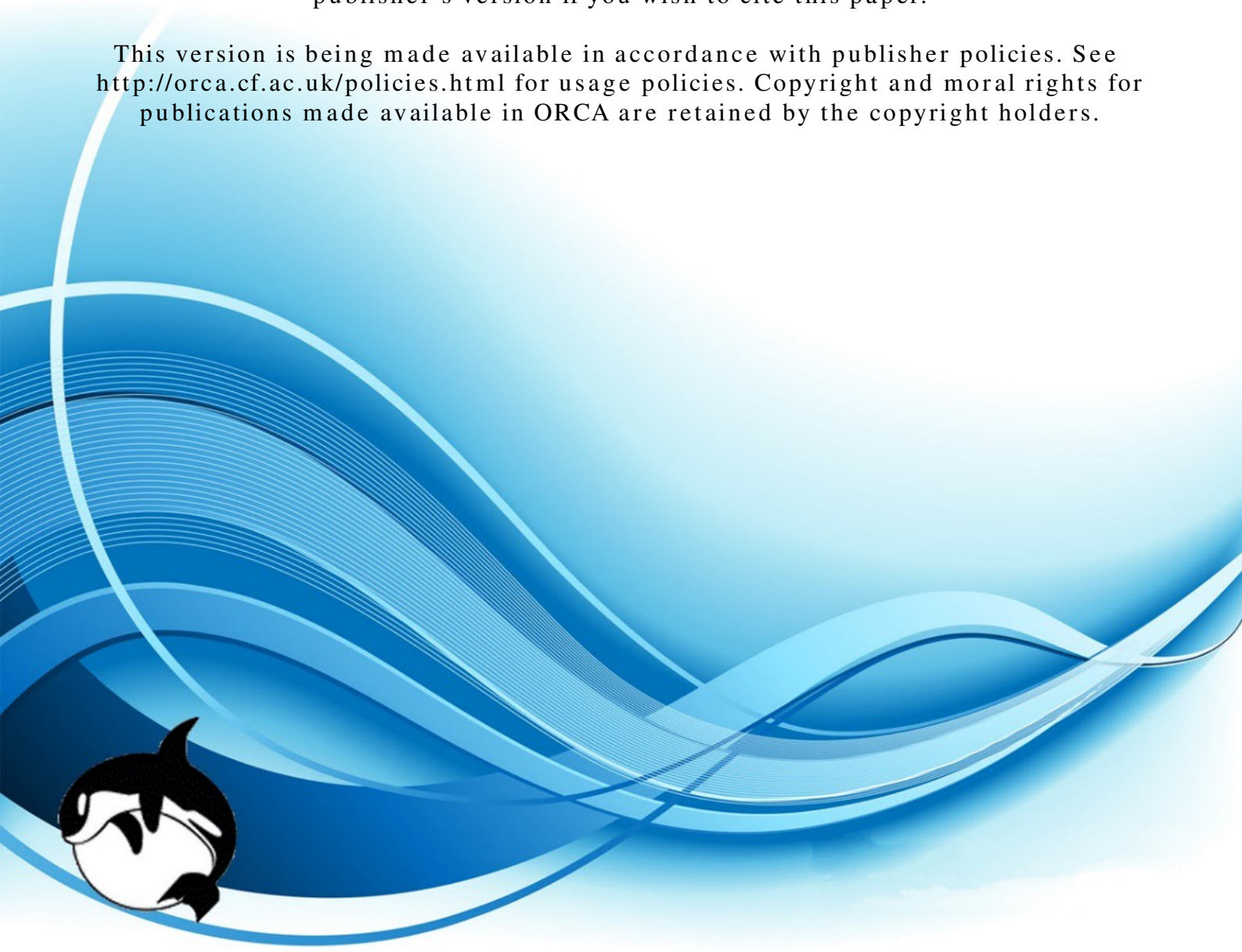

Does Bank Stakeholder Orientation Enhance Financial Stability?

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Woon Sau Leung[†]
Cardiff Business School, Cardiff University
Aberconway Building, Colum Drive, Cardiff, UK, CF10 3EU
LeungWS1@cardiff.ac.uk

Wei Song
School of Management, Swansea University
Bay Campus, Fabian Way, Swansea, UK, SA1 8EN
w.song@swansea.ac.uk

Jie Chen
Leeds University Business School, University of Leeds
Maurice Keyworth Building, Leeds, UK, LS2 9JT
J.Chen3@leeds.ac.uk

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[†] Corresponding author. Cardiff Business School, Cardiff University, Aberconway Building, Colum Drive, Cardiff, CF10 3EU; Email address: LeungWS1@cardiff.ac.uk. Tel: +44 (0)29 2087 6736.

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ABSTRACT

Using the staggered enactment of constituency statutes across US states, we find that banks with directors whose legal duties are expanded to consider stakeholder and long-term interests significantly reduce risk-taking by increasing capital and shifting to safer borrowers. Additionally, we find that the effect of statute enactment on bank performance is insignificant on average but significantly positive for banks that take excessive risk. Furthermore, we find that banks that previously received a statute enactment fared significantly better during the crises. Our findings support the increasing calls for greater emphasis on stakeholder interests amidst the current bank regulatory and governance reforms.

Keywords: Bank risk-taking; Stakeholder orientation; Constituency statutes; Fiduciary duties; Financial stability.

JEL Classification: G01; G21; G28; G32; M14.

1. Introduction

The 2007-09 financial crisis has cast doubt on the adequacy of the prevailing bank regulatory and governance frameworks, which focus primarily on shareholder value maximization (Senior Supervisors Group, 2009). In particular, banks with more shareholder-friendly boards incurred significantly greater losses during the crisis (Beltratti and Stulz, 2012). As noted by Mr. Klaus Schwab, the founding chairman of the World Economic Forum, "[t]he current crisis should be a warning shot for us to fundamentally rethink … the regulatory mechanisms that underpin our economy … we need to embrace stakeholder principle …" (The Wall Street Journal, 2010). Similarly, Macey and O'Hara (2003, p.92) argue that " … [to safeguard the banking system, bank directors'] duties should not run exclusively to shareholders." Echoing these concerns, the current bank reform initiatives advocate that banks place greater emphasis on value creation for stakeholders to ensure the safety of the banking sector (Laeven, 2013). In this article, we exploit the plausibly exogenous variation in directors' fiduciary duties to examine whether stakeholder orientation affects bank risk taking, performance, and stability.

Shareholders and stakeholders have different risk preferences. Stakeholders such as creditors have less risk-taking incentives and tend to focus more on long-term stability than shareholders because they typically receive fixed income but bear high downside risk. By contrast, shareholders, due to limited liability and asymmetric payoffs, are prone to take greater risk than is desired by stakeholders (Jensen and Meckling, 1976). This conflict between shareholders and stakeholders (the asset-substitution problem) is especially severe among banks because they are highly leveraged (Jensen and Meckling, 1976) and opaque (Morgan, 2002), face limited disciplining from insured depositors (Merton, 1977), and have implicit government guarantees (Gandhi and Lustig, 2015). These unique features provide greater incentives for bank shareholders and managers to invest in excessively risky assets (Macey and O'Hara, 2003; Thakor, 2014; Acharya, Mehran, and Thakor, 2016). These arguments amount to the view that the prevailing bank governance model, which focuses on shareholder profits, may promote excessive risk taking, undermining systemic stability (Macey and O'Hara, 2003, 2016).

Directors' oversight of bank operations and risk taking is important for mitigating excessive bank risk taking and systemic risk. There has always been a strong demand from regulators, practitioners, and

the public for bank directors to consider long-term, stakeholder interests. For instance, Section 5000.1 of the Bank Holding Company (BHC) Supervision Manual (Federal Reserve, 2016, p.1) states that "[t]he board of directors is responsible to the bank's depositors, other creditors, and shareholders for safeguarding their interests … ".[1] However, the traditional fiduciary laws diverge from these expectations and require bank directors to account for shareholder interests only (Herlihy and Makow, 2014). Since the shareholder primacy view prevails in court and the business community, such legal pressure discourages bank directors from challenging bank policies that favor shareholders but could jeopardize stability and social interests (Macey and O'Hara, 2003). To mitigate this problem, Macey and O'Hara (2003, 2016) propose that the fiduciary duties of bank directors should be *broadened* to consider the long-term, stability implications of bank actions. On a similar note, Federal Reserve Governor Daniel Tarullo (2014) highlights that modifying the fiduciary duties of boards of financial firms will " … make the boards of financial firms responsive to the broader interests implicated by their risk-taking decisions … ". Based on these arguments, we hypothesize that an increase in bank directors' authority to consider stakeholder interests reduces bank risk taking and enhances financial stability.[2]

To test our hypothesis, we exploit a unique empirical setting of the staggered passage of constituency statutes across US states. Constituency statutes expand directors' fiduciary duties, which are traditionally owed to shareholders, to consider the interests of a variety of stakeholders, including creditors, suppliers, employees, the government, etc. The motivation behind the development of the statutes is to provide directors with a legal mechanism for pursuing stakeholder interests and long-term corporate objectives without violating their legal duties to shareholders (Hale, 2003). The statutes are enforceable and

[1] Similarly, the Office of the Comptroller of the Currency (OCC) (1997, p.2) also demands that "[d]irectors of a national bank are accountable not only to their shareholders and depositors but also to their regulators." Furthermore, in their special report on governance of financial institutions, the Group of Thirty Working Group (2012, pp.39-40) recommends that "[g]iven the unique externalities of FIs [(Financial Institutions)], society's interests and well-being must always be a high priority for the board … Regardless of who the primary stakeholder is, FI boards in every country ought to take a long-term view on strategy … ".

[2] There are at least two reasons as to why bank directors may choose to consider stakeholder interests and financial stability. First, since banks are important financial intermediaries and their failures could have widespread negative consequences to many market participants, bank directors may feel that it is morally right for them to pursue banking policies favoring stakeholders or financial stability. Second, since banking is a specialized, competitive, and highly regulated industry, bank directors face severe reputation concerns in the labor markets, and thus would be willing to consider the broader impact of risk-taking on stakeholders and the society.

have been applied by courts in defending directors' considerations of stakeholder interests. Based on 30 years of case evidence, Geczy, Jeffers, Musto, and Tucker (2015) conclude that the statutes are a "true expansion of directors' authority" to consider stakeholder interests.

Following Bertrand and Mullainathan (2003), we estimate a Difference-In-Differences (DID) model to examine the relation between stakeholder orientation and bank risk taking. The treatment group comprises states that pass the constituency statutes and the control group comprises states that do not in a given year. Based on 939 publicly traded US BHCs from 1986 to 2012, statute passage is shown to reduce bank total risk and idiosyncratic risk by 6.6% and 7.9%, respectively, and increases Z-score by 15.4%, relative to the sample means. This negative relation is robust to alternative fixed effects, standard errors, model specifications, estimation approaches and samples, additional controls for bank culture, and an alternative measure of bank stakeholder orientation.

To reduce the endogeneity concern relating to reverse causality, a hazard model that examines whether bank risk, aggregated to the state level, may influence the timing of the enactment of constituency statutes, is estimated. The results show that none of these bank-risk measures are significant in determining statute adoption, suggesting that this concern is unlikely to affect our results.

The key identifying assumption of our DID tests is that the treated and the control BHCs are on parallel trends prior to statute passage (Roberts and Whited, 2012). Further tests show that the statute effects up to two years prior to the treatment are small and insignificant, suggesting that the differences in pre-trend between the two groups are indeed indistinguishable. The decrease in bank risk occurs after statute passage, further ruling out reverse causality.

To ensure that our results are not driven by chance, three placebo tests are conducted. The first maintains the distribution of enactment years but randomly assigns states into these years. The second keeps the distribution of the treated states but randomizes their enactment years. The third randomizes both the enactment years and treated states. In each test, we create 5,000 placebo samples and obtain a distribution of placebo estimates. Our true DID estimate is smaller than at least 90 percent of the placebo estimates.

Unobserved changes in local economic conditions influencing the introduction of constituency statutes and bank risk-taking might have biased our estimates. To rule out this concern, we exploit the fact

that local economic conditions tend to be similar across adjacent states, whereas the statute effect stops at the borderline, and analyze a subsample of treated and control BHCs that are geographically close to each other and similar in size. This concern cannot explain our results.

Since some of the constituency statutes were passed at similar times as other state antitakeover laws and banking deregulation policies, our results might be confounded by such coincidental events. Reassuringly, our results are robust to excluding "contaminated" states where the statute's passage coincides with the major antitakeover laws or banking deregulation policy changes and to controlling jointly for these events in the baseline bank-risk models.

To offer more direct stability implications, we examine three additional risk measures that capture the default likelihood of banks. The first is the expected default frequency estimated using the KMV-Merton model following Bharath and Shumway (2008). The second is "Tail beta", a stock-market-based measure of bank systemic risk exposure defined as the probability of a sharp decline in a bank's stock price conditional on a crash in a broad US bank index (de Jonghe, 2010). The third is the number of banks that either failed or received financial assistance in a given state-year. The results show that statute passage significantly reduces the three additional risk measures, consistent with the statutes reinforcing bank stability.

Two channels through which banks could reduce risk are examined. First, to the extent that constituency statutes expand bank directors' authority to pursue more prudent policies favoring stakeholders and stability, banks could increase equity capital to reduce asset-substitution, moral hazard problems and better absorb losses (Jensen and Meckling, 1976; Macey and O'Hara, 2003; see Thakor, 2014, for a review). Consistent with this channel, banks are shown to increase the capital-to-assets ratio by 6.9% (relative to the sample mean) following statute passage. Second, we explore the bank-lending channel and find evidence that banks shift their lending from riskier to safer borrowers after statute passage.

An alternative explanation for our results is that constituency statutes might have been used primarily as a takeover defense. Facing reduced takeover threat, bank managers may prefer a "quiet life" and avoid the difficult decisions and costly effort associated with risk taking (Bertrand and Mullainathan, 2003). Our findings do not support this alternative view because the negative statute effect on risk taking

concentrates on those statutes that permit consideration of stakeholder interests in both ordinary and change-of-control situations. In addition, statute passage is shown to have little effect on BHCs' acquisition likelihood.

Having established a negative effect of stakeholder orientation on bank risk taking, we turn our attention to understanding its performance implications. The relation between stakeholder orientation and bank performance is ambiguous. While the reduced loan risk and losses after statute passage may increase profitability, the lower loan prices owing to the shift towards safer borrowers may hurt performance. A setting where the former effect likely dominates the latter is identified. Theory suggests that banks with moral hazard problems have strong incentives to invest in excessively risky and inefficient assets (i.e., high-risk-low-return assets) (Merton, 1973; Jensen and Meckling, 1976; Acharya, Mehran, and Thakor, 2016). Among these banks, their inefficient risk taking would make a shift from riskier to safer borrowers more desirable insofar as the gain from reducing loan losses exceeds the reduction in loan interest income. However, for banks with safer borrowers in the first place, such a shift would be less appealing as the marginal benefit from reducing loan losses is lower. Consistent with our prediction, the effect of statute passage on bank performance is small and insignificant on average, but significantly positive for high-risk banks.

Finally, we explore the longer-term value implications of stakeholder orientation by studying bank crisis performance. Tracing BHCs that have received a statute passage prior to the crises, our tests show that the previously-treated BHCs fared significantly better than the size-location-matched control BHCs in crises—with DID estimates ranging from 6.5 to 9.7 percentage points for the 1998 crisis and from 13.8 to 33.8 percentage points for the 2007-09 crisis. This evidence suggests that stakeholder orientation mitigates losses during crises and enhances stability.

Our paper makes several contributions to the literature. To start with, we present new evidence that supports the widespread calls for greater emphasis on stakeholder interests in the current bank governance reforms (Macey and O'Hara, 2003; Mehran, Morrison, and Shapiro, 2011; Laeven, 2013). Our findings suggest that bank stakeholder orientation reduces risk taking, mitigates losses during crises, and in turn improves stability. We also complement existing studies on the incompatibility between shareholder value

maximization and financial stability by showing that more shareholder-oriented banks have lower crisis performance, more severe moral hazard problems, and greater systemic risk.[3]

Second, we contribute to one of the debates in the current bank governance reforms on whether the fiduciary duties of bank directors should be expanded to consider the long-term, stability implications of bank decisions (Macey and O'Hara, 2003, 2016; Tarullo, 2014). While there is a great demand for bank directors to consider long-term, social interests, the traditional fiduciary laws stipulate that bank directors owe legal duties only to shareholders and have no obligations to mitigate systemic risk. Under this legal pressure to maximize shareholder profits, bank directors may tolerate risky bank policies, potentially threatening stability (Macey and O'Hara, 2003, 2016). Constituency statutes, which alleviate such legal pressure, offer a unique empirical setting for testing these arguments. Our evidence shows that banks with directors whose duties are expanded after statute passage reduce risk taking through adopting a less risky capital structure and lending to safer borrowers.

Third, our paper relates to the literature on the stakeholder theory. Allen, Carletti, and Marquez (2015) incorporate the costs of layoffs for workers during bankruptcies into the firm's objective function and show that stakeholder firms are more valuable when marginal cost uncertainty is greater than demand uncertainty. Magill, Quinzii, and Rochet (2015) argue that large firms face endogenous risk that generates negative externalities on stakeholders which are not internalized by shareholders. The resulting under-investment problem in the prevention of such risk can be alleviated if firms' objectives are to maximize the total welfare of their stakeholders rather than just shareholder value. Our paper complements these studies by showing empirically that stakeholder orientation improves bank stability and reduces negative externalities.

Fourth, our paper adds to the management literature on whether it is socially desirable for firms to pursue stakeholder interests (see, e.g., Freeman, 1984; Cornell and Shapiro, 1987; Freeman, Wicks, and Parmar, 2004). As Jensen (Agle et al., 2008) notes, " … [in] the debate about stakeholder theory versus

[3] See, for example, John and Qian (2003), Bebchuk, Cohen, and Spamann (2010), Fahlenbrach and Stulz (2011), Beltratti and Stulz (2012), Berger, Imbierowicz, and Rauch (2016), etc.

stockholder theory … there is way too much noise, way too much sloppy thinking and way too *little* empirical evidence present". We are one of the few to offer evidence to this debate.

Fifth, our paper extends the banking literature (see, e.g., Keeley, 1990; Laeven and Levine, 2009; Hasan, Massoud, Saunders, and Song, 2015; Ho, Huang, Lin, and Yen, 2016; Delis, Hasan, and Mylonidis, 2017) by showing that stakeholder orientation is an important determinant of bank risk taking and crisis performance. Finally, our paper relates to studies examining how constituency statutes affect corporate policies. Flammer and Kacperczyk (2016) show that statute enactment increases non-financial firms' stakeholder-friendly policies and corporate innovation. Radhakrishnan, Wang, and Wang (2018) find that statute passages significantly reduces accounting conservatism, consistent with stakeholders becoming less concerned about shareholder expropriation. We differ from these studies in that we derive policy implications relevant to the current bank governance reforms and systemic stability.

Our paper is organized as follows: Section 2 introduces the constituency statutes. Section 3 explains our sample and variables. Section 4 explains our empirical strategy and reports our results, and Section 5 concludes.

2. Background on Constituency Statutes

The origin of constituency statutes traces back to the 1930s debate on the fundamental nature of corporation and specifically on whether management had a duty to shareholders or to a broader group of stakeholders (Orts, 1992). Under the traditional US laws, corporate leaders were legally required to maximize shareholder value and were not allowed to consider stakeholder interests or other nonmonetary corporate objectives. Since this shareholder primacy view prevailed in court and the business society, fearing opposition and litigation from shareholders, corporate leaders were reluctant to divert their attention from shareholder value maximization (Smith, 1998; Fisch, 2005). Nonetheless, a number of corporate policies, e.g., restructuring, liquidations, and acquisitions, that favor shareholder interests may often adversely affect stakeholders (including managers) and impair their firm-specific investments which are not fully protected through explicit contracts (Gavis, 1990). Because corporate actions affect both shareholders and

stakeholders, proponents of stakeholder theories argue that corporations should consider the interests of the latter in their strategies and actions.

This debate regained its popularity in the 1980s amidst the hostile takeover waves in the US. While takeovers could benefit target firm shareholders, the associated changes in ownership often imposed substantial costs onto its stakeholders, including creditors, employees, customers, etc. There was widespread criticism from the public about the inability of corporate laws in protecting stakeholder interests. The increasing debate on whether firms should be allowed to consider stakeholder interests eventually led to the development of the constituency statutes. Although the introduction of the constituency statutes was initially triggered by the merger waves in the 1980s (Karpoff and Wittry, 2018), their applications were not limited to takeover situations; in fact, over half of the statutes can be applied in general business decisions (Bainbridge, 1992; Oswald, 1998). As of 2012, 35 US states have passed the constituency statutes (Barzuza, 2009). Table 1 shows a timeline of the passage of constituency statutes.

Insert Table 1 about here

Although the constituency statutes differ across states, they share a common theme—to permit directors or officers to consider the effects on various specified constituencies when making business decisions, without breaching their fiduciary duties to shareholders (Geczy, Jeffers, Musto, and Tucker, 2015). For instance, the 1987 Minnesota statute (*MINN. STAT. ANN. § 302A.251*) stipulates that when considering the best interest of the company, a director may consider the interests of (1) its employees, customers, suppliers, and creditors, (2) the state and national economy, and (3) the community and the rest of the society, as well as may consider (4) the long- and short-term interests of the corporation. The relevant clauses in state legislatures of the 35 statutes are provided in Table IA.2 of the online appendix.

The constituency statutes are legally enforceable and have been applied by courts to defend stakeholder interests. According to Geczy, Jeffers, Musto, and Tucker (2015), over the period from 1983 to 2013, there are in total 47 federal and state court cases that discussed the constituency statutes. These cases range from mergers, liquidations, restructuring, and other shareholder derivative suits. The authors have analyzed the courts' enforcement opinions and coded 20 cases as having a "Positive" opinion, 9 cases

as "Positive/Neutral", 14 cases as "Neutral", and as few as 4 cases as "Neutral/Negative".[4] Based on the 30 years of court opinions that are mostly positive and above-neutral, the authors conclude that the constituency statutes are a "true expansion of directors' authority" to consider stakeholder interests.

Several empirical studies have established that the enactment of constituency statutes improves corporate stakeholder performance. For instance, Luoma and Goodstein (1999) find that constituency statutes are associated with a greater board stakeholder representation. Flammer and Kacperczyk (2016) find that firms incorporated in states with statute passage adopt more stakeholder-friendly policies (measured by the firm-level KLD index) than states without passage. In our setting, because the coverage by KLD for BHCs is very limited, we are unable to directly test for the effect of constituency statutes on bank stakeholder performance. However, although not ideal, we offer some indirect evidence (unreported) that statute passage has likely improved BHCs' stakeholder performance. First, we find that passing the statutes significantly lowers the likelihood of a large reduction in BHCs' employees, consistent with greater job stability.[5] Second, consistent with banks engaging with the rest of the society through greater tax transparency[6], we find that statute passage significantly increases bank effective tax rates.[7]

Finally, because the constituency statutes do not likely reflect the bank's strategic decisions, the associated increase in stakeholder orientation is plausibly exogenous to its risk-taking. Given the staggered

[4] A case is coded as "Positive" when the constituency statute "was a determinative element in the court's ruling and the court … recognized directors' expanded ability to consider non-shareholder constituents, but did not create a private enforcement right in such non-shareholder constituents." A case is coded as "Positive/Neutral" for court opinions "where the court discussed the scope of constituency statutes both in terms of expanding director rights and declining to extend constituency statutes to create a positive right in non-shareholder constituents … ". A case is coded as "Neutral" for court opinions "that cited, referenced by name, or included dicta regarding constituency statutes … ". The "Neutral/Negative" category "reflects cases where the court discussed constituency statutes, but declined to recognize expanded director authority … " (see Geczy, Jeffers, Musto, and Tucker, 2015, pp. 106-110).

[5] Specifically, we find that the constituency statutes significantly lower the likelihood of having an economically significant reduction in the number of employees (significant at the 5% level). We define an economically significant reduction in the number of employees as any negative percentage changes that are less than its 10th percentile in our sample BHCs. These results can be found in Table IA.3 of the online appendix.

[6] For instance, in the 2014 global citizenship report, Citi Group (2014) wrote, " … our culture of responsible finance extends to our approach to paying taxes … We emphasize both strong internal controls and transparency with global taxing authorities … "; in the 2013 sustainability report, HSBC Holding plc (2013) stated that, "By running a sustainable banking business, HSBC is able to make a valuable contribution to the global economy by … [paying] tax revenues to governments in the countries and territories where we operate … HSBC does not enter into or promote tax avoidance."

[7] The effective tax rate is computed as the ratio of tax expense to pre-tax earnings. This measure is commonly used in the tax literature as a proxy for tax avoidance [see for a discussion of this measure in Hanlon and Heitzman (2010)]. These results can be found in Table IA.3 in the online appendix.

nature of statute passage, BHCs are exogenously shocked at different times, thereby allowing us to setup a DID model with multiple treatment groups and time periods for identification. This setting helps reduce the potential biases and noise associated with analysis with only a single exogenous shock (Roberts and Whited, 2012).

3. Sample Formation and Variable Construction

We start with all publicly traded US BHCs that filed FR Y-9C reports with the Federal Reserve during the period from 1986 to 2012. We download the BHCs' consolidated financial information from the Bank Regulatory database and their stock information from CRSP.[8] We supplement these data with additional firm attributes, such as states of location and incorporation, from Compustat. Our final sample consists of 9,248 bank-year observations from 939 BHCs. In total, 166 of these BHCs received a statute enactment during our sample period (see Table 1).[9]

Following Goetz, Laeven, and Levine (2016), we construct three measures of bank risk. First, bank total risk is defined as the annualized standard deviation of daily stock returns for each year. Second, bank idiosyncratic risk (*IVol)* is defined as the annualized standard deviation of the residuals from the following model (this specification follows Chen, Steiner, and Whyte (2006) and Pathan (2009)):

$$R_{it} = \alpha_i + \beta_{1i} R_{mkt,t} + \beta_{2i} INTEREST_t + \varepsilon_{it}. \quad (1)$$

where R_{it} is daily stock returns of BHC i, $R_{mkt,t}$ is excess market returns of the value-weighted CRSP market index, and $INTEREST_t$ is the changes in the three-month Treasury bill rates. Third, a BHC's Z-score is computed as follows:

$$Z\text{-}score_{it} = (ROA_{it} + Capital/TA_{it})/\sigma_{it} \quad (2)$$

[8] The stock information is merged with the bank fundamentals, using the linking table (called "CRSP-FRB link") provided by the New York Federal Reserve Bank. The linking table can be accessed at: [http://www.newyorkfed.org/research/banking_research/datasets.html] As mentioned in their documentation, the beginning and end dates for each BHC in the linking table are manually verified by researchers. Our BHC sample is unbalanced and covers all BHCs that exist in a given year. When a BHC is merged, it leaves our sample in the next year.

[9] In untabulated tests, we exclude all bank-year observations in which banks are involved in mergers and acquisitions (M&As) activities and confirm that our results are unaffected. Alternatively, we construct a M&A dummy that equals one if a BHC is involved in any bank merger deals in a given year and zero otherwise and find that our results hold after controlling for it. These results can be found in Table IA.23 of the online appendix.

where ROA_{it} is the return on assets, $Capital/TA_{it}$ is the capital to total asset ratio, and σ_{it} is the annualized standard deviation of daily stock returns over a year. *Z-score* can be interpreted as the number of standard deviations by which profit can decrease before a bank goes into bankruptcy, where a larger *Z-score* indicates greater bank stability and lower risk.

We control for several bank characteristics that may influence risk-taking. First, bank size is measured as the natural logarithm of the book value of deflated total assets. Bank profitability is measured by return on assets (*ROA*), which is calculated as earnings before taxes and extraordinary items divided by total assets. Income from nontraditional banking activities is measured by the ratio of non-interest income to assets (*Non-interest income/TA*). Liquidity risk is captured by the ratio of non-core deposits to assets (*Non-core deposits/TA*) and the ratio of total loans to assets (*Loans/TA*). Bank franchise value is captured by the market-to-book equity ratio (*Market-to-book equity ratio*) (Keeley, 1990).[10] We control for the frequency of trading in BHC stocks (*FREQ),* defined as the average daily volume of shares traded divided by the number of shares outstanding. To account for the effect of macroeconomic conditions on bank risk-taking, we include state real GDP growth and the natural logarithm of state population in the model, which are both downloaded from the US Bureau of Economic Analysis (BEA). To reduce the effects of outliers, all bank variables are winsorized at the 1st and 99th percentiles.

Table 2 reports the summary statistics. We find that BHC size, as measured by deflated total assets, is highly skewed and ranges from a minimum of $111 million to a maximum of $2.07 trillion[11] with a mean (median) of $17.7 ($1.4) billion. For bank risk, the means (medians) of *Total risk, IVol*, and *Z-score* are 40.9% (34.3%), 38.2% (31.9%), and 0.298 (0.273), respectively. For bank profitability, the mean (median) *ROA* and *Non-interest income/TA* are 1.1% (1.3%) and 1.2% (1%), respectively. As for the other bank variables, the average non-core deposits to total assets, loan-to-assets ratio, market-to-book equity ratio, and frequency of trading are 12.6%, 64.9%, 1.48, and 0.2%, respectively.

Insert Table 2 about here

[10] Our results are similar when we measure franchise value by Keeley's *q* (Keeley, 1990), computed as the sum of the market value of equity plus the book value of liabilities divided by the book value of total assets.
[11] In our sample, the maximum total deflated assets of 2.07 trillion refers to Citigroup Inc. in 2007 while the minimum total deflated assets of 111 million refers to American Bancorporation in 1987.

4. Empirical Results

4.1. Constituency Statutes and Bank Risk Taking

To test whether stakeholder orientation affects bank risk, we follow Bertrand and Mullainathan (2003) and formulate a DID test design in a multiple treatment groups and time periods setting based on the staggered passage of constituency statutes across US states. The baseline model is written as follows:

$$Risk_{islrt} = \alpha_t + \alpha_i + \alpha_r \times \alpha_t + \lambda \times Constituency\ Statute_{st} + \delta X_{islrt} + \gamma W_{lrt} + \varepsilon_{islrt}, \quad (3)$$

where i is a BHC, s is the state of incorporation, l is the state of location, r is the economic region, and t is year. $Risk_{islrt}$ is *Total risk*, *IVol,* or *Z-score*. $Constituency\ Statute_{st}$ is a treatment dummy variable, which equals one if a BHC is incorporated in state s that has passed a constituency statute by year t, and zero otherwise. X_{islrt} and W_{lrt} are vectors of bank and state control variables, respectively (see Section 3). Bank (α_i) and year fixed effects (α_t) are included to control for unobservable time-invariant bank characteristics and economy-wide shocks on bank risk. We account for the effects of time-varying regional shocks on bank risk-taking by interacting a region indicator with the year dummies ($\alpha_r \times \alpha_t$) in the model. The BHCs are grouped into four regions—Northeast, South, Midwest, or West—based on their states of location following the US Census Bureau. Standard errors are clustered at the state of incorporation to account for serial correlation following the recommendations by Bertrand, Duflo, and Mullainathan (2004).

The average treatment effect of stakeholder orientation on bank risk is captured by the estimated coefficient on *Constituency Statute*, λ. Including the bank, year, and region-year fixed effects allows λ to be estimated as the *within-state* differences before and after the passage of constituency statute as opposed to the before-after differences in states where there are no changes in statute in the same period (Imbens and Wooldridge, 2009).

To illustrate, assume that we are interested in estimating the effect of constituency statute in Indiana in 1989 on bank risk, we can subtract the bank risk before 1989 from the bank risk after 1989 for BHCs incorporated in Indiana. However, it is difficult to identify the statute effect because bank risk may also be influenced by other events or economy-wide shocks that occurred in about 1989. To control for these confounding effects, a control state where there is no change in constituency statute in the same period can be used. We calculate the difference between the difference in bank risk in Indiana before and after 1989

with the same before-after difference in bank risk in the control state. This difference in the two differences is an estimate of the statute effect in Indiana. The regression framework extends this example to account for the fact that there are many constituency statutes staggered over time.

Table 3 reports the estimation of equation (3). In columns (1), (4), and (7) where bank controls, bank, and year fixed effects are included, the estimated coefficients of *Constituency Statute* are negative and significant for *Total risk,* and *IVol*, and is significantly positive for *Z-score*. The coefficient estimates for *Constituency Statute* remain similar in magnitude and statistical significance after we control for the state GDP growth and log population (see columns (2), (5), and (8)) and further controlling for region-year fixed effects (see columns (3), (6), and (9)). In addition, the statutes' negative effects on bank risk are economically significant. The enactment of constituency statutes decreases bank total risk by 6.6% and idiosyncratic risk by 7.9%, and increases the Z-score by 15.4%, relative to their sample means. This evidence is consistent with our hypothesis that stakeholder orientation reduces bank risk-taking.

Insert Table 3 about here

4.2. Robustness Tests

Table 4 presents the robustness tests. We use the same bank and state control variables, and fixed effects as described in equation (3), unless stated otherwise. For brevity, we report only the estimated coefficients and standard errors for *Constituency Statute* and present them in rows rather than columns.

First, we cluster standard errors at the BHC level and obtain similar results (see row (1)). Second, to account for serial correlations in the error terms, we use the two-step procedure following Bertrand, Duflo, and Mullainathan (2004). In the first step, the bank-risk measures are regressed on the bank and state controls, and fixed effects as described in equation (3) without the treatment variable. We extract the regression residuals for the treated BHCs and then average them for the pre- and post-treatment periods to arrive at a two-period panel. In the second step, these averaged residuals are regressed on *Constituency Statute* with White robust standard errors. Results remain similar qualitatively (see row (2)).

Row (3) includes the state-of-incorporation fixed effects to account for persistent differences across states, such as differences in the banking industry structure or in the relative market power of large versus small banks (Rice and Strahan, 2010), that may influence bank risk taking. Our results hold.

As some states have more observations and treated BHCs than others, our results may be driven by the over-representation of such states. To address this, we give equal weights to each state by estimating weighted least squares (WLS) regressions in which weights are calculated as the inverse of the number of observations in each incorporation state. Our results are even stronger (see row (4)).

We exclude 16 states (down to 5,696 observations) in which no constituency statutes were enacted to avoid potential concerns about selection—BHCs incorporated in states with statute passage may be different from those incorporated in states without statute passage—and estimate the regressions. Our results hold (see row (5)).

A potential concern is that Delaware-incorporated BHCs may be different from BHCs incorporated elsewhere. A notable difference is that under the Delaware law, firms may consider stakeholder interests to the extent that such considerations maximize shareholder interests (Barzuza, 2009). Likewise, the 1991 Delaware's court ruling of the Credit Lyonnais case shifted the fiduciary duties of corporate directors towards creditors (from shareholders) for distressed firms (Becker and Strömberg, 2012). To ensure that our results are not driven by the Delaware-incorporated BHCs, we exclude them (down to 7,273 observations) and reestimate the baseline models. As row (6) shows, the subsample results remain similar despite being less significant.

Since the included bank and state control variables may be influenced by the adoption of constituency statutes, a problem of "bad controls" may arise (see Angrist and Pischk, 2008). Row (7) shows that results from regressions that only include the treatment variable and fixed effects are similar.

Economy-wide shocks may cause bank characteristics to affect risk taking differently in different periods. Rows (8) to (14) test whether the statute effects are sensitive to controlling for such differences. We interact bank controls, one at each time, with the year dummies and find that our results hold.

Since the majority of statutes were enacted in the late 1980s (in 25 of 35 states over 1986-1990) and that our sample covers 1986-2012, there are more "after treatment" than "before treatment" years. Rows (15) an (16) show that our results hold when subsamples ending in either 1995 or 2005 are used.

The extant literature acknowledges that bank competition affects risk taking (see, e.g., Martínez-Miera and Repullo, 2010). Row (17) controls for the Herfindahl Index (*H-index*) of loan concentration and its squared term in the model and shows that our results are unaffected.[12]

There are some inconsistencies in the legal literature about the enactment year of constituency statutes. Compared to those reported by Karpoff and Wittry (2018), 7 states in our sample have statutes enacted at different years, including Connecticut, Indiana, Kentucky, Maine, Missouri, Nebraska, and Texas. To ensure that such discrepancies do not drive our results, row (18) excludes all BHCs incorporated in these 7 states (down to 8,200 observations), confirming that our results hold.

Our results may be driven by those banks who are deemed to be systemically important, have responded to the capital adequacy policy differently, and happen to be in the states with statute passage. To address this issue, we collect a list of 19 systemically important financial institutions (SIFIs) from the 2011 Comprehensive Capital Analysis and Review, and a list of 11 banks to which the Comptroller of the Currency intended the "too big to fail" policy to apply in September 1984 from O'Hara and Shaw (1990). Our results (unreported) hold after excluding these systemically important banks from our sample (see Table IA.4 in the online appendix for more details).

Finally, culture has been shown to shape bank risk-taking behaviors (see, e.g., Fahlenbrach, Prilmeier, and Stulz, 2012). Since cultural traits are hard-to-measure, we exploit the stylized fact that culture is persistent and control for its role with two sets of fixed effects. Based on the notion that banks of different size may differ in business models, the first is size-decile fixed effects. The second is crisis-performance-decile fixed effects. We sum a bank's annual stock returns in both 1998 and 2008, divide banks into deciles using this crisis-return measure, and apply this decile ranking to the full sample of banks. Since culture explains the variation in bank crisis performance, this set of crisis-performance fixed effects likely capture some cultural variation. Unreported tests show that our results are robust after including these fixed effects (see Table IA.5 in the online appendix for more details).

Insert Table 4 about here

[12] The *H-index* is defined as the sum of BHCs' squared market shares in total loans within the state of location in a given year. Our results remain robust when *H-index* is defined as the sum of BHCs' squared market shares in total loans within the state of incorporation in a given year.

4.3. Additional Endogeneity Tests

4.3.1. Addressing the Reverse Causality Concern

A potential endogeneity concern is reverse causality. For instance, lawmakers may respond to negative events owing to excessive risk taking and enact policies favoring stakeholders. Two approaches to rule this concern out are adopted.

First, we examine whether bank risk taking determines the timing of statute enactment using a hazard model. Table 5 tests whether the three state-average bank-risk measures influence the likelihood of a state adopting the statute in a specific year given that no statute has been enacted yet. The sample includes 35 states that have received a statute passage. Sample period covers 1980-2007 (the last statute was passed in Nebraska in 2007) (except column (3) where the sample covers 1986-2007 due to data restriction). Additional state controls relating to the workplace and political environment are included, such as union coverage rates, state unemployment rates, amount of unemployment benefits, and governor party. As shown, none of the three state-average bank risk measures enter the regressions significantly.

Insert Table 5 about here

Second, we estimate the dynamic treatment model as follows:

$$Risk_{islrt} = \alpha_t + \alpha_i + \alpha_r \times \alpha_t + \lambda_1 \times Before^{-2\ or\ -1}{}_{st} + \lambda_2 \times Current^{0}{}_{st} + \lambda_3 \times After^{+1}{}_{st} + \lambda_4 \times After^{>=+2}{}_{st} + \delta X_{islrt} + \gamma W_{lrt} + \varepsilon_{islrt}, \quad (4)$$

where $Before^{-2\ or\ -1}{}_{st}$ is a dummy variable equal to one for each of the two years preceding enactment of a constituency statute, $Current^{0}{}_{st}$ is a dummy variable equal to one for years of statute enactment, $After^{+1}{}_{st}$ is a dummy variable equal to one for the year after statute enactment, and $After^{>=+2}{}_{st}$ is a dummy variable equal to one if it is two or more years after a statute enactment. The coefficient estimate for $Before^{-2\ or\ -1}{}_{st}$, λ_1, is of particular interest as its magnitude and statistical significance indicate whether the pre-trends (in bank risk) are systematically different between the treated and control BHCs.

Table 6 reports the results of the dynamic treatment analysis. We find that the coefficient estimates for $Before^{-2\ or\ -1}{}_{st}$ are small and insignificant in general for all of the three risk measures, suggesting no systematic differences in pre-trends between the treated and control BHCs and that the parallel trend assumption is likely satisfied (Roberts and Whited, 2012). Compared to the pre-treatment years, we observe an increase in the negative statute effects on bank risk-taking at the year of statute enactment as shown by the considerably larger and significant (for *IVol* and *Z-score*) coefficient estimates for $Current^{0}{}_{st}$. In the one

year after statute enactment, the negative statute effects generally become stronger, and the coefficient estimates for $After^{+1}_{st}$ are significant for *Total risk* (at the 10% level), *IVol* (at the 10% level), and *Z-score* (at the 1% level). Finally, the coefficient estimates for $After^{>=+2}_{st}$ are the largest compared to other periods and are significant for all models, suggesting that the negative statute effects have persisted in the longer-term and that reverse causality does not drive our results.

Insert Table 6 about here

4.3.2. Placebo Tests

We perform three placebo tests. Our first test keeps the distribution of the enactment years unchanged and randomly assigns states into each of these years (without replacement) [13], allowing any unobservable shocks occurring at about the same time as the statute passage to remain in the analysis. The second test maintains the distribution of the treated states but randomizes their enactment years, allowing any persistent state effects to continue to drive our results. The third test randomizes both the enactment years and the states (with no replacement for states). Within each placebo sample, we estimate the baseline model of equation (3) and store the coefficient and standard error estimates for the placebo statute. We repeat this procedure 5,000 times to obtain a distribution of the placebo estimates for each test.

In unreported results, we find that the effect of the placebo statute is small and insignificant for all placebo tests. In the first placebo test, the actual estimate for *Constituency Statute* is larger than the placebo estimate in 4,995 out of 5,000 placebo samples (99.9%) for *Z-score*, implying a 0.1 of 100 chance of randomly observing the actual estimate when the null of no statute effect is in fact true (Rosenbaum, 2009). For the remaining risk measures, the actual estimate is smaller than the placebo estimate in at least 4,638 of 5,000 (92.8%) cases. The second and third placebo tests yield similar and even more significant results. These unreported results can be found in Table IA.6 of the online appendix. Overall, the placebo tests show that our results could not have been driven by chance, and by unobserved market-wide shocks, persistent state effects, and the imperfect structure in the standard error matrix.

[13] For instance, in 1990, the constituency statutes were passed in Mississippi, Pennsylvania, and Rhode Island. In the remaining 48 states, we randomly choose three states and assign a treatment event to them. We do this for each year with at least one statute passage and are careful not to assign a treatment to states which have already been treated (i.e., with no replacement).

4.3.3. Different Event Windows Surrounding the Statute Enactment

Given the unbalanced nature of our panel data, our staggered difference-in-differences setting is subject to a potential "survival" issue to the extent that certain banks are more likely to enter/exist our sample than others across years. In turn, this sampling issue may render our results less credible.

To address this issue, for each statute enactment, we match each treated bank with a control bank that is closest in size in the pretreatment year. We keep the three years before and after (i.e., the [-3, +3] window) the enactment for both banks, excluding the treatment year, and estimate baseline bank risk regressions on the matched sample (i.e., a balanced panel now). As shown in columns (1) to (3) of Table 7, our results remain similar on this matched sample. Columns (4) to (6) and columns (7) to (9) further restrict the sample to be within the windows of [-2, +2] and [-1, +1]. While sample size reduces, our results remain robust in these alternative windows. Overall, this "survival" issue is unlikely to be severe. Summary statistics for these matched samples can be found in Table IA.7.

4.3.4. Unobservable Confounding Local Economic Conditions

A potential concern is that unobservable changes in local economic conditions, driving both the introduction of constituency statutes and bank risk taking, may bias our results. A plausible example is that politicians tend to be more successful in introducing a constituency statute in a booming economy (Flammer and Kacperczyk, 2016), and such favorable economic conditions may determine bank risk taking. We perform two tests to address this concern.

First, we run a placebo test that uses the treated states' (pre-treatment) GDP-matched bordering states as placebo states. Since the treated and placebo states likely experience similar economic conditions, if the coefficient estimate for the placebo statute continues to be significant, our earlier results could not have been caused by the statute. Unreported results show that the placebo statute has small and insignificant coefficients in all models (see Table IA.8 of the online appendix).

Second, we exploit the discontinuity in the effects of constituency statutes on bank risk between the treated BHCs and the control BHCs located in adjacent states, just across the border. Since local

economic conditions tend to spill across state borders, it will therefore be the case that if the statutes are driven by unobserved changes in local economic conditions and if these changes (rather than the statutes) also affect bank risk taking, the treated and control BHCs in bordering states would spuriously appear to respond to the statutes. Thus, this concern predicts no significant differences in bank risk between the treated BHCs and the control BHCs in bordering states.

We match each treated BHC to a control BHC located in a bordering state without statute passage and is closest in size.[14] For each matched BHC pair, we keep the year before and after the statute passage for the analysis. To ensure that the treated and control BHCs in bordering states are subject to similar local economic shocks, we remove all matched pairs with a distance greater than 500 miles (from 166 down to 130 pairs). We further restrict the distances to be within 250 miles (down to 99 pairs) and 125 miles (down to 44 pairs)[15]. The diagnostics for these matched samples can be found in Table IA.9 of the online appendix. We estimate equation (3) on the matched samples and report the results in Table 8.

Insert Table 8 about here

In Panel A, with a 500-mile distance restriction, the coefficients for *Constituency Statute* are significant at the 5% level or better for all three models, again revealing a negative statute effect on bank risk. Panels B and C restrict the distance to be within 250 and 125 miles, respectively. While the power of the tests becomes more constrained in the smaller samples, our results remain similar. In Panel C, where distance is within 125 miles, we still find a significantly negative (for *Total risk* and *IVol*) statute effect on bank risk. Our results are unlikely driven by unobservable confounding local economic conditions.

4.3.5. Coincidental State Antitakeover Laws

The literature shows that after the adoption of state antitakeover laws, managers who are risk-averse and now face increased takeover protection may seek to reduce firm risk (see, e.g., Bertrand and Mullainathan,

[14] Of the 166 treated BHCs, 10 are located and incorporated in different states. We match each of these 10 BHCs with a control BHC located in the same state of location but incorporated in another state. Our conclusions are unaffected if we match each of these 10 BHCs with a control BHC located in a bordering state.

[15] The restrictions of 250-mile and 125-mile correspond to the 75th and 50th percentiles of distance in the final matched sample. We do not further restrict the distance to be within 100 miles (the 25th percentile, observations drop to 124) to avoid the small sample problem.

2003; Garvey and Hanka, 1999). If some of the constituency statutes were passed at similar times with the antitakeover laws, the reduced bank risk we document might be driven by these confounding laws.

To address this concern, we collect information on the passage of three major state antitakeover laws, namely the control share acquisition laws, business combination laws, and the fair price laws, from Bertrand and Mullainathan (2003) (these details can be found in Table IA.10 of the online appendix). We define a state as "contaminated" by other antitakeover laws if its constituency statute was enacted in the one year prior to, during the year of, and in the one year after a passage of any of the antitakeover laws and have identified 16 contaminated states and 66 treated BHCs (39.8% of the total treated BHCs) incorporated in these states. As a first test, Panel A of Table 9 excludes these 16 contaminated states (down to 6,796 observations) and finds that our baseline results hold.

Insert Table 9 about here

Panel B presents a more formal test to distinguish the effect of constituency statutes from those of the antitakeover laws. Specifically, we construct an indicator variable for each of the three major antitakeover laws, which equals one after the law passage and zero otherwise. We then estimate the baseline risk models and jointly control for the three antitakeover law variables in the regressions. Our results reveal that none of the state antitakeover laws has a significant effect on bank risk. However, the constituency statutes remain highly significant in reducing bank risk. Together, our results are unlikely be driven by the omitted variables problem relating to coincidental state antitakeover laws.

4.3.6. Coincidental Banking Policy Changes

A similar potential concern is that the enactment of constituency statutes may have coincided with several US state banking deregulation policy changes which may affect bank competition and risk-taking.

During the 1970-1990s, the US states deregulated *interstate* banking and *intrastate* branching activities. These staggered policy changes across states and time may have driven our results if the resulting increases in geographical expansion affected bank business models, competition, and thus risk-taking. Moreover, while the Interstate Banking and Branching Efficiency Act (IBBEA) (in 1999) removed all remaining federal restrictions on interstate banking as of 1995, it allowed states to have their own discretion

in erecting out-of-state entry barriers to interstate branching. Such staggered erection of entry barriers may affect bank risk through changing the banking competitive environments (more details about these policy changes can be found in Table IA.11 of the online appendix).

To address this concern, we exclude BHCs incorporated or located in 10 contaminated states (2,051 observations excluded) in which the statute enactment is either in the *one year before*, *during the year of*, or *in the one year after* a banking deregulation policy change, removing 30 treated BHCs in total (18.1% of the total treated BHCs). Panel C of Table 9 shows that our results are similar.

Further, we control for these coincidental banking policy changes in the baseline risk models. We construct two indicator variables, *Inter* and *Intra*, which equal zero before the interstate and intrastate banking deregulations, and one otherwise. We use the Rice and Strahan (2010) index (*RS index*), which ranges from zero (the least restrictive) to four (the most restrictive), to measure the state branching restrictions. Panel D jointly controls for *Inter*, *Intra,* and *RS index* in the models and finds that our results hold, suggesting that these coincidental banking policy changes do not drive our results.

4.3.7. Using Alternative Bank Risk Measures

This section presents additional evidence on the impact of constituency statutes using four alternative bank-risk measures, three of which are more directly related to bank stability.

First, we use an alternative specification to estimate bank idiosyncratic risk following Gandhi and Lustig (2015) who document a significant size effect specific to banking firms. Following their sample selection, we annually sort banks into size-decile portfolios at the beginning of the year based on market capitalization and compute value-weighted portfolio returns. On each portfolio we estimate the Fama-French three-factor model augmented with the daily changes in the 10-Year US Treasury Benchmark bond yield and the daily changes in the Moody's AAA corporate bond yield[16] (henceforth, "expanded FF3

[16] Gandhi and Lustig (2015) augment the Fama-French three-factor model with the excess returns on the US 10-year Government Bond Total Return Index from Global Financial Data and the excess returns on an index of investment grade corporate bonds from Dow Jones. However, due to data unavailability, we replace these two bond-related factors with the daily changes in 10-Year US Treasury Benchmark bond yield and the daily changes in the Moody's AAA corporate bond yield and obtain the estimated residuals. For robustness, we also collect the daily returns of the Bank of America AML US Treasury Index and the daily returns of the Bank of America AML US Corporate Index, which only have daily data since November 1986. A significant size effect is similarly confirmed when we use these

model"). Principal component analysis is then applied to the portfolios' residuals. The bank size factor is constructed as the linear combination of estimated loadings of the second principal component and portfolio residuals. For each bank in a given year, we estimate the expanded FF3 model augmented with the bank size factor and compute the annualized residual volatilities (*IVol (Gandhi and Lustig)*). Column (1) of Table 10 shows that the enactment of constituency statutes remains significantly and negatively associated with this alternative measure of bank-specific risk.

Insert Table 10 about here

Second, to more directly capture the risk of bank default, we closely follow Bharath and Shumway (2008) and estimate a bank's expected default frequency (*EDF*) using the KMV-Merton model (see Table IA.14 in the online appendix for the estimation details). According to the authors, *EDF* is more accurate in predicting the likelihood of corporate defaults, compared to measures from traditional models. Higher *EDF* denotes greater expected default probability. Column (2) shows that statute enactment is associated with significantly (at the 10% level) lower *EDF,* consistent with greater bank stability.

Third, to capture bank systemic risk exposure, we follow de Jonghe (2010) and estimate the probability of a sharp decline in a bank's stock price conditional on a crash in a US banking index[17], denoted as *Tail beta,* using extreme value analysis (see Table IA.15 of the online appendix for the estimation details). Following de Jonghe (2010), *Tail beta* in year *t* for a given bank is estimated using all available daily bank returns from year *t* to *t+5* (6 years of data; returns data for the sample banks are extended to 2017 to estimate *Tail beta* for year 2012). Column (3) presents a bank fixed effects regression (i.e., a linear probability model) with *Tail beta* as dependent variable. Results show that statute passage has a significantly negative impact on *Tail beta*. Specifically, passing a statute reduces the co-crash probability by 3.3 percentage points (mean of *Tail beta* is 0.133). For robustness, since *Tail beta* is bounded between zero and one, i.e., fractional, a generalized linear model (GLM) with a probit link function is estimated (Papke and Wooldridge, 1996).

alternative indexes to construct the size factor and our baseline results remain robust (see Table IA.12 and IA.13 of the online appendix).

[17] The US bank index we use is computed as the value-weighted average returns of all US banks selected following the criteria in Gandhi and Lustig (2015). For robustness, we use an alternative bank index compiled by DataStream that includes the major, large US banks, and find similar results. These results can be found in Table IA.15 of the online appendix.

Our results (unreported) hold. To further accommodate the unbalanced nature of our sample, we follow Wooldridge (2011) and estimate a nonlinear fractional response model allowing for unobserved heterogeneity in unbalanced panel. Our results (unreported) under this alternative estimation approach are similar qualitatively.

Fourth, following DeYoung and Torna (2013) in spirit, we examine whether statute enactment influences the incidence of bank failures and bankruptcies at the state level. For each state in a given year, we collect the number of "problem" banks from the Federal Deposit Insurance Corporation's (FDIC) Failures and Assistance Transactions - Historical Statistics on Banking database. "Problem" banks comprise those with transaction type classified as either "All failures" or "Assistance Transactions". We estimate a fixed-effects Tobit model censored at zero, regressing the log of one plus the number of problem banks on the treatment variable, state controls (the same as those used in Table 5), and state and region-year fixed effects.[18] Column (4) shows that the number of problem banks declines significantly following statute enactment, again consistent with improved bank stability (see Table IA.16 of the online appendix).

In unreported analysis (see Table IA.17 of the online appendix), we run linear probability models to test whether statute enactment affects credit ratings (from Compustat). Due to limited data availability of the rating data, our sample is much smaller and the DID estimate is positive but insignificant.

4.3.8. Using an Alternative Bank-Level Stakeholder Orientation Measure

In this section, we use an alternative bank-level measure of stakeholder orientation using data from the MSCI KLD database. KLD data captures how well firms care for their stakeholders in several dimensions, such as community, employees, diversity, etc. Following Deng, Kang, and Low (2013), a two-step procedure to construct an overall rating (*KLD*) for each BHC is used.[19] On a merged KLD sample

[18] Until recently, the maximum likelihood estimator (MLE) in nonlinear panel data models with fixed effects is thought to be biased and inconsistent when the length of the panel data is small and fixed, the so-called "incidental parameters problem". Using Monte Carlo simulation, Greene (2004) examines the behavior of the MLE of the fixed-effects Tobit model and finds that the estimators' slope coefficient is unaffected by the "incidental parameters problem", though there remains some bias in the estimated standard errors. Nonetheless, since this bias is shown to be mild when T is 5 and larger, little bias is expected in our estimation as our sample consists of 27 years.

[19] We check our results to alternative approaches in constructing *KLD*, including no standardization, the inclusion of the human rights and corporate governance dimensions, and the use of indicator variables that are only available throughout our sample period, and confirm that our results are robust. These results are available upon request.

comprising 314 BHCs (1,619 bank-year observations) over 1991-2010[20], bank risk is regressed on *KLD*, the bank and state controls, and fixed effects. Standard errors are clustered at the bank level. Consistent with our DID results, a significantly negative association between *KLD* and bank risk is documented.

A potential concern is that the relationship between bank stakeholder orientation and risk may be dynamically endogenous: past risk may influence both current risk and stakeholder engagement (see Wintoki, Linck, and Netter, 2012). A dynamic panel system Generalized Method of Moments (GMM) estimator (Arellano and Bover, 1995; Blundell and Bond, 1998) that allows us to control for lagged risks and use the BHCs' past information as instruments is used. Our results (untabulated) show that *KLD* remains negatively and significantly associated with bank risk (see Table IA.19 in the online appendix).

4.4. Constituency Statutes and Bank Risk Taking: Further Evidence

This section explores two channels through which bank risk could be reduced.

4.4.1. Constituency Statutes and Bank Capital

An important way banks could lower risk is to alter their capital structure. The literature posits that higher capital levels mitigate moral hazard (risk-shifting) problems and contribute to stability (see, e.g., Acharya, Mehran and Thakor, 2016; Thakor, 2015, for a review). First, banks may exploit the deposit insurance put option (Merton, 1977) or respond to the shareholder-stakeholder conflict (Jensen and Meckling, 1976; Macey and O'Hara, 2003), and choose to invest in excessively risky and inefficient assets. Since these risk-shifting incentives would become more severe the more highly leveraged the bank is, increasing equity capital helps to curb excessive risk taking (see, e.g., Merton, 1977; Coval and Thakor, 2005). Moreover, equity capital helps banks to better absorb losses with their own resources and prevents them from being insolvent and requiring government bailouts, thereby enhancing stability and social welfare (Demirgüç-Kunt, Detragiache, and Merrouche, 2013). To the extent that the enactment of constituency statutes expands

[20] The summary statistics of this KLD-merged BHC sample are provided in Table IA.18 of the online appendix.

bank directors' authority and discretion to pursue more prudent banking policies in favor of stakeholders and stability, these directors may encourage banks to increase capital levels.

Insert Table 11 about here

Table 11 presents baseline models replacing bank risks with capital ratios. The same controls and fixed effects as in the baseline models are used. To isolate the effect of statute passage on bank capital from that of other coincidental events, the state antitakeover laws and banking deregulation policy variables are controlled for. Standard errors are clustered at the state of incorporation.

Columns (1) and (3) of Table 11 show that statute enactment significantly (at the 5% level or better) increases bank capital-to-assets ratios (*Capital/TA*) by 6.9% and capital-to-loans ratios (*Capital/TL*) by 7.7%, relative to their sample means. In columns (2) and (4), where the dynamic treatment effects for both capital measures are examined, the significant increases in bank capital occur one year after the statute enactment and become more significant in subsequent years. In addition, the statute effect in the two years prior to statute enactment are small and insignificant, suggesting little differences in pretrends between the treated and control BHCs.[21] Overall, this evidence is consistent with BHCs reducing risk taking through maintaining a higher capital level to buffer losses and mitigate moral hazard conflicts.

4.4.2. Constituency Statutes and Bank Lending Activities

Next, we examine whether banks also reduce risk through adjusting their lending (the lending channel) and argue that there are at least three ways through which banks could achieve that.

First, banks could reduce the amount of risky lending, which would be reflected by a reduction in total loan volume. Second, banks could choose not to reduce lending volume but rather shift lending from riskier to safer borrowers. If this is true, after statute passage, there would be little changes in total loan volume, an improvement in loan quality, and lower interest margins due to cheaper loan prices. Third, given that the statute enactment has expanded bank directors' responsibilities to consider more long-term results

[21] Our findings may be alternatively interpreted: a reduction in bank leverage, as opposed to an increase in bank capital, leads to lower bank risk, as leverage is essentially one minus bank capital-to-asset ratio. Moreover, in unreported analysis, we show that results from our baseline bank-risk regressions continue to hold, despite being slightly less significant, after controlling for book and market leverage (see Table IA.20 in the online appendix).

and be less tolerant to moral hazard problems, the increased risk oversight by these directors may lead to an improved lending quality or efficiency. If that be the case, we may find little changes in bank total loan volume, improved loan quality, and little decrease in bank interest margins.

To distinguish between these views, we estimate the baseline and dynamic treatment models to examine how statute enactment influences bank total loans, loan quality, and net interest margins. The same bank and state controls as in the baseline models as well as the state antitakeover laws and banking deregulation policies are included. Standard errors are clustered at the state of incorporation.

Insert Table 12 about here

Column (1) of Table 12 shows that statute enactment is insignificant in explaining log total loans (*Ln(Loans)*). The dynamic treatment model of column (2) shows similarly that the statute effect in all periods around the enactment is small and insignificant. The first explanation is not supported.

We test whether the statute enactment influences bank loan quality. Loan quality is measured by non-performing loans, defined as the sum of loans 30 days or more past due and nonaccrual loans, scaled by total assets (*NPL/TA*).[22] Since BHCs are required to report these two data items in the FR Y-9C reports beginning from 1990, the tests on loan quality are performed on a subsample covering 1990-2012. This data issue reduces the number of treated BHCs to 31. To address potential selection issues, we follow the matching procedure as described in Section 4.3.4 and match each of the 31 treated BHCs with a control BHC located in a bordering state and closest in size to create a matched sample for the DID test. The matching is satisfactory in removing the differences in observables between the two groups (see Table IA.9 of the online appendix).

Column (3) shows a significantly (at the 10% level) negative effect of constituency statutes on *NPL/TA*. To gauge the economic magnitude, passing the statutes reduces *NPL/TA* by 16.3% relative to the sample mean. Column (2) reports results based on the size-location-matched sample, showing qualitatively similar results. The dynamic treatment model in column (5) shows that the significantly negative statute effect on *NPL/TA* occurs at the year of and in the one year post treatment, whereas the pretreatment effect

[22] In unreported results, we scale non-performing loans by total loans and find qualitatively similar results.

is small and insignificant. Our evidence suggests that statute passage improves loan quality, consistent with both the second and third explanations.

Finally, to distinguish between the second and third explanations, we test whether the overall loan prices, as measured by the ratio of net interest income to total assets (*Net interest income/TA*) following Ho and Saunders (1981), have changed after statute passage. As column (6) shows, the coefficient for *Constituency Statute* is negative and significant at the 1% level, implying that the statutes reduce loan prices. Specifically, the statute enactment reduces bank net interest margin by 3.1% relative to the sample mean. Column (7) presents the dynamic treatment model showing that the negative statute effect on interest margin is significant from the year of enactment onwards, whereas the statute effect in the two years prior to the enactment is insignificant. Overall, our results are most consistent with the second explanation that banks did not reduce risky lending but rather lent to safer borrowers.

4.5. Alternative Explanation

As previously discussed, the introduction of constituency statutes was initially triggered by the merger waves in the 1980s. While the statutes' applications and implications were not limited to takeovers, one may argue that they might serve primarily as takeover defenses and reduced takeover threat induces bank managers to pursue a "quiet life" and avoid the difficult decisions and costly efforts in risk taking (Bertrand and Mullainathan, 2003). Likewise, state antitakeover laws could also lower bank financing costs through reducing the agency costs of debts (Francis, Hasan, John, and Waisman, 2010).

Two tests are run to rule this explanation out. First, we divide the statutes according to whether they allow the considerations of stakeholder interests in *any* instance of business decision making (24 statutes) as opposed to *only* change-of-control situations (11 statutes).[23] If the statutes were primarily used as takeover defenses, one would not expect the statute effects to vary with their scope of application. Our results (unreported) show that the reduction in bank risk concentrates on those statutes allowing stakeholder interests to be considered in a *wider* range of business decision making.

[23] We collect this information from Oswald (1998, p.5, see footnote 25) and manually check each statute. The details can be found in Table IA.2 of the online appendix.

Second, we test whether statute enactment affects the acquisition likelihood of BHCs. Acquisition likelihood is measured by the number of BHCs acquired in a state of incorporation[24] and the total number of BHCs acquired in a state of incorporation scaled by the total number of BHCs incorporated in the same state. State-averaged bank controls, state controls, state antitakeover laws and banking deregulation policies, state and region-year fixed effects are included in the regressions. Standard errors are clustered at the state level. Our results (unreported) indicate that the statute passage has little effect on bank acquisition likelihood and thus the threat of takeover, inconsistent with the alternative explanation (these unreported results can be found in Table IA.21 of the online appendix).

4.6. Constituency Statutes and Bank Performance

So far, we have shown that after statute passage, banks reduce risk by adopting less risky capital structures and lending to safer borrowers. We now study the value implications of the reduced risk taking.

The effect of constituency statutes on bank performance is somewhat ambiguous. On the one hand, since statute passage significantly enhances loan quality, the reduced loan losses may increase profitability. On the other hand, the lower interest margins owing to lending to safer borrowers may hurt bank performance. Thus, the overall statute effect on bank performance may depend on the relative significance of the two effects aforementioned, which is, therefore, ultimately an empirical question.

To test this question, the DID regressions of equation (3) with return on assets (*ROA*), bank stock annual returns (*Annual returns*), and log market-to-book equity ratio (*Ln(Market-to-book equity ratio)*) as dependent variables are estimated. *Annual returns* is the buy-and-hold return on the BHC stock over a calendar year. In the performance regressions, we include the same set of bank and state control variables (except *ROA* and *Market-to-book equity ratios*), and fixed effects as in the baseline model. The market beta (*Market beta*) estimated from equation (1) is included to control for BHCs' exposure to market risk. The

[24] The BHC mergers are collected from the Thomson One Banker. Our final merger sample is established based on the following criteria: (1) the target ultimate parent firms are public and have 3-digit SIC codes: 602 or 671; (2) deals are removed if they are classified as repurchases, liquidations, restructurings, divestitures, leverage buyouts, reserve takeovers, privatizations, bankruptcy acquisitions or going private transactions; (3) the company identifier, PERMCO, could be obtained from the CRSP/Compustat merged dataset. The final sample consists of 656 BHC mergers over the period from 1986 to 2012.

state antitakeover laws and banking deregulation policy changes are included in all regressions. Standard errors are clustered at the state of incorporation. The results are reported in Table 13.

Insert Table 13 about here

The positive but insignificant coefficient for *Constituency Statute* in column (1) indicates that there are little changes in bank profitability after statute enactment. In column (3), where *ROA* is replaced with *Annual returns*, we continue to find an insignificant coefficient for *Constituency Statute*. Finally, column (5) shows that the statute effect on bank market-to-book equity ratio is similarly insignificant. The overall effect of stakeholder orientation on bank performance appears to be small and insignificant.

We identify a setting in which the marginal benefits of risk reduction are likely to exceed its marginal costs. In theory, banks with severe moral hazard problems have greater incentives to invest in excessively risky and inefficient assets that are high-risk-low-return (Merton, 1973; Jensen and Meckling, 1976; Acharya, Mehran, and Thakor, 2016). Such inefficiency would make a shift from riskier to safer borrowers more desirable because the gain from mitigating loan losses is likely to outweigh the reduction in loan interest income. By contrast, for banks characterized by safer borrowers, such a shift would be less appealing because of the lower marginal benefit from reducing loan losses. Therefore, we conjecture that the enactment of constituency statutes improves bank performance only for banks that take excessive risk, but may not affect or may even reduce performance for banks that do not.

To test this conjecture, a dummy variable, *High risk*, which equals one for BHCs with above-size-adjusted-average *Total risk* and zero otherwise, is constructed. To adjust for size, we annually sort BHCs into five portfolios based on size and compute portfolio-average total risk. We posit that a *High risk* bank is more likely to take excessive risk and would benefit more from shifting to safer borrowers. We augment the performance regressions with the interaction between *Constituency Statute* and *High risk*.

In column (2), where *ROA* is the dependent variable, we find that the coefficient for *Constituency Statute*×*High risk* is positive and significant at the 5% level, consistent with statute passage improving profitability only for banks that likely engage in excessive risk taking. Economically, for *High risk* banks, enacting the constituency statute increases their *ROA* by 0.18 percentage points, corresponding to a 16.4%

increase relative to the sample mean, or a $31.9 million increase in earnings (mean total assets is $17.7 billion). As expected, the statute effect is insignificant for the low-risk banks.

In column (4) where *Annual returns* is the dependent variable, we again find that the coefficient for *Constituency Statute×High risk* is positive and statistically significant (at the 1% level). For *High risk* banks, the enactment of constituency statute increases their annual stock returns by 0.6 percentage points or 4.3% relative to the sample mean, whereas the statute effect is insignificant for the low-risk banks.

Column (6) shows very similar results for *Ln(Market-to-book equity ratio)* in that the coefficient for *Constituency Statute×High risk* is again significantly positive at the 5% level. For *High-risk* banks, enacting the statute increases market-to-book equity ratio by 4.2% relative to the sample mean.[25] Similarly, we find insignificant statute effects on bank market-to-book equity ratio for the low-risk banks.

Overall, the effect of stakeholder orientation on bank performance is small and insignificant on average. However, stakeholder orientation appears value-enhancing only for banks who likely engage in excessive risk taking but not for banks that do not.

4.7. Constituency Statutes and Crisis Performance

Our results thus far show that the enactment of constituency statutes significantly reduces bank risk taking by inducing banks to adopt less risky capital structures and to lend to safer borrowers. The previous section further shows that statute passage improves bank performance only for the high-risk banks. This section explores the long-term value implications of stakeholder orientation by testing whether the reduced risk taking translates into better performance during the subsequent financial crises.

Recent studies document that a bank's "risk culture" is important for determining its crisis performance. For instance, Fahlenbrach, Prilmeier, and Stulz (2012) document that bank underperformance during the 1998 crisis predicts lower bank performance during the 2007-09 crisis, consistent with risk culture being persistent and making banks sensitive to crises. Ellul and Yerramilli (2013) find that banks with more tail risk during the 1998 crisis continued to have poorer risk control, consistent with bank culture

[25] This is calculated as: exp*(0.041)-1=0.0419 or 4.2%.*

being fairly rigid. To the extent that the constituency statutes have influenced BHCs' risk culture in favor of stakeholders and stability, we hypothesize that BHCs that have previously received a statute passage incur smaller losses during the financial crises relative to control BHCs.

We analyze bank stock performance during both the 1998 and 2007-09 crises. Our sample for the 1998 crisis begins with all available BHCs (378 BHCs) over 1997-98. All BHCs that have been treated before 1998 (88 BHCs) are in the treatment group. Similarly, for the 2007-09 crisis, we begin with all available BHCs (304 BHCs) over 2006-09 of which 42 of them have been treated before 2007. We match each treated BHC with a control BHC that is incorporated in a state that has not passed or does not have a constituency statute and is closest in size before the crisis. We require the control BHCs to be located in a bordering state of the treated BHC to increase the likelihood that the treated and control BHCs experience similar economic conditions. Following Fahlenbrach, Prilmeier, and Stulz (2012), we define three measures of 1998 crisis returns as the buy-and-hold bank stock returns from 3rd August 1998 to the date on which it attains the lowest stock price ($Crisis_{1998}\,RET_1$), to 30th September 1998 ($Crisis_{1998}\,RET_2$), and to 30th October 1998 ($Crisis_{1998}\,RET_3$). For the 2007-09 crisis, we define three crisis performance measures as the buy-and-hold bank stock returns from 1st July 2007 to the date on which a bank attains its lowest price ($Crisis_{07\text{-}08}\ RET_1$), to 31st December 2008 ($Crisis_{07\text{-}08}\ RET_2$), and to 31st December 2009 ($Crisis_{07\text{-}08}\ RET_3$).[26] We estimate the following DID model separately for the two crises:

$$\Delta RET_{isl} = \alpha + \beta \times Previously\ Treated_s + \delta X_{isl} + \gamma W_l + \varepsilon_{isl}. \quad (5)$$

where i is a bank, s is the state of incorporation, and l is the state of location. ΔRET_{isl} is the changes in bank stock returns calculated as the crisis returns (*Crisis RET*) minus the precrisis returns (*Annual returns*). $Previously\ Treated_s$ is a treatment dummy equals one for BHCs that have been treated prior to the crisis and zero otherwise. X_{isl} and W_l are vectors of precrisis bank and state controls, respectively, augmented by the precrisis market beta, the three state antitakeover laws, *RS index,* and *Intra*.[27] White heteroscedasticity-

[26] If a bank was delisted or merged during 1997-98 or 2007-09, we put the proceeds into a daily bank index until the end of 1998 or 2009, respectively, to avoid potential survivorship bias. The bank index is collected from French's data library under the 49-industry classification. Our results hold if the proceeds are instead put in a cash account.

[27] We do not control for state interstate deregulation because all BHCs are located in deregulated states. Market beta is estimated using daily bank stock returns data using equation (1) and controls for a bank's market risk exposure.

robust standard errors are reported. β is a DID estimate capturing the differences in the Crisis-minus-Precrisis bank stock returns between the treatment and control groups.

Insert Table 14 about here

Panel A of Table 14 compares the precrisis bank and state characteristics between the treatment and control groups for both crises. All bank and state characteristics are insignificantly different between the two groups except for non-interest income and state population for the 2007-09 crisis, suggesting that the matching is in general satisfactory in eliminating the differences in observables.

Panel B presents the estimates for the Crisis-minus-Precrisis differences and DID of the bank stock returns. For the 1998 (2007-09) crisis, the after-before differences in bank stock returns range from -76.8 to -91.9 (-36.0 to -58.0) percentage points for the treated BHCs and from -88.8 to 105.3 (-61.7 to -79.1) percentage points for the control BHCs. These after-before differences are all statistically significant at the 1% level, consistent with the stylized fact that BHCs suffered huge losses during both crises. The DID (treatment-control) estimates range from 12.0 to 13.3 percentage points for the 1998 crisis and from 21.1 to 25.8 percentage points for the 2007-09 crisis, all significant at the 5% level, revealing that the treated BHCs fared significantly better than the matched BHCs during both crises.

Panel C reports the DID estimates after we control for the precrisis bank and state control variables. The DID estimates range from 6.5 to 9.7 percentage points for the 1998 crisis (significant at the 10% level or better for *Crisis* RET_1 and *Crisis* RET_3) and from 13.8 to 33.8 percentage points for the 2007-09 crisis (significant at the 1% level for all three crisis performance measures).

In sum, our evidence suggests that reduced bank risk taking due to statute passage translates into significantly smaller losses during subsequent crises, consistent with stakeholder orientation enhancing stability. As a final test, we collect a list of banks that have received TARP rescue money during and after the crisis from the TARP Investment Program Transaction Reports. Since TARP receipt banks likely suffered greater losses, we test whether previously treated banks have a lower likelihood of being a TARP receipt. Our results are insignificant but the sign of the estimate on the previously-treated dummy is consistent with our conjecture (for more details, see Table IA.22 of the online appendix).

5. Conclusion

The 2007-09 financial crisis has raised widespread concern about whether the traditional shareholder primacy view in bank governance model is detrimental to financial stability. Theory suggests that induced by deposit insurance and due to limited liability, bank shareholders and managers have incentives to take excessive risk, posing potential threats to the banking system (Jensen and Meckling, 1976; Merton, 1977; see Thakor, 2014, for a review). To reduce this negative externality, practitioners have advocated for a more stakeholder-oriented approach to bank governance in the current policy reforms (Laeven, 2013). Our paper examines these stability implications of stakeholder orientation in the US banking industry.

Our paper exploits the plausibly exogenous variation in stakeholder orientation arising from the staggered enactment of state constituency statutes, which expand directors' fiduciary duties to consider stakeholder and long-term interests, and estimates a DID model. We find a negative and significant effect of statute enactment on bank risk. This relationship is robust to reverse causality and controlling for state antitakeover laws, banking policy changes, and unobserved local economic conditions. Our channel tests suggest that after statute passage banks reduce risk through increasing capital and lending to safer borrowers. Further, we find that the effect of statute passage on bank performance is insignificant on average, but is significantly positive only for the high-risk banks. Finally, banks that previously received a statute passage fared significantly better during the financial crises than the matched banks.

Our results yield several policy implications. First, our finding supports the increasing call for greater emphasis on stakeholder value in the current banking regulatory and governance reforms (Macey and O'Hara, 2003, 2016; Laeven, 2013), focusing specifically on whether bank directors' fiduciary duties should owe exclusively to shareholders. Banks with directors whose duties are expanded to consider stakeholder and long-term interests are shown to reduce risk through altering capital structures and lending activities. Moreover, our results highlight the significant influence of directors on bank strategic decision making, implying that the imposition of additional requirements or responsibilities on bank directors, such as requiring specialized expertise in bank risk control (Macey and O'Hara, 2016), may be helpful in preventing future systemic instability.

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TABLE 1
The Enactment of Constituency Statutes

This table shows the staggered enactment of constituency statutes across US states over time and the number of treated BHCs for each statute. The total number of treated BHCs is 166 in 28 states.

State	Year	Number of treated BHCs
Ohio	1984	
Illinois	1985	
Maine	1986	
Arizona	1987	1
Minnesota	1987	1
New Mexico	1987	1
New York	1987	11
Wisconsin	1987	3
Idaho	1988	1
Louisiana	1988	3
Tennessee	1988	4
Virginia	1988	9
Florida	1989	5
Georgia	1989	7
Hawaii	1989	1
Indiana	1989	7
Iowa	1989	4
Kentucky	1989	5
Massachusetts	1989	7
Missouri	1989	5
New Jersey	1989	20
Oregon	1989	2
Mississippi	1990	4
Pennsylvania	1990	23
Rhode Island	1990	2
South Dakota	1990	
Wyoming	1990	
Nevada	1991	1
North Carolina	1993	11
North Dakota	1993	
Connecticut	1997	5
Vermont	1998	1
Maryland	1999	12
Texas	2006	10
Nebraska	2007	
Total	35 states	166 BHCs incorporated in 28 states

TABLE 2
Summary Statistics

This table presents the summary statistics. Our sample consists of 939 (9,248 bank-year observations) publicly traded US BHCs that filed the FR Y-9C reports with the Federal Reserve over 1986-2012. Bank fundamental information is obtained from the Bank Regulatory Database. Bank stock information is downloaded from CRSP. Other accounting information and firm attributes are downloaded from Compustat. The state GDP and population data are downloaded from the US Bureau of Economic Analysis (BEA). To reduce the effects of outliers, we winsorize all continuous variables at the 1st and 99th percentiles. The detailed definitions of the bank and state variables are provided in Table IA.1 of the online appendix.

Variable	Obs.	Mean	Std. Dev.	Min	25%	Median	75%	Max
Total risk	9,248	0.409	0.239	0.129	0.260	0.343	0.471	1.558
IVol	9,248	0.382	0.231	0.115	0.235	0.319	0.447	1.514
Z-score	9,248	0.298	0.165	-0.002	0.186	0.273	0.377	0.958
NPL/TA	7,495	0.0129	0.0157	0.0000	0.0027	0.0074	0.0165	0.0877
Deflated total assets (in mil.)	9,248	17,707	110,729	111	620	1,412	4,600	2,069,691
ROA	9,248	0.011	0.012	-0.043	0.008	0.013	0.018	0.032
Non-interest income/TA	9,248	0.012	0.010	0.001	0.006	0.010	0.014	0.063
Net interest income/TA	9,248	0.088	0.020	0.030	0.076	0.088	0.099	0.143
Non-core deposits/TA	9,248	0.126	0.082	0.016	0.067	0.106	0.161	0.435
Loans (in mil.)	9,248	8,527	50,200	17	369	836	2,531	993,000
Loans/TA	9,248	0.649	0.119	0.250	0.586	0.661	0.726	0.875
Market-to-book equity ratio	9,248	1.480	0.743	0.213	0.968	1.367	1.873	4.091
FREQ	9,248	0.002	0.004	0.000	0.001	0.001	0.003	0.120
Capital/TA	9,248	0.089	0.024	0.033	0.073	0.086	0.101	0.183
Capital/TL	9,248	0.143	0.055	0.050	0.110	0.133	0.163	0.412
Market beta	9,248	0.524	0.554	-0.494	0.104	0.364	0.863	2.115
Annual returns	9,203	0.141	0.375	-0.756	-0.089	0.112	0.341	1.364
State GDP growth	9,248	0.028	0.027	-0.284	0.013	0.026	0.043	0.159
Ln(State population)	9,248	15.861	0.874	13.188	15.396	15.901	16.336	17.455

TABLE 3
Constituency Statutes and Bank Risk-Taking

This table examines the effect of constituency statutes on bank risk using the Difference-In-Differences (DID) model of equation (3) that exploits the staggered enactment of constituency statutes across US states. The dependent variables are bank total risk (*Total risk*), idiosyncratic risk (*IVol*), and Z-score (*Z-score*). The main explanatory variable of interest is *Constituency Statute*, which is a treatment dummy variable taking on a value of one when a constituency statute is enacted in a state where a BHC is incorporated, and a value of zero otherwise. All other bank and state control variables are defined in Table IA.1 of the online appendix. Standard errors reported in parentheses are clustered at the state of incorporation. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Total risk			*IVol*			*Z-score*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Constituency Statute	-0.027**	-0.028***	-0.027**	-0.029***	-0.030***	-0.030***	0.037***	0.037***	0.046***
	(0.010)	(0.010)	(0.011)	(0.010)	(0.010)	(0.011)	(0.009)	(0.009)	(0.007)
Ln(TA)	-0.049***	-0.051***	-0.053***	-0.058***	-0.060***	-0.062***	0.032***	0.031***	0.030***
	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.010)	(0.007)	(0.007)	(0.008)
ROA	-6.408***	-6.378***	-6.304***	-6.380***	-6.359***	-6.304***	4.409***	4.398***	4.467***
	(0.380)	(0.379)	(0.350)	(0.385)	(0.381)	(0.352)	(0.212)	(0.214)	(0.182)
Non-interest income/TA	3.566***	3.555***	3.440***	3.203***	3.197***	3.117***	-0.072	-0.066	-0.003
	(0.683)	(0.675)	(0.651)	(0.648)	(0.639)	(0.601)	(0.392)	(0.393)	(0.389)
Non-core deposits/TA	0.063	0.063	0.062	0.098**	0.099**	0.099**	-0.151**	-0.150**	-0.147***
	(0.047)	(0.046)	(0.047)	(0.044)	(0.044)	(0.045)	(0.058)	(0.058)	(0.051)
Loans/TA	-0.017	-0.020	-0.050	-0.023	-0.025	-0.051	0.056	0.055	0.071**
	(0.051)	(0.051)	(0.045)	(0.050)	(0.050)	(0.045)	(0.036)	(0.036)	(0.035)
Ln(Market-to-book equity ratio)	-0.076***	-0.075***	-0.070***	-0.084***	-0.083***	-0.079***	-0.016*	-0.016*	-0.018**
	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)	(0.008)	(0.008)
FREQ	7.812***	7.770***	7.500***	5.094***	5.046***	4.797***	-0.531	-0.551	-0.396
	(0.977)	(0.998)	(1.018)	(0.853)	(0.869)	(0.881)	(0.497)	(0.500)	(0.469)
State GDP growth	—	-0.157**	-0.216**	—	-0.117	-0.178**	—	0.038	0.059
		(0.071)	(0.086)		(0.073)	(0.087)		(0.098)	(0.097)
Ln(State population)	—	0.054***	0.071***	—	0.055***	0.068**	—	0.014	-0.008
		(0.017)	(0.026)		(0.019)	(0.028)		(0.011)	(0.013)
Intercept	0.726***	-0.097	-0.346	0.802***	-0.044	-0.224	-0.002	-0.227	0.147
	(0.081)	(0.261)	(0.380)	(0.078)	(0.288)	(0.420)	(0.045)	(0.170)	(0.213)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region×Year FE			Yes			Yes			Yes
Obs.	9,248	9,248	9,248	9,248	9,248	9,248	9,248	9,248	9,248
Adj. R^2	0.596	0.596	0.605	0.548	0.549	0.558	0.406	0.406	0.417

TABLE 4
Robustness Tests

This table presents our robustness tests. The dependent variables are bank total risk (*Total risk*), idiosyncratic risk (*IVol*), and Z-score (*Z-score*). For brevity, we only report the estimated coefficients and cluster-robust (at the state of incorporation, if not stated otherwise) standard errors of *Constituency Statute*, which is a treatment dummy variable taking on a value of one when a constituency statue is enacted in a state where a BHC is incorporated, and a value of zero otherwise. The bank and state control variables and fixed effects are identical to those used in columns (3), (6), and (9) of Table 3. Row (1) clusters the standard errors at the bank level. Row (2) adjusts for serial correlation in the error terms using the two-step procedure of Bertrand, Duflo, and Mullainathan (2004). Row (3) controls for the state of incorporation fixed effects. Row (4) reports the results of weighted least squares (WLS) regressions with weights calculated as the inverse of the number of bank-year observations in each state of incorporation. Row (5) excludes states without statute enactment (excluded 16 states, down to 5,696 observations). Row (6) excludes BHCs that are incorporated in Delaware (down to 7,273 observations). Row (7) estimates the baseline models with no control variables, i.e., only region-year and bank fixed effects. Rows (8) to (14) control for the interaction between the bank controls and the year dummies in the regressions. Rows (15) and (16) restrict the sample to end in 1995 and 2005, respectively. Row (17) controls for banking competition, as measured by the concentration of loans within the states of location. Row (18) excludes 7 states in which there are discrepancies from legal studies about the enactment year of the constituency statutes. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

		Dependent variables:					
		Total risk		*IVol*		*Z-score*	
		Coef.	*S.E*	*Coef.*	*S.E*	*Coef.*	*S.E*
(1)	Clustered at the state of incorporation level	-0.027**	(0.014)	-0.030**	(0.013)	0.046***	(0.009)
(2)	Serial correlation in the error terms	-0.015*	(0.008)	-0.015**	(0.007)	0.020***	(0.006)
(3)	State of incorporation FE	-0.027	(0.016)	-0.032**	(0.015)	0.030***	(0.008)
(4)	Weighted Least Squares (WLS) regressions	-0.038**	(0.017)	-0.037**	(0.017)	0.051***	(0.011)
(5)	Exclude states without statute enactment (5,696 obs.)	-0.017	(0.011)	-0.022**	(0.010)	0.042***	(0.012)
(6)	Exclude DE-incorporated BHCs (7,273 obs.)	-0.018	(0.012)	-0.021*	(0.011)	0.048***	(0.010)
(7)	Only fixed effects and no control variables	-0.032**	(0.013)	-0.038***	(0.014)	0.052***	(0.009)
(8)	Control for *Ln(TA)×Year*	-0.021**	(0.010)	-0.025**	(0.010)	0.044***	(0.007)
(9)	Control for *ROA×Year*	-0.021**	(0.011)	-0.024**	(0.011)	0.046***	(0.007)
(10)	Control for *Non-interest income/TA×Year*	-0.030***	(0.010)	-0.033***	(0.010)	0.046***	(0.007)
(11)	Control for *Non-core deposits/TA×Year*	-0.029**	(0.012)	-0.032***	(0.011)	0.049***	(0.007)
(12)	Control for *Loans/TA×Year*	-0.024**	(0.011)	-0.027**	(0.011)	0.045***	(0.007)
(13)	Control for *Ln(Market-to-book equity ratio)×Year*	-0.021*	(0.011)	-0.024**	(0.011)	0.040***	(0.008)
(14)	Control for *FREQ×Year*	-0.025**	(0.011)	-0.028**	(0.011)	0.046***	(0.007)
(15)	Time window up to 1995 (3,058 obs.)	-0.037***	(0.013)	-0.039***	(0.013)	0.040***	(0.010)
(16)	Time window up to 2005 (7,025 obs.)	-0.033**	(0.012)	-0.035***	(0.013)	0.041***	(0.008)
(17)	Control for *H-index* and *H-index*^{2} (state of location)	-0.028**	(0.011)	-0.031***	(0.011)	0.046***	(0.007)
(18)	Exclude 7 states with discrepancies in the enactment years of constituency statutes (8,200 obs.)	-0.032**	(0.013)	-0.033**	(0.013)	0.046***	(0.008)

TABLE 5
Timing of the Adoption of Constituency Statutes and Pre-existing bank risk: The Duration Model

The model is a Weibul hazard model where the dependent variable is the log expected time to the enactment of constituency statutes. The sample covers the period from 1980 to 2007 and comprises 35 states that received a statute enactment after 1980. States are dropped from the sample once they received a statute enactment. *State-average total risk* (*State-average IVol*) [*State-average Z-score*] is the state-level averages of our sample BHCs' *Total risk* (*IVol*) [*Z-score*]. Control variables include State GDP growth (*State GDP growth*), natural logarithm of state population (*State ln(Population)*), union coverage rates from www.unionstats.com maintained by Professor Barry Hirsch, state unemployment rate (*Unemployment rate*), natural log of the maximum total potential benefit available under the state's unemployment insurance system (*Ln(Unemployment insurance)*) from the US Department of Labor, and a dummy equals one when the state governor belongs to the democratic party (*Democratic governor*) from Harvard dataverse maintained by Carl Klarner. Since accounting data for computing *Z-score* are only available from 1986 onwards, column (3) presents results based on the sample covering the period from 1986-2007. Region (four regions in total) dummy variables are included to account for unobserved heterogeneity across regions. Standard errors in parentheses are clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Sample	1980-2007	1980-2007	1986-2007
	(1)	(2)	(3)
State-average total risk	-0.107	—	—
	(1.375)		
State-average IVol	—	-0.051	—
		(1.326)	
State-average Z-score	—	—	2.448
			(1.687)
State GDP growth	-2.470	-2.394	-10.684
	(5.470)	(5.441)	(8.164)
State ln(Population)	0.185	0.187	0.193
	(0.264)	(0.265)	(0.248)
Union coverage rate	0.027	0.027	0.045
	(0.029)	(0.029)	(0.035)
Unemployment rate	2.691	2.745	4.646
	(12.950)	(12.952)	(15.978)
Ln(Unemployment insurance)	0.648	0.644	0.000
	(0.830)	(0.829)	(1.053)
Democratic governor	0.586	0.582	0.211
	(0.536)	(0.536)	(0.393)
Regional dummy	Yes	Yes	Yes
Observations	321	321	158

TABLE 6
Dynamic Treatment Models

This table examines the dynamic treatment effects of constituency statutes on bank risk. We regress our bank risk measures (*Total risk, IVol,* and *Z-score*) on four indicator variables—$Before^{-2\ or\ -1}$, $Current^{0}$, $After^{+1}$, and $After^{>=+2}$—to examine *when* the statute effects have occurred. $Before^{-2\ or\ -1}{}_{st}$ is a dummy equal to one if it is one or two years prior to the statute enactment, $Current^{0}{}_{st}$ is a dummy equal to one if it is the year of statute enactment, $After^{+1}{}_{st}$ is a dummy equal to one if it is one year after the statute enactment, and $After^{>=+2}{}_{st}$ is dummy equal to one if it is two or more years after the statute enactment. We include the same bank and state controls and fixed effects as in the baseline model and cluster standard errors at the state of incorporation. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Total risk	*IVol*	*Z-score*
	(1)	(2)	(3)
$Before^{-2\ or\ -1}$	-0.008	-0.011	0.014
	(0.007)	(0.007)	(0.012)
$Current^{0}$	-0.022	-0.028*	0.040***
	(0.015)	(0.015)	(0.012)
$After^{+1}$	-0.026*	-0.031*	0.043***
	(0.017)	(0.017)	(0.015)
$After^{>=+2}$	-0.037**	-0.042***	0.063***
	(0.014)	(0.014)	(0.013)
Ln(TA)	-0.053***	-0.062***	0.030***
	(0.010)	(0.010)	(0.008)
ROA	-6.307***	-6.307***	4.472***
	(0.347)	(0.350)	(0.181)
Non-interest income/TA	3.441***	3.117***	-0.004
	(0.651)	(0.600)	(0.388)
Non-core deposits/TA	0.061	0.097**	-0.146***
	(0.047)	(0.045)	(0.052)
Loans/TA	-0.049	-0.049	0.069*
	(0.045)	(0.045)	(0.035)
Ln(Market-to-book equity ratio)	-0.070***	-0.079***	-0.018**
	(0.010)	(0.010)	(0.008)
FREQ	7.509***	4.807***	-0.411
	(1.021)	(0.884)	(0.470)
State GDP growth	-0.213**	-0.174*	0.054
	(0.087)	(0.087)	(0.096)
Ln(State population)	0.073***	0.070**	-0.011
	(0.026)	(0.028)	(0.012)
Intercept	-0.376	-0.259	0.199
	(0.377)	(0.417)	(0.194)
Bank FE	Yes	Yes	Yes
Region×Year FE	Yes	Yes	Yes
Obs.	9,248	9,248	9,248
Adj.R^2	0.605	0.558	0.417

TABLE 7
Different Event Windows Surrounding Statute Adoptions

This table reports results of the bank risk regressions estimated on subsamples constructed based on different event windows surrounding the statute enactments. To construct these subsamples, for each statute enactment, we match each treated bank with a control bank that is closest in size in the pretreatment year. We obtain 166 matched pairs in total. 118 and 88 of these matched pairs have data within the [-2, +2] and [-3, +3] window surrounding each enactment. The observations at the treatment year are excluded. We then estimate the baseline regression with bank and region-year interacted fixed effects on the matched samples of these three windows. Columns (1) to (3) ((4) to (6)) [(7) to (9)] reports the results based on the [-3, +3] ([-2, +2]) [[-1, +1]] windows. Control variables are identical to those used in the baseline model and their summary statistics in each event year are reported in Table IA.7 of the online appendix. Standard errors are clustered at the state of incorporation level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Window	[-3, +3]			[-2, +2]			[-1, +1]		
	Total risk	*IVol*	*Z-score*	*Total risk*	*IVol*	*Z-score*	*Total risk*	*IVol*	*Z-score*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Constituency statutes	-0.110***	-0.110***	0.047***	-0.071***	-0.071***	0.040***	-0.066***	-0.065***	0.029*
	(0.019)	(0.018)	(0.016)	(0.014)	(0.014)	(0.011)	(0.016)	(0.016)	(0.016)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	1,056	1,056	1,056	1,064	1,064	1,064	592	592	592
Adj. R^2	0.597	0.575	0.474	0.578	0.559	0.428	0.593	0.586	0.446

TABLE 8
Controlling for Unobserved Local Economic Conditions

This table examines whether unobserved changes in local economic conditions are driving our results. We match each treated BHC with a control BHC that is located in a bordering state without statute enactment and closest in bank size (pre-treatment). For each matched pair of BHCs, we include the bank-years before and after the statute enactment to construct a matched sample. To increase the likelihood that the treated and control BHCs share similar local economic conditions, we first remove all matched pairs with distance more than 500 miles. We also restrict the distance between the treated and control BHCs to lie within 250 miles and 125 miles, which correspond to the 75th and 50th percentiles distance of the matched sample, respectively. The matching diagnostics are provided in Table IA.9 of the online appendix. On each distance-restricted subsample, we estimate the DID regression of equation (3). The dependent variables include bank total risk (*Total risk*), idiosyncratic risk (*IVol*), and Z-score (*Z-score*). The main explanatory variable of interest is *Constituency Statute*, which is a treatment dummy variable taking on a value of one when a constituency statue is enacted in a state where a BHC is incorporated, and a value of zero otherwise. All regressions include the same bank and state controls, and bank and region-year fixed effects as in the baseline model. Standard errors reported in parentheses are clustered at the state of incorporation. We only report the estimates for *Constituency Statute* for brevity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Within 500 miles

	Total risk	*IVol*	*Z-score*
	(1)	(2)	(3)
Constituency Statute	-0.084***	-0.079***	0.063**
	(0.024)	(0.024)	(0.025)
Obs.	520	520	520
Adj. R^2	0.718	0.713	0.367

Panel B: Within 250 miles

	Total risk	*IVol*	*Z-score*
	(1)	(2)	(3)
Constituency Statute	-0.079**	-0.073**	0.057*
	(0.030)	(0.029)	(0.030)
Obs.	396	396	396
Adj. R^2	0.730	0.727	0.413

Panel C: Within 125 miles

	Total risk	*IVol*	*Z-score*
	(1)	(2)	(3)
Constituency Statute	-0.110***	-0.101**	0.049
	(0.037)	(0.037)	(0.034)
Obs.	176	176	176
Adj. R^2	0.777	0.776	0.481

TABLE 9
Controlling for State Antitakeover Laws and Coincidental Banking Policy Changes

This table reports results after controlling for the state antitakeover laws and the coincidental banking policy changes. We collect information and control for three state antitakeover laws, namely the control share acquisition laws, business combination laws, and fair price laws from Bertrand and Mullainathan (2003). We construct an indicator variable for each of the three major antitakeover laws, which equals one after the law passage, and zero otherwise. We have identified 16 "contaminated" states in which the enactment of constituency statutes is either in the one year before, during the year of, or in the one year after an enactment of any one of the three major state antitakeover laws, affecting 66 BHCs in total. Panel A excludes all BHCs that are incorporated in these contaminated states and re-estimate the baseline bank risk models. Panel B jointly controls for the three state antitakeover laws in the baseline bank risk models. We also collect information and control for three coincidental banking policy changes, namely the interstate and intrastate banking deregulation policies, and the erection of out-of-state entry barriers for interstate branching activities (the Rice and Strahan index) from Amore, Schneider, and Žaldokas (2013), Jayaratne and Strahan (1996), and Rice and Strahan (2010). We construct two indicator variables, *Inter* and *Intra*, which equal zero before the interstate and intrastate banking deregulations, and one otherwise. The Rice and Strahan index (*RS index*) is constructed by adding one if a state has any one of the following provisions, (i) the minimum age of the target institution, (ii) de novo interstate branching, (iii) the acquisition of individual branches, and (iv) a state-wide deposit cap. We have identified 10 states in which the enactment of constituency statutes is either in the one year before, during the year of, or in the one year after a banking deregulation policy change, affecting 30 treated BHCs in total. Panel C excludes the BHCs that are incorporated and headquartered in these 10 states and re-estimate the baseline bank risk models. Panel D jointly controls for *Inter*, *Intra*, and *RS index* in the baseline bank risk models. All regressions in Panels A and D include the same bank and state controls, and bank and region-year fixed effects as in the baseline model, and cluster standard errors at the state of incorporation. More details about the antitakeover laws and banking policy changes can be found in Tables IA.10 and IA.11 of the online appendix. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Excluding states with contaminating state antitakeover laws (2,452 observations excluded; 66 treated BHCs removed)

	Total risk	*IVol*	*Z-score*
	(1)	(2)	(3)
Constituency Statutes	-0.022*	-0.026*	0.052***
	(0.013)	(0.013)	(0.013)
Obs.	6,796	6,796	6,796
Adj. R^2	0.619	0.571	0.413

Panel B: Controlling for the coincidental state antitakeover laws

	Total risk	*IVol*	*Z-score*
	(1)	(2)	(3)
Constituency Statutes	-0.030***	-0.033***	0.045***
	(0.011)	(0.011)	(0.008)
Control share acquisition law	0.013	0.010	0.018
	(0.015)	(0.016)	(0.014)
Business combination law	-0.007	-0.003	0.025
	(0.013)	(0.014)	(0.015)
Fair price law	0.019	0.017	-0.046
	(0.018)	(0.019)	(0.035)
Obs.	9,248	9,248	9,248
Adj. R^2	0.605	0.558	0.418

Panel C: Excluding states with coincidental banking policy changes (2,051 observations excluded;30 treated BHCs removed)

	Total risk	*IVol*	*Z-score*
	(1)	(2)	(3)
Constituency Statute	-0.029**	-0.032**	0.048***
	(0.013)	(0.013)	(0.008)
Obs.	7,197	7,197	7,197
Adj. R^2	0.595	0.557	0.429

Panel D: Controlling for Inter, Intra and RS index

	Total risk	*IVol*	*Z-score*
	(1)	(2)	(3)
Constituency Statute	-0.025**	-0.029**	0.045***
	(0.011)	(0.011)	(0.007)
RS index	0.005	0.004	-0.003
	(0.003)	(0.003)	(0.005)
Inter	-0.011	-0.016	0.010
	(0.020)	(0.020)	(0.017)
Intra	-0.014	-0.012	0.006
	(0.016)	(0.016)	(0.026)
Obs.	9,248	9,248	9,248
Adj. R^2	0.605	0.558	0.417

TABLE 10
Alternative Bank Risk and Instability Measures

This table examines the relationship between constituency statutes and four alternative bank risk measures, of which three of them could better capture the risk associated with bank defaults, bankruptcies, and failures. *IVol (Gandhi and Lustig)* is an alternative measure for bank-specific risk, which is the estimated (yearly) annualized residual volatilities from a daily Fama-French three factor model augmented with the daily changes in 10-year US Treasury benchmark bond yields and in the Moody's AAA corporate bond yield. *EDF* is a measure of expected default probability following the procedure by . *Tail beta* is a market-based measure of bank systemic risk exposure, which equals the probability of a sharp decline in a bank's stock price conditional on a crash in a broad value-weighted banking index (based on all banks in the CRSP database Gandhi and Lustig, 2015), estimated using extreme value analysis following Jonghe (2010). *Ln(1+No. of problem banks)* is the natural log of one plus the number of problem commercial banks in a state in a given year. Column (4) reports the estimation of a tobit model with state controls, and state and region-year fixed effects. Sample period for column (4) is from 1981 to 2011. The state controls for columns (1) to (3) include state GDP growth and natural log of population. State controls in column (4) further introduces union coverage rates from www.unionstats.com maintained by Professor Barry Hirsch, state unemployment rate (*Unemployment rate*), natural log of the maximum total potential benefit available under the state's unemployment insurance system (*Ln(Unemployment insurance)*) from the US Department of Labor, and a dummy equals one when the state governor belongs to the democratic party (*Democratic governor*) from Harvard dataverse maintained by Carl Klarner. Standard errors are clustered at the state of incorporation level for columns (1) to (3) and at the state level for column (4). ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Sample	Bank-year			State-year
	1986-2012			1981-2011
	Bank fixed effects model			Tobit model
Dependent variables	*IVol (Gandhi and Lustig)*	*EDF*	*Tail beta*	*Ln(1+No. of problem banks)*
	(1)	(2)	(3)	(4)
Constituency statutes	-0.028**	-0.018*	-0.033**	-0.284***
	(0.011)	(0.010)	(0.016)	(0.017)
Bank controls	Yes	Yes	Yes	
State controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	
State FE				Yes
Region×Year FE	Yes	Yes	Yes	Yes
Obs.	9,248	8,071	6,753	1,550
Adj. R^2	0.541	0.578	0.226	0.395

TABLE 11
Constituency Statutes and Bank Capital

This table examines the effects of the enactment of constituency statutes on bank capital. The dependent variables are bank capital-to-asset ratio (*Capital/TA*) and capital-to-loan ratio (*Capital/TL*). In columns (1) and (3), the main explanatory variable of interest is *Constituency Statute*, which is a treatment dummy variable taking on a value of one when a constituency statute is enacted in a state where a BHC is incorporated, and a value of zero otherwise. In columns (2) and (4), we examine the dynamic treatment effects using the decomposed treatment variables as in Table 6 as our main explanatory variables. The bank and state controls as well as fixed effects are identical to those used in the baseline model. We also control for the coincidental state antitakeover laws and the banking deregulation policy changes as defined in Table 9. Standard errors are clustered at the state of incorporation. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variables	*Capital/TA*		*Capital/TL*	
	(1)	(2)	(3)	(4)
Constituency Statute	0.0061***	—	0.0110***	—
	(0.0020)		(0.0040)	
$Before^{-2 \text{ or } -1}$	—	0.0011	—	0.0031
		(0.0021)		(0.0035)
$Current^{0}$	—	0.0025	—	0.0058
		(0.0019)		(0.0036)
$After^{+1}$	—	0.0043*	—	0.0083*
		(0.0024)		(0.0046)
$After^{>=+2}$	—	0.0094***	—	0.0173***
		(0.0023)		(0.0049)
Bank controls	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes
Control for state antitakeover laws	Yes	Yes	Yes	Yes
Control for banking deregulation	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Region×Year FE	Yes	Yes	Yes	Yes
Obs.	9,248	9,248	9,248	9,248
Adj. R^2	0.326	0.3284	0.465	0.467

TABLE 12
Constituency Statutes and Bank Lending Activities

This table examines the effect of the enactment of constituency statutes on bank lending activities. The dependent variables are bank log total loans (*Ln(Loans)*), the ratio of non-performing loans to total assets (*NPL/TA*), and the ratio of net interest income to total assets (*Net interest income/TA*). Given that data on BHC non-performing loans are only available from 1990 onwards, the analyses in columns (3) to (5) are based on a subsample covering 1990-2012. In columns (1), (3), (4), and (6), the main explanatory variable of interest is *Constituency Statute*, which is a treatment dummy variable taking on a value of one when a constituency statue is enacted in a state where a BHC is incorporated, and a value of zero otherwise. In column (4), we use a matched sample in which each treated BHC is matched to a control bank with the closest bank size and located in a bordering state without statute passage. The diagnostics for the matched sample are provided in Panel B of Table IA.9 of the online appendix. Columns (2), (5), and (7) report the results of the dynamic treatment models. The bank and state controls as well as fixed effects are identical to those used in the baseline model. We also control for the coincidental state antitakeover laws and the banking deregulation policy changes as defined in Table 9. Standard errors are clustered at the state of incorporation. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Sample	*Ln(Loans)*		*NPL/TA*			*Net interest income/TA*	
	Full		Full	Matched	Full	Full	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Constituency Statute	-0.022	—	-0.0021*	-0.0024**	—	-0.0027***	—
	(0.022)		(0.0013)	(0.0010)		(0.0009)	
Before$^{-2 \text{ or } -1}$	—	0.005	—	—	-0.0003	—	-0.0010
		(0.023)			(0.0006)		(0.0013)
Current^{0}	—	-0.011	—	—	-0.0038**	—	-0.0036**
		(0.029)			(0.0017)		(0.0017)
After^{+1}	—	-0.014	—	—	-0.0026*	—	-0.0031*
		(0.030)			(0.0015)		(0.0018)
After$^{>=+2}$	—	-0.023	—	—	-0.0016	—	-0.0033**
		(0.032)			(0.0018)		(0.0015)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for state antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for banking deregulation	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	9,248	9,248	7,495	124	7,495	9,248	9,248
Adj. R^2	0.950	0.950	0.650	0.582	0.650	0.386	0.386

TABLE 13
Constituency Statutes and Bank Performance

This table examines the effects of the enactment of constituency statutes on bank performance. The dependent variables include bank return on assets (*ROA*), annual stock returns (*Annual returns*), and log market-to-book equity ratio (*Ln(Market-to-book equity ratio)*). The main explanatory variable of interest is *Constituency Statute*, which is a treatment dummy variable taking on a value of one when a constituency statue is enacted in a state where a BHC is incorporated, and a value of zero otherwise. The bank and state controls as well as fixed effects are identical to those used in the baseline model [except *ROA* and *Ln*(*Market-to-book equity ratio*) that are excluded]. We also control for the coincidental state antitakeover laws and the banking deregulation policy changes as defined in Table 9. To capture a bank's propensity to take excessive risk, we first sort the banks into quintile portfolios annually according to their log total assets, and then divide the banks according to whether its *Total risk* is greater than the portfolio average *Total risk*. *High risk* is a dummy variable taking the value of one if a bank's *Total risk* is greater than that of its size-matched peers in a given year, and zero otherwise. Standard errors are clustered at the state of incorporation. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	ROA		*Annual returns*		*Ln(Market-to-book equity ratio)*	
	(1)	(2)	(3)	(4)	(5)	(6)
Constituency Statute	0.0011	0.0005	-0.013	-0.025	0.018	-0.000
	(0.0010)	(0.0011)	(0.022)	(0.021)	(0.043)	(0.044)
Constituency Statute×High risk	—	0.0013**	—	0.031***	—	0.041**
		(0.0006)		(0.010)		(0.016)
High risk	—	-0.0053***	—	-0.051***	—	-0.130***
		(0.0005)		(0.007)		(0.009)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes	Yes	Yes
Control for state antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes
Control for bank deregulation	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Region×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	9,248	9,248	9,203	9,203	9,248	9,248
Adj. R^2	0.325	0.351	0.455	0.457	0.570	0.581

TABLE 14
Constituency Statutes and Crisis Performance

This table reports the DID analysis examining whether BHCs that were previously treated fared significantly better than the control BHCs during the 1998 crisis and the 2007-09 crisis. Our sample for the 1998 crisis begins with all available BHCs (378 BHCs) over 1997-98. We include all BHCs that have been treated before 1998 (88 BHCs) as the treatment group. Similarly, for the 2007-09 crisis, we begin with all available BHCs (304 BHCs) over 2006-09 of which 42 of them have been treated before 2007. For both crises, we match each treated BHC with a control BHC that is incorporated in a state that has not passed or does not have a constituency statute before 1998 or 2007, is located in a bordering state, and is closest in size before crisis (i.e. in 1997 or in 2006). Following Fahlenbrach, Prilmeier and Stulz (2012), we measure bank crisis performance using buy-and-hold stock returns defined as follows:

$Crisis_{1998}\ RET_1$: From 3rd August 1998 to the date on which it attains the lowest stock price (before 31st December 1998).
$Crisis_{1998}\ RET_2$: From 3rd August 1998 to 30th September 1998.
$Crisis_{1998}\ RET_3$: From 3rd August 1998 to 30th October 1998.
$Crisis_{07\text{-}08}\ RET_1$: From 1st July 2007 to the date on which it attains the lowest stock price (before 31st December 2008).
$Crisis_{07\text{-}08}\ RET_2$: From 1st July 2007 to 31st December 2008.
$Crisis_{07\text{-}08}\ RET_3$: From 1st July 2007 to 31st December 2009.

If a bank was delisted during the 1997-98 or 2007-09 periods, we put the proceeds into a daily bank index until the end of 1998 or 2009, to avoid potential survivorship bias. Panel A compares the means of pre-treatment bank and state characteristics between the treatment and control groups and report the *t*-statistics. Panel B reports the before-after differences in bank stock returns, calculated as the crisis returns (*Crisis RET*) minus the precrisis bank annual stock returns (*Annual stocks*) for the treatment and control groups, as well as the DID estimates between the groups. Panel C reports the DID estimates after controlling for the precrisis bank and state control variables. Precrisis years are defined as 1997 for the 1998 crisis and 2006 for the 2007-09 crisis. *Previously Treated* is a treatment dummy variable equal one for banks that have received a statute enactment prior to the 1998 or 2007-09 crisis, and zero otherwise. The detailed definition of the control variables can be found in Table IA.1 of the online appendix. We report White heteroscedasticity-robust standard errors (in parentheses) in both Panels B and C. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Matching diagnostics for the 1998 crisis and 2007-09 crisis

Crisis	1998 crisis				2007-09 crisis			
	Treatment	Control	T-C	*t*-stat	Treatment	Control	T-C	*t*-stat
Ln(TA)	8.305	8.268	0.037	0.115	8.391	8.390	0.001	0.005
ROA	0.018	0.018	0.001	0.66	0.016	0.016	0.000	0.081
Non-interest income/TA	0.015	0.013	0.001	0.457	0.015	0.010	0.005**	2.023
Non-core deposits//TA	0.093	0.116	-0.023	-1.507	0.155	0.179	-0.023	-1.132
Loans/TA	0.630	0.652	-0.022	-0.981	0.668	0.707	-0.039	-1.130
FREQ	0.002	0.003	-0.001	-1.184	0.003	0.003	0.000	-0.234
Market beta	0.462	0.472	-0.010	-0.130	1.219	1.233	-0.014	-0.100
State GDP growth	0.054	0.052	0.002	0.710	0.030	0.027	0.003	0.550
Ln(Population)	15.825	15.696	0.128	0.770	16.006	15.557	0.448*	1.770

Panel B: DID estimates

Crisis	1998 crisis			2007-09 crisis		
	(Crisis - Precrisis) differences			(Crisis - Precrisis) differences		
	Treatment	Control	DiD (Treatment-control)	Treatment	Control	DiD (Treatment-control)
Crisis RET₁	-0.919***	-1.053***	0.133**	-0.580***	-0.791***	0.211**
	(0.026)	(0.045)	(0.052)	(0.038)	(0.081)	(0.089)
Crisis RET₂	-0.798***	-0.919***	0.121**	-0.360***	-0.617***	0.258**
	(0.028)	(0.048)	(0.055)	(0.053)	(0.094)	(0.107)
Crisis RET₃	-0.768***	-0.888***	0.120**	-0.514***	-0.756***	0.242**
	(0.029)	(0.044)	(0.052)	(0.058)	(0.089)	(0.106)

Panel C: Controlling for precrisis bank and state characteristics

	1998 crisis			2007-09 crisis		
	(Crisis - Precrisis) differences			(Crisis - Precrisis) differences		
Crisis definitions	*Crisis₁₉₉₈ RET₁*	*Crisis₁₉₉₈ RET₂*	*Crisis₁₉₉₈ RET₃*	*Crisis₀₇₋₀₈ RET₁*	*Crisis₀₇₋₀₈ RET₂*	*Crisis₀₇₋₀₈ RET₃*
	(1)	(2)	(3)	(4)	(5)	(6)
Previously Treated	0.097**	0.065	0.086*	0.138***	0.191***	0.338***
	(0.046)	(0.051)	(0.048)	(0.052)	(0.065)	(0.083)
***Precrisis control variables**:*						
Ln(TA)	0.082***	0.088***	0.091***	0.055*	0.069	0.110***
	(0.025)	(0.026)	(0.027)	(0.032)	(0.042)	(0.040)
ROA	-6.629*	-5.325	-5.962	15.807***	16.208**	17.812***
	(3.793)	(4.111)	(4.219)	(4.733)	(6.294)	(5.899)
Non-interest income/TA	-2.290	-1.487	-0.845	-5.578	-5.761	-7.786
	(2.051)	(2.179)	(2.201)	(3.519)	(3.931)	(5.897)
Non-core deposits/TA	-0.208	-0.340	-0.295	-1.369***	-1.619***	-0.568
	(0.390)	(0.388)	(0.398)	(0.447)	(0.604)	(0.580)
Loans/TA	-0.204	-0.290	-0.242	-1.231***	-1.551***	-1.748***
	(0.182)	(0.188)	(0.194)	(0.216)	(0.292)	(0.280)
FREQ	-37.876***	-35.303***	-20.041*	-40.139***	-55.113***	-64.502***
	(11.632)	(11.532)	(11.845)	(13.113)	(17.434)	(17.150)

Market beta	-0.198*	-0.246**	-0.267**	-0.029	0.078	-0.012
	(0.115)	(0.123)	(0.134)	(0.044)	(0.053)	(0.060)
State GDP growth	0.313	1.378	0.122	-1.062	-1.021	-2.191
	(1.843)	(1.934)	(1.962)	(1.033)	(1.387)	(1.815)
Ln(State population)	-0.049*	-0.063*	-0.041	0.006	-0.012	-0.016
	(0.028)	(0.032)	(0.029)	(0.041)	(0.053)	(0.050)
Precrisis Control share acquisition law	0.055	0.055	0.078	-0.072	-0.089	-0.014
	(0.051)	(0.051)	(0.054)	(0.060)	(0.074)	(0.095)
Precrisis Business combination law	0.022	0.004	0.018	-0.070	-0.177*	-0.139
	(0.066)	(0.068)	(0.070)	(0.072)	(0.105)	(0.104)
Precrisis Fair price law	-0.022	-0.004	-0.035	0.086	0.086	-0.148
	(0.051)	(0.054)	(0.054)	(0.064)	(0.075)	(0.121)
Precrisis RS index	-0.018	-0.018	-0.014	-0.011	-0.005	0.022
	(0.017)	(0.018)	(0.018)	(0.021)	(0.026)	(0.027)
Precrisis Intra	-0.144*	-0.139	-0.117	-0.341	-0.524	-0.541**
	(0.080)	(0.093)	(0.087)	(0.263)	(0.390)	(0.269)
Intercept	-0.335	-0.026	-0.413	0.173	0.956	0.599
	(0.529)	(0.580)	(0.560)	(0.697)	(0.907)	(0.919)
Obs.	176	176	176	84	84	84
Adj. R^2	0.189	0.173	0.140	0.539	0.556	0.519