yes
Industry FE	Yes	Yes	No	No
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	No	No
No. of obs.	79,231	79,231	79,231	79,231
Adj. R^2	0.075	0.083	0.053	0.060

Table 4. Endogeneity Tests: Pre-treatment Trends Analysis

This table presents the estimation results of the pre-treatment trends analysis using a dynamic specification. We replace the bank deregulation indicator (*BRANCH*) with a set of time indicators. 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 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. Propensity Score Matching Analysis

This table reports the results using the propensity-score-matched sample. We match control and treatment firms on all control variables used in the baseline regression model, as well as industry, state, and year, using a caliper width of 0.5% (without replacement). Panel A reports diagnostic statistics for the difference in firm characteristics between the treatment and control groups. Panel B reports the average treatment effects. Panel C reports the regression results based on the matched sample. The dependent variable, crash risk, is proxied by negative conditional skewness (*NCSKEW*) and down-to-up volatility (*DUVOL*) in year *t*+1. See Appendix A for other 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.

Panel A. Diagnostics statistics – differences 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

	Pre-deregulation	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)	(3)	(4)	(5)	(6)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.034*	-0.021**	-0.050***	-0.027***	-0.034*	-0.021**
	(-1.85)	(-2.23)	(-3.86)	(-4.09)	(-1.96)	(-2.31)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	No	No	Yes	Yes
Industry FE	No	No	Yes	Yes	Yes	Yes
No. of obs.	13,066	13,066	13,066	13,066	13,066	13,066
Adj. R^2	0.081	0.086	0.082	0.088	0.085	0.090

Table 6. The Role of External Financial Dependence

This table presents the results regarding the impact of bank branch deregulation on future stock price crash risk conditional on the degree of external financial dependence. External financial dependence is proxied by three measures, namely the external finance dependence ratio, net change in debt capital, and bank loan. In Columns (1) and (2), we follow Rajan and Zingales (1998) and compute the external finance dependence ratio as investment plus R&D expenses and acquisitions minus operating income before depreciation, divided by investment. Then, we set a dummy variable (*EXDEP*) equal to one for firms with above-median external finance dependence and zero otherwise. In Columns (3) and (4), we follow Frank and Goyal (2003) and define the net change in debt capital as long-term debt issuance minus long-term debt reduction, scaled by total assets. Then, we set a dummy variable (*NCD*) equal to one for firms with above-median net change in debt capital and zero otherwise. In Columns (5) and (6), we set a dummy variable (*BANKLOAN*) equal to one for firms with an above-median loan ratio and zero otherwise. The 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)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.012	-0.006	-0.024**	-0.010**	-0.004	-0.010
	(-1.13)	(-1.21)	(-2.36)	(-2.03)	(-0.14)	(-0.82)
$EXDEP_t$	0.036***	0.019***				
	(3.52)	(3.90)				
$BRANCH_t \times EXDEP_t$	-0.029***	-0.016***				
	(-2.71)	(-3.15)				
NCD_t			0.097***	0.047***		
			(11.24)	(11.77)		
$BRANCH_t \times NCD_t$			-0.017*	-0.013***		
			(-1.73)	(-2.73)		
$BANKLOAN_t$					0.061***	0.025***
					(4.81)	(3.90)
$BRANCH_t \times BANKLOAN_t$					-0.040***	-0.017**
					(-3.11)	(-2.64)
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.	67,353	67,353	64,973	64,973	24,882	24,882
Adj. R^2	0.067	0.074	0.048	0.054	0.046	0.049

Table 7. 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 for 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 the 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*) as one for industries with below-median distance and zero otherwise. In Columns (3) and (4), we set a dummy variable (*GROWDIS*) as one for industries with below-median increase in distance and zero otherwise. In Columns (5) and (6), we set a dummy variable (*AVLENGTH*) as 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

Table 8. The Role of Corporate Governance

This table presents the results regarding the impact of bank branch deregulation on future stock price crash risk conditional on the role of corporate governance. We employ two corporate governance proxies, namely institutional ownership and the corporate governance G-index (Gompers et al., 2003). In Columns (1) and (2), we set a dummy variable (*INSOWN*) as equal to one for firms with below-median institutional ownership in each industry-year and zero otherwise. In Columns (3) and (4), we set a dummy variable (*GINDEX*) equal to one for firms with above-median G-index in each industry-year and zero otherwise. 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)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.008	-0.002	-0.020	-0.014
	(-0.21)	(-0.11)	(-0.53)	(-0.79)
$INSOWN_t$	0.003	0.010		
	(0.06)	(0.40)		
$BRANCH_t \times INSOWN_t$	-0.109*	-0.055**		
	(-1.86)	(-2.14)		
$GINDEX_t$			0.065*	0.025
			(1.89)	(1.64)
$BRANCH_t \times GINDEX_t$			-0.080**	-0.033**
			(-2.71)	(-2.53)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
No. of obs.	10,296	10,296	14,076	14,076
Adj. R^2	0.053	0.057	0.051	0.056

Table 9. The Role of Financial Constraints

This table presents the results regarding the impact of bank branch deregulation on future stock price crash risk conditional on financial constraints. We employ three measures of financial constraints, namely (1) the WW index as in Whited and Wu (2006), (2) the KZ index as in Kaplan and Zingales (1997), and (3) an unrated dummy variable. In Columns (1) and (2), we set a dummy variable (*WW_INDEX*) equal to one for firms with an above-median WW index in each year and zero otherwise. In Columns (3) and (4), we set a dummy variable (*KZ_INDEX*) equal to one for firms with an above-median KZ index in each year and zero otherwise. In Columns (5) and (6), we set a dummy variable (*UNRATED*) equal to one for firms without long-term bond ratings and zero otherwise. 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)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.004	-0.002	-0.014	-0.010*	0.028	0.008
	(-0.35)	(-0.46)	(-1.17)	(-1.76)	(0.71)	(0.35)
WW_INDEX_t	0.010	0.007				
	(0.83)	(1.11)				
$BRANCH_t \times WW\_INDEX_t$	-0.045***	-0.024***				
	(-4.51)	(-4.71)				
KZ_INDEX_t			0.026**	0.012**		
			(2.60)	(2.30)		
$BRANCH_t \times KZ\_INDEX_t$			-0.029*	-0.012		
			(-1.78)	(-1.53)		
$UNRATED_t$					0.133***	0.053***
					(4.39)	(3.54)
$BRANCH_t \times UNRATED_t$					-0.068**	-0.026*
					(-2.29)	(-1.79)
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	42,730	42,730
Adj. R^2	0.073	0.081	0.073	0.080	0.057	0.061

Table 10. Bank Deregulation and Bad News Withholding

This table presents the mediation analysis of the role of measures of bad news hoarding, namely conditional conservatism and financial restatement. We compute firm-level conditional conservatism (*CSCORE*) following Khan and Watts (2009) and Kim and Zhang (2016); see Appendix A for details. Financial restatement (*REST*) is an indicator variable that takes one if a firm restates its financial statements and zero otherwise. The controls in Columns (1) and (4) include *SIZE*, *MB*, *LEV*, and *ROA*. The controls in the remaining columns include all the covariates in our baseline regression (Table 3). All models include state and year fixed effects. 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)
	$CSCORE_t$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$REST_t$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	0.052***	-0.027***	-0.015***	-0.010**	-0.066**	-0.039***
	(15.74)	(-3.09)	(-3.41)	(-2.36)	(-2.38)	(-2.76)
$CSCORE_t$		-0.298***	-0.135***			
		(-5.98)	(-5.96)			
$REST_t$					0.085***	0.036**
					(3.02)	(2.46)
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.	73,718	73,718	73,718	37,508	37,508	37,508
Adj. R^2	0.365	0.066	0.073	0.031	0.052	0.057

Table 11. Bank Deregulation and Bad News Formation

This table presents the mediation analysis of the role of key firm fundamentals and performance measures, including profitability, earnings surprise, and investment efficiency. Profitability is measured using *ROA*. Earnings surprise (*SURPRISE*) is defined as the difference between actual earnings and the last individual analyst forecast, divided by year-end stock price. Investment inefficiency (*INEFF_INV*) is defined as the difference between a firm's current investment and the industry-level average of investment. Investment is the sum of capital expenditure, research and development expenditures, less cash receipts from the sale of property, plant, and equipment, all scaled by lagged total assets. To economize on space, all other control variables are suppressed. The controls in Columns (1), (4), and (7) include *SIZE*, *MB*, *LEV*, and *ROA*. The controls in the remaining columns include all the covariates in our baseline regression (Table 3). All models include state and year fixed effects. 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.

Panel A. Mediation analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ΔROA_{t+1}	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$SURPRISE_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$INEFF\_INV_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	0.006*	-0.026***	-0.014***	0.005**	-0.040**	-0.018*	-0.024***	-0.025***	-0.014***
	(1.84)	(-2.76)	(-2.98)	(2.36)	(-2.13)	(-1.72)	(-9.10)	(-2.72)	(-3.00)
ΔROA_{t+1}		-0.269***	-0.141***						
		(-3.20)	(-3.13)						
$SURPRISE_{t+1}$					-0.234	-0.136**			
					(-1.60)	(-2.09)			
$INEFF\_INV_{t+1}$								0.131***	0.058***
								(5.37)	(4.88)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	69,920	69,920	69,920	22,832	22,832	22,832	67,834	67,834	67,834
Adj. R^2	0.165	0.078	0.084	0.040	0.043	0.048	0.072	0.077	0.083

Panel B. The role of past and current change in earnings				
	(1)	(2)	(3)	(4)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.029***	-0.016***	-0.025**	-0.013***
	(-2.77)	(-3.00)	(-2.61)	(-2.82)
ΔROA_t	0.191**	0.078*		
	(2.25)	(1.84)		
$BRANCH_t \times \Delta ROA_t$	-0.260**	-0.113**		
	(-2.53)	(-2.17)		
ΔROA_{t+1}			-0.688***	-0.356***
			(-5.83)	(-5.99)
$BRANCH_t \times \Delta ROA_{t+1}$			0.476***	0.244***
			(3.59)	(3.64)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
No. of obs.	69,906	69,906	69,906	69,906
Adj. R^2	0.080	0.086	0.078	0.085

Appendix A. Variable definitions

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.

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×prcc_f) divided by the book value of equity (market-to-book).

SIZE is the natural logarithm of market capitalization (csho×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.

EXDEP is a dummy variable set to one for firms with above-median external finance dependence and zero otherwise. As reported in Rajan and Zingales (1998), the 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.

NCD is a dummy variable set to one for firms with above-median net change in debt capital and zero otherwise. Net change in debt capital is defined as long-term debt issuance (dltis) minus long-term debt reduction (dltr), 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 (2017), 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.

INSOWN is a dummy variable that equals one for firms with below-median institutional ownership in each industry-year and zero otherwise.

GINDEX is a dummy variable that equals one for firms with an above-median G-index in each industry-year and zero otherwise. G-index is developed by Gompers et al. (2003).

WW is computed as −0.091×*CF* − 0.062×*DIVPOS* + 0.021×*TLTD* − 0.044×*SIZE* + 0.102×*ISG* − 0.035×*SG*, where *CF* is the ratio of cash flow to total assets; *DIVPOS* is a dummy variable that takes the value one if the firm pays cash dividends, and zero otherwise; *TLTD* is the ratio of long-term debt to total assets; *SIZE* is the natural log of total assets; *ISG* is the three-digit industry sales growth; and *SG* is firm sales growth.

KZ is computed as −1.002×(*Cashflow*/*K*) + 0.283×(*Q*) + 3.139×(*Debt*/*Capital*) − 39.368×(*Dividends*/*K*) − 1.315×(*Cash*/*K*), where *K* is beginning-of-year property, plant, and equipment; *Cashflow* is operating income plus depreciation; *Q* is book assets minus book common equity minus deferred taxes plus market equity, dividends by book assets; *Debt* is short-term plus long-term debt; *Dividends* are total annual dividend payments; *Cash* is cash plus marketable securities; and *Capital* is debt plus total stockholders' equity.

UNRATED is a dummy variable equal to one for firms without long-term bond ratings and zero otherwise.

CSORE is conditional conservatism, calculated using the approach developed by Khan and Watts (2009) and used in Kim and Zhang (2016).

REST is an indicator variable equal to one if the firm restates its financial statements, and zero otherwise.

SURPRISE is earnings surprise, defined as the difference between actual earnings and the last individual analyst forecast, divided by year-end stock price.

INEFF_INV is investment inefficiency, defined as the difference between a firm's current investment and the industry-level average of investment. Investment is the sum of capital expenditure (capex), research and development expenditures (xrd), less cash receipts from the sale of property, plant, and equipment (sppe), all scaled by lagged total assets.

Other variables used in internet appendix

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.

ANTITAKEOVER1 represents first-generation state antitakeover laws, adopted by 38 U.S. states over the period 1968–1981 (Karpoff and Wittry, 2018).

ANTITAKEOVER2 represents second-generation state antitakeover laws, developed by Bebchuk and Cohen (2003). These laws contain five most common types of laws adopted by various states since 1982, namely, control share acquisition laws (CS), business combination laws (BC), fair price laws (FP), directors' duties laws (DD), and poison pill laws (PP). *ANTITAKEOVER2* is a count variable that varies from zero to five and increases by one for coverage by BC, CS, FP, PP, and DD laws (Karpoff and Wittry, 2018).

IDD is a dummy variable that reflects the state-level adoption of the Inevitable Disclosure Doctrine.

GOOD_FAITH, *IMPLIED_CONTRACT*, and *PUBLIC_POLICY* are indicator variables equal to one if the state where a firm is headquartered has adopted the good faith exception, implied contract, and public policy exceptions, respectively.