Does Credit Supply Competition Affect Accounting Conservatism?

by

Wei Huang

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Graduate Supervisory Committee:

Yinghua Li, Chair
Shawn (Xiaochuan) Huang
Steve Kaplan

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ABSTRACT

This study investigates the relation between credit supply competition among banks and their clients' conditional accounting conservatism (i.e., asymmetric timely loss recognition). The Interstate Banking and Branching Efficiency Act (IBBEA) of 1994 permits banks and bank holding companies to expand their business across state lines, introducing a positive shock to credit supply competition in the banking industry. The increase in credit supply competition weakens banks' bargaining power in the negotiation process, which in turn may weaken their ability to demand conservative financial reporting from borrowers. Consistent with this prediction, results show that firms report less conservatively after the IBBEA is passed in their headquartered states. The effect of the IBBEA on conditional conservatism is particularly stronger for firms in states with a greater increase in competition among banks, firms whose operations are more concentrated in their headquarter states, firms with greater financial constraints, and firms subject to less external monitoring. Robustness tests confirm that the observed decline in conditional conservatism is causally related to the passage of IBBEA. Overall, this study highlights the impact of credit supply competition on financial reporting practices.

DEDICATION

To my dear mother and father, the best parents ever. Thank you for your unconditional love and support. I love you.

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1. Introduction

This study investigates whether credit supply competition among banks affects their clients' conditional conservatism in financial reporting. Numerous studies document that lenders make extensive use of borrowers' financial reporting information in debt contracting to reduce conflicts of interest between lenders and borrowers (Ahmed et al. 2002; Christensen and Nikolaev 2012; Watts 2003; Zhang 2008). For instance, Christensen and Nikolaev (2012) maintain that capital covenants and performance covenants that rely on borrowers' accounting information reduce agency costs and help lenders exert their monitoring over borrowers. Among all accounting information characteristics, asymmetrically timely recognition of bad news—namely, conditional conservatism—is one of the essential characteristics on which lenders focus. Because lenders only receive fixed payments when borrowers perform well, and may not get fully paid if borrowers go bankrupt, they place a greater emphasis on the lower bounds of borrowers' earnings distributions (Watts 2003). Conservative financial reporting not only can mitigate conflicts of interest between debtholders and shareholders over dividend policy (Ahmed et al. 2002) but also can trigger debt covenant violations more quickly (Zhang 2008), allowing lenders to intervene directly with borrowers and take protective actions. Therefore, lenders have a strong demand for borrowers' conservative financial reporting that recognize bad news more quickly than good news.

Since banks play an important role in shaping borrowers' financial reporting decisions (Gormley et al. 2012; Tan 2013; Watts 2003), significant changes in characteristics of the banking sector are likely to affect borrowers' financial reporting practices. The past century has seen the development and prosperity of the banking industry. One noteworthy change is that banks no longer focus on regional business only; they expand their business nationwide and compete with one another. The expansion of banking business is important because it meets the demand for credits as the economy grows. More importantly, the increase in banking competition lowers the costs of debt (Johnson and Rice 2008; Jayaratne and Strahan 1998; Rice and Strahan 2010).

The increase in competition in the banking industry affects banks' ability to demand for conservative reporting from their clients, because greater competition among banks weakens their bargaining power relative to their clients'. Focusing on the U.S. audit market, Casterella et al. (2004) argue that when competition among auditors increases after a deregulation, auditors have weaker bargaining power, which, in turn, incites a price war over clients. Similarly, when the availability of credits is expanded as the credit supply competition increases, clients' bargaining advantage over banks increases. In general, managers in client firms would prefer to deliver less conservative reports because conditional conservatism can reduce cumulative reported earnings, which in turn affects managers' compensation (Ahmed et al. 2002, Watts 2003). Due to concerns about losing potential and existing clients when credit supply increases, banks may be willing to relax the constraints imposed on borrowing firms as well as their demand for conservative financial reporting. Therefore, borrowing firms are likely to report less conservatively after the competition among banks increases.

To study changes in banking competition, I use a watershed event in the banking industry, the passage of the Interstate Banking and Branching Efficiency Act (IBBEA) of 1994, as a positive shock to credit supply competition. Prior to the passage of the IBBEA, the geographic scope of banks was greatly limited. Legislative constraints severely restricted interstate banking and branching. Although states started to pass laws that relaxed the restrictions on interstate banking on a reciprocal basis, interstate branching was still greatly restricted until the passage of the IBBEA in 1994. The IBBEA permitted full interstate branching in addition to full interstate banking (Rice and Strahan 2010). Thus, after the IBBEA was passed, expanding business across state borders became much more feasible for banks. Horvitz (1965) suggests that increasing the entry of new banks is one of the most effective ways to stimulate competition in the banking sector. Consistent with Horvitz (1965), prior studies document that both the number of bank branches and the credit supply competition among banks increase in each state in the post-IBBEA period (Johnson and Rice 2008; Rice and Strahan 2010). Such surges in credit supply competition have various consequences, including the structure of the banking market, firms' external financing, and banks' disclosure decisions (Burks et al. 2017; Cornaggia et al. 2015; Dick 2006; Rice and Strahan 2010; Zarutskie 2006).

To measure the change in conditional conservatism after the passage of the IBBEA, I extend the Basu (1997) model by incorporating a dummy variable to distinguish the pre- and post-IBBEA periods. As discussed in prior research (Armstrong et al. 2012; Bertrand et al. 2004), this model is an effective difference-in-differences design. Consistent with my prediction, I find that firms report less conservatively after their headquartered states adopt the IBBEA provisions.

I further investigate whether the effect of the IBBEA on conditional conservatism varies across states and firms. States can adopt part of the IBBEA provisions and enact barriers on interstate banking and branching by passing legislations. If states are more open to interstate banking and branching, the increase in credit supply competition among banks is likely to be greater. Therefore, I predict that the effect of increased banking competition on conditional conservatism is more pronounced in states that enact fewer restrictions on interstate banking and branching. The measure of the increase in competition among banks in each state after the passage of the IBBEA is based on a Branching Restrictiveness Index, formulated by Rice and Strahan (2010). I incorporate the Branching Restrictiveness Index into the Basu (1997) model and find that firms headquartered in states that impose fewer restrictions on interstate banking and branching report less conservatively than firms headquartered in states that impose more restrictions. In addition, some states enact more restrictive provisions than others (Johnson and Rice 2008). If states are more restrictive on interstate banking and branching, then banking competition in these states will be limited, and accordingly, firms headquartered in these states will exhibit a smaller decrease in conditional conservatism. Consistent with the prediction, I find that firms headquartered in states that enact less restrictive barriers exhibit a greater decline in conditional conservatism than firms in states that enact more restrictive barriers. I also find that the effect of the IBBEA on conditional conservatism is more pronounced for firms whose operations are more concentrated in their headquartered states, when clients are riskier, and for firms with lower analyst coverage.

To enhance the credibility of the results, I conduct robustness tests to provide further evidence that the change in conditional conservatism is causally related to the IBBEA. First, if the decline in conditional conservatism is simply a time-trend or is induced by other factors prior to the passage of the IBBEA, then the change in conditional conservatism is likely to precede the passage of the IBBEA. However, I find that the change in conditional conservatism is only observed after the IBBEA is adopted. The effect of the IBBEA on conditional conservatism lasts for two years after the IBBEA is passed. Second, I follow Burks et al. (2017) and conduct placebo tests using pseudo dates of the passage of the IBBEA in each state. The results show that there is no significant change in conditional conservatism in the pseudo post-IBBEA periods. Taken together, the results from the robustness tests support the argument that the passage of the IBBEA leads to changes in borrowing firms' conditional conservatism.

I conduct two additional tests to address the measurement issues with conditional conservatism in Basu (1997). First, I follow prior research (Collins et al. 2014; Jayaraman and Shivakumar 2013) and decompose earnings in the Basu (1997) model into accruals and cash flows. Jayaraman and Shivakumar (2013) and Collins et al. (2014) suggest that asymmetrically timely recognition of bad news should be reflected in accruals but not in cash flows. Collins et al. (2014) show that cash flow asymmetry is predicable based on firms' life-cycle characteristics. Therefore, using earnings as the dependent variable in the Basu (1997) measure for conditional conservatism may be biased by cash flow asymmetry. I find that the decline of conditional conservatism after the passage of the IBBEA only exists in accruals but not in cash flows, indicating that the main results of the study are not attributed to cash flow asymmetry. Second, I follow Khan and Watts (2009) and use the firm-year specific measure for conditional conservatism. The main results remain unchanged when I use the alternative measure for conditional conservatism.

This study makes several contributions to the extant literature. First, this study shows an unintended consequence of increased competition in the banking industry. Prior research has documented various benefits of banking competition, such as an increase in economic income (Besanko and Thakor 1992), a decrease in loan rates (Jayaratne and Strahan 1998), and better resource allocation (Gilje et al. 2016). However, only a handful of studies have investigated the effects of competition among banks on their clients' financial reporting practices. My study presents evidence that a competitive banking market can induce changes in borrowing firms' corporate financial reporting choices in such a way that losses are reported in a less timely fashion. Since firms' conditional conservatism is essential to constraining managers' opportunism, less conservative reporting will hurt not only debtholders but also shareholders (Watts 2003). As a result, share value is reduced (Watts 2003). My findings should be of interest to regulators. When the competition in banking industry increases, regulators should consider the unintended impacts on firms' financial reporting and intensify their monitoring to ensure that the quality of corporate financial reporting remains uncompromised.

Second, the findings in the study add evidence to the literature on determinants of conditional conservatism. Prior research has discussed the origins of firms' financial reporting choices, specifically conditional conservatism, from various perspectives, including debt contracting demands, agency conflicts, litigation concerns, national culture, and political aspects (Basu 1995; Kanagaretnam et al. 2014; LaFond and Watts 2008; Watts 2003; Watts and Zimmerman 1986). Several studies employ exogenous shocks to investigate the causal effect of lenders' demand on borrowers' conditional conservatism (Aier et al. 2014; Jayaraman and Shivakumar 2013). However, these studies focus on shocks that introduce changes in the characteristics of borrowers. There is scant evidence on how the characteristics of lenders affect the reporting practices of borrowers.

Gormley et al. (2012) conduct a closely related study in which they use the staggered entry of foreign banks into India as an exogenous regulatory change in its banking industry. The authors find that foreign bank entry is associated with a higher level of conditional conservatism. Because foreign banks know little about Indian firms when they enter the market, they demand conservative financial reporting from clients to reduce information asymmetry. However, they acknowledge that the findings may not be applicable to similar regulatory changes in developed countries such as the United States. Using a different exogenous regulatory change in the banking industry in the United States, this study complements Gormley et al. (2012) and provides new insights into the role of the banking sector in shaping corporate financial reporting.

The remainder of the paper is organized as follows: Section 2 summarizes the history of banking and branching deregulation and develops testable hypotheses. Section 3 describes the data and research design. Section 4 presents empirical results, and Section 5 concludes the paper.

2. Institutional Background and Hypothesis Development

2.1 Background on Banking and Branching Deregulation

Regulations related to interstate banking and branching have long been a controversial issue. Deregulation in the banking industry is a result of the rivalry between interested parties and economic growth (Rice and Strahan 2010). Small banks seek to shield themselves from competition with large banks, and consequently lobby political authorities against regulations that may increase competition in the local banking markets. Large banks, on the other hand, have incentives to expand their geographic scope, especially given the economic growth and the evolution of technology that makes remote operation and communication feasible. Consistent with the different incentives between small and large banks, Kroszner and Strahan (1999) find that deregulation in the banking industry took place more quickly in states with fewer small banks. Johnson and Rice (2008) document that the efforts and lobbies made by small banks greatly restricted branch banking by large banks.

The dual banking system—both the federal government and states being entitled to issue bank charters—prompts another conflict of interest. National banks are chartered by the federal government to control the monetary system and to conduct business for the federal government (Blair and Kushmeider 2006). States, however, receive chartering fees from state banks, but not from banks incorporated outside the state or national banks. In addition, because state governments purchase and own shares of local banks, states have incentives to prohibit the geographic expansion[1] of banks or to limit competition

[1] Johnson and Rice (2008), Page 75, document four means of geographic expansion: "(1) interstate banking (acquiring or establishing a charter in a state outside the main bank's home state); (2) interstate

among banks to enhance their revenue. For instance, the McFadden Act of 1927 dictated that national banks could only open branches in the city in which they were situated and that states had the right to prevent interstate branching. Arousing marked change, the Glass-Steagall Act of 1933 allowed national banks to branch in the same areas as state banks. However, states then restricted intrastate branching to prevent the expansion of national banks in the states. (Johnson and Rice 2008; Kroszner and Strahan 1999).

Despite the rivalry between interested parties, geographic expansion of banks and deregulation in the banking industry were inevitable—particularly due to the rapid economic growth at the time. Appendix A shows the timeline of banking and branching deregulation. In 1978, Maine was the first state to pass a law that relaxed the restrictions on acquisitions, but not branching, by out-of-state banks on a reciprocal basis. Over time, other states passed similar laws and joined the reciprocal agreements. By 1993, all states but Hawaii had permitted reciprocal interstate banking (Kroszner and Strahan 1999); however, interstate branching remained greatly restricted.

To meet the increased credit demand from corporations and individuals and to balance the benefits between state-chartered and federal-chartered banks, Congress passed the Interstate Banking and Branching Efficiency Act (IBBEA) in 1994. The passage of the IBBEA was a watershed event as it aimed to remove the final barriers to full interstate banking and permit full interstate branching. The IBBEA struck down the McFadden Act of 1927 and superseded the Douglas Amendment to the Bank Holding

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branching (acquiring or establishing a branch office, an office which is not separately chartered or capitalized, in a state outside the main bank's home state); (3) intrastate banking (acquiring or establishing a charter within the main bank's home state); and (4) intrastate branching (acquiring or establishing a branch office within the main bank's home state).”.

Company Act of 1956 by allowing interstate acquisitions of banks, interstate consolidation of subsidiaries, interstate branching, and interstate de novo branching.[2] States had four options as to whether and when to adopt the following provisions: (1) passing legislation before June 1, 1997, to "opt out" of interstate acquisitions of banks[3]; (2) passing legislation to adopt interstate consolidation of subsidiaries and interstate de novo branching, otherwise these two provisions would not be effective; (3) passing legislation to adopt more desirable provisions, such as acquiring single branches without having to acquire the whole bank to enter the market; and (4) adopting provisions earlier than June 1, 1997, or adopting additional provisions later on (Burks et al. 2017; Johnson and Rice 2008; Rice and Strahan 2010; Zarutskie 2006).

Given the strong restrictions on interstate banking and branching in previous legislations, the passage of the IBBEA created a positive shock to state-level credit supply competition. While certain reciprocal interstate banking was permitted prior to the IBBEA, those regulations were not as influential as the IBBEA for two reasons. First, as documented by Johnson and Rice (2008), the reciprocal regulations limited interstate banking to only a specific region, whereas the IBBEA had nationwide influence. Second, compared to interstate *banking*, interstate *branching* permitted by the IBBEA was less costly to implement and made the state-level credit markets more competitive. Interstate banking allowed banks to acquire out-of-state banks and convert subsidiaries into

[2] Johnson and Rice (2008), Page 86, define the provisions as follows. "Interstate bank acquisitions: acquisitions of separately chartered institutions. Interstate agency operations: allowing a bank subsidiary of a banking company to act as an agent of an affiliate of the banking company without being legally considered as a branch of that affiliate. Interstate branching: consolidation of acquired banks or individual branches into branches of the acquiring bank. De novo branching: establishment of a new branch office of a banking company across state lines, into states which have passed a statute expressly allowing it."

[3] Only Texas and Montana passed laws to "opt out" of interstate branching provisions prior to June 1, 1997. Eventually, Texas "opted in" in 1999, and Montana "opted in" in 2001.

branches, whereas interstate branching allowed banks to directly establish branches in areas with strong credit demand (Rice and Strahan 2010). Therefore, the entry of out-of-state banks was easier after states adopted the interstate branching provisions of the IBBEA.

2.2 Prior Literature and Hypothesis Development

2.2.1 Banks' Demand for Conditional Conservatism from Borrowing Firms

Prior literature documents that lenders are an effective monitor of their borrowers (Besanko and Kanatas 1993; Diamond 1984; Dichev and Skinner 2002; Nini et al. 2009; Nini et al. 2012). For instance, Dichev and Skinner (2002) document that debt covenants set by private lenders act as "trip wires" for borrowers. Tight covenants help private lenders closely scrutinize borrowers' performance. Nini et al. (2009) find that banks impose explicit restrictions on firms' capital expenditures in the covenants, which significantly reduce firms' investments.

Prior research shows that borrowers' financial reporting information plays an important role in lending decisions and shaping debt contracts by reducing conflicts of interest between lenders and borrowers (Ahmed et al. 2002; Christensen and Nikolaev 2012; Watts 2003; Zhang 2008). Christensen and Nikolaev (2012) show that accounting information required by debt covenants reduces agency costs and facilitates creditors' monitoring of borrowers. Specifically, capital covenants (C-covenants), using balance sheet information, restrict the debt-to-equity ratio in firms' capital structure to better align the interests between debtholders and shareholders. Performance covenants (P-covenants), relying on information from income statements or cash flow statements in addition to balance sheets, transfer control to lenders by providing them with an option to restrict managers' behavior when clients do not perform well. P-covenants tend to be more timely indicators of distress, and thus serve as better "tripwires" than C-covenants in controlling agency problems. In other words, timely indicators of failure revealed by borrowers' financial reports are an important and useful tool for lenders to mitigate agency concerns.

Because conservative financial reporting recognizes economic losses in a more timely fashion than gains, conditional conservatism helps trigger "tripwires" and facilitates transferring control rights to lenders when borrowers are close to financial distress (Christensen and Nikolaev 2012). Extant literature has discussed why lenders demand conservative financial reporting from borrowers (Ahmed et al. 2002; Basu 1997; Beatty et al. 2008; Nikolaev 2010; Watts 2003; Watts and Zimmerman 1986; Wittenberg-Moerman 2008; Zhang 2008). Ahmed et al. (2002) document that conditional conservatism mitigates the dividend policy conflicts between fixed and residual claimants. Because the excessive payment of dividends transfers bondholders' wealth to shareholders, restrictions on dividend policy are typically included in debt contracts. In this case, conservative reporting limits dividends paid to shareholders because conditional conservatism results in lower reported earnings upon which dividends are based. Thus, bondholders' wealth is protected. Watts (2003) also argues that lenders have a strong incentive to demand conservative financial reporting from borrowers. As suggested by agency theory, when borrowers perform well, lenders are not paid above their contracted sum. However, in the event of bankruptcy, due to limited liability, lenders may lose their investments if borrowers' net assets are not sufficient to cover the promised payments in the debt contracts. Because of this downside risk, lenders focus more on the left tail of the earnings distribution in debt contracting. Such lower bound measures help lenders monitor borrowers' ability to pay. In a similar vein, Zhang (2008) documents that conditional conservatism provides an early signal of default risk and thus trigger debt covenant violations more quickly. Such violations help reduce lenders' downside risk by allowing them to take protective actions.

2.2.2 The Economic Consequences of Banking Deregulation

Prior literature documents various economic effects of banking deregulation. Deregulation in the banking sector fosters competition among banks because it relaxes the entry barriers into other banking markets (Besanko and Thakor 1992; Black and Strahan 2002; Stiroh and Stranhan 2003). Theoretical work shows that as the competition in the banking system increases, the level of economic income increases, the severity of business cycles declines, the equilibrium loan interest rates decrease, and the equilibrium deposit interest rates increase (Besanko and Thakor 1992; Smith 1998). Empirical evidence supports the conclusions in theory. Jayaratne and Strahan (1996) present that the rate of real per capita growth in income increases following banking deregulation. Jayaratne and Strahan (1998) find that loan rates decrease after relaxing the restrictions on bank expansion. In addition, the increase in competition among banks after deregulation is related to the increase in the number of new incorporations, since banking competition stimulates innovation (Black and Strahan 2002).

Banking competition also improves resource allocation in the banking industry and bank performance. Jayaratne and Strahan (1998) note that banking deregulation allows banks to enter new markets, but only better-managed and lower-cost banks can expand their business. Less efficient banks fail to compete with better-performing banks.

Thus, the market share of more efficient banks increases because resources are reallocated to them. This transfer of resources is consistent with the disciplinary role of competition and is a clear benefit of banking deregulation (Stiroh and Strahan 2003). In addition, as the network of bank branches is enlarged after deregulation, the capital allocation among different areas is more efficient. Deposits in one area can be transferred to another area with high growth prospects (Gilje et al. 2016). The consolidation among banks permitted by banking deregulation improves operation efficiency due to scale economies (Struck and Mandell 1983). Also, banks in more active takeover markets improve their performance to maximize their value (Schranz 1993). As a result, CEOs of banks in these markets get higher rewards (Hubbard and Palia 1995).

As discussed in Section 2.1, state-level competition in the credit market significantly increases after the passage of the IBBEA, because of the entry of out-of-state banks. The increased competition induced by the IBBEA is consistent with the argument by Horvitz (1965) that one of the most effective ways to stimulate competition in the banking industry is an increase in the entry of new banks. Using the Summary of Deposits from Federal Deposit Insurance Corporation (FDIC), I calculate the average number of out-of-state branches per state in twelve years after the passage of the IBBEA. Appendix C shows that the average number of out-of-state branches per state increases from less than 38 in 1994 to 558 in 2005, and the average number of in-state branches decreases from 1,336 in 1994 to 1,045 in 2005.

Because the passage of the IBBEA is a watershed deregulation event in the banking industry, numerous studies have used the IBBEA as a positive shock to competition among banks and have investigated the effect of banking competition on the structure and quality of the banking market (Dick, 2006), borrowing and investment of private firms (Zarutskie 2006), small-firm finance (Rice and Strahan 2010), state-level innovation (Cornaggia et al. 2015), voluntary disclosure decisions of banks (Burks et al. 2017), and banks’ loan loss provisions (Dou et al. 2017). However, prior research offered little evidence of the impact of banking competition on borrowers’ decision making, particularly their financial reporting choices.

2.2.3 The Impact of the IBBEA on Borrowing Firms’ Conditional Conservatism

Since borrowers’ conditional conservatism arises from lenders’ demand, any changes in banks’ demand for conservative financial reporting is likely to directly impact clients’ conditional conservatism. For instance, Martin and Roychowdhury (2015) argue that when lenders, especially banks, invest in credit default swaps (CDS), they receive payoffs from the counter-party of the CDS contracts if a loan defaults. Thus, the risk of a loan can be estimated based on the credit rating of the contractor of the CDS instead of the original borrower. Therefore, banks’ monitoring and demand for conservative reporting from borrowers are diminished by the investment in CDS. They find that borrowers’ financial reporting is less conservative in the post-CDS period. Tan (2013) argues that due to the information asymmetry between creditors and borrowers, creditors have a strong demand for information to verify the actual state of nature to protect their claims following debt covenant violations. He finds that firms report more conservatively immediately after debt covenant violations.

Prior studies have employed different shocks that induce changes in conditional conservatism. Jayaraman and Shivakumar (2013) investigate changes in conditional conservatism among firms with greater debt-contracting demand after the passage of state antitakeover laws. Because antitakeover laws require acquirers to pay higher prices for targets' shares, the market for corporate control may not serve as an effective disciplining mechanism in influencing managers to act in shareholders' interests. As such, managers' power and entrenchment are likely to increase significantly, which aggravates agency conflicts between borrowers and lenders. Therefore, managers of firms with greater debt-contracting demand are likely to signal their commitment to not harm lenders after antitakeover laws become effective. The authors find that firms under greater debt-contracting pressure report more conservatively after the passage of antitakeover laws. Aier et al. (2014) establish a causal link between lenders' demand and borrowers' accounting conservatism using a setting where directors' fiduciary duties were expanded to include lenders. Specifically, in 1991 Delaware court ruled that directors should act in the interests of lenders when firms approach insolvency. Lenders could sue directors if the board of a near insolvent firm acted too much in the interests of shareholders. Thus, directors have greater legal obligations and are more likely to act in the interests of lenders. The authors argue that such expansion in directors' fiduciary duties induces an increase in conditional conservatism. Consistent with their argument, Aier et al. (2014) find that firms near insolvency report more conservatively after the Delaware court ruling.

However, the settings used by prior studies focus primarily on shocks to borrowers' characteristics instead of lenders' characteristics. There is scant evidence on how lenders' own characteristics affect their demand for conservative reporting. A noteworthy change among lenders, particularly banks, is the increase in competition in the market. The increase of credit supply competition in the banking industry is likely to affect banks' demand for conservative financial reporting. Specifically, banks' bargaining power relative to that of clients is weakened after the increase of banking competition. Although banks still prefer conditional conservatism, the changes in bargaining power affect banks' ability to demand conservative financial reporting from their clients. In the setting of the U.S. audit market, Casterella et al. (2004) document that the deregulation of the audit market in 1970s led to increased competition among accounting firms, which weakened auditors' bargaining power, induced price wars, and reduced audit industry profitability in the 1980s. In a similar vein, banks' clients will be in a stronger bargaining position when the credit supply competition is greater. When contracting with banks, managers of banks' clients are likely to be less compelled to meet the demand for conservative financial reporting from banks. From the supply side, managers have incentives to report less conservatively because conservative financial reporting reduces managers' private benefits and is costly to firms. Conditional conservatism reduces the income upon which managers' compensation and investors' evaluation are based (Ahmed et al. 2002; Watts 2003). Therefore, because managers possess relatively strong bargaining power in the negotiation process after a positive shock to credit supply competition, banks are likely to relax their demand for conservative reporting to retain clients in a more competitive banking market.

I follow extant research and use the IBBEA as a positive shock to state-level credit supply competition. Based on the above discussion, I state the first hypothesis in the alternative form as follows:

H1: Firms' financial reporting conservatism will decrease when the credit supply competition in the banking industry increases after the passage of the IBBEA in their headquartered states.

I expect that the effect of the IBBEA will vary across states. As discussed in Section 2.1, not all states fully adopted the provisions when the IBBEA became effective in the states. Furthermore, the IBBEA allowed states to enact up to four restrictions[4] on interstate banking and branching prior to June 1, 1997. The more restrictions a state enacts, the more difficult it is for out-of-state banks to enter the state, and consequently, the increase in credit supply competition will be suppressed. Moreover, Johnson and Rice (2008) conclude that Restrictions (3) and (4) are more restrictive than Restrictions (1) and (2). They find that Restrictions (1) and (2) do not significantly affect out-of-state branch growth. If branch growth is more limited in a state, the banking industry in the state will be less competitive. Therefore, I state the second hypothesis in the alternative form as follows:

H2: The negative relation between the passage of the IBBEA and conditional conservatism will be less pronounced in states with greater restrictions.

However, I may not find a negative relation between the credit supply competition among banks and borrowing firms' conditional conservatism. As Stiroh and Strahan (2003) suggest, deregulation in the banking industry increases competitive pressure for both strong and weak performers in the banking market. When banks

[4] Rice and Strahan (2010), Page 867, summarize the four restrictions as follows: (1) "a minimum age of 3 years or more on target institutions of interstate acquirers"; (2) "a state does not permit de novo interstate branching"; (3) "a state does not permit the acquisition of individual branches by an out-of-state bank"; and (4) "a state imposes a deposit cap less than 30%".

perform poorly and do not pass the market test, they have to exit the market. Resources will be transferred to better-performed banks (Jayaratne and Strahan 1998; Stiroh and Strahan 2003). Thus, to survive in a competitive market, banks are likely to screen their clients carefully to ensure that all payments can be collected from borrowers. In this case, banks are not likely to relax their demand for conservative financial reporting, since recognizing losses in a timely manner by borrowing firms helps trigger covenant violations early and allows banks to take protective actions (Zhang 2008). In other words, firms may experience an increase in conditional conservatism when the credit supply competition becomes greater.

3. Data and Research Design

3.1 Measuring Conditional Conservatism

To measure conditional conservatism, I follow Basu (1997) and use the following model:

$$NI_t = \beta_0 + \beta_1 NEG_t + \beta_2 RET_t + \beta_3 NEG_t * RET_t + \varepsilon_t \quad (1)$$

In Equation (1), *NI* is net income in year *t* deflated by the market value of common equity at the end of year *t-1*. *RET* is cumulative buy-and-hold returns from nine months before the fiscal year end to three months after the fiscal year end. *NEG* is an indicator variable that equals one if *RET* is negative (bad news), and zero otherwise (good news). The coefficient on *RET*, β_2, captures the sensitivity of earnings to good news, and β_3, the coefficient on $NEG_t * RET_t$, measures the incremental sensitivity of earnings to bad news than to good news, thus representing the level of conditional conservatism.

To investigate the effect of the IBBEA on conditional conservatism, I follow extant research (Armstrong et al. 2012; Bertrand et al. 2004; Bertrand and Mullainathan 2003; Huang et al. 2016) and extend Equation (1) by incorporating a dummy variable *POST* in the model as follows:

$$NI_t = \beta_0 + \beta_1 NEG_t + \beta_2 RET_t + \beta_3 NEG_t * RET_t + \beta_4 POST + \beta_5 POST * NEG_t$$
$$+ \beta_6 POST * RET_t + \beta_7 POST * NEG_t * RET_t + \gamma \sum Controls_{t-1} + \alpha_s + \alpha_t + \varepsilon_t \quad (2)$$

NI, RET, and *NEG* are defined as before. *POST* is an indicator variable that equals one if the state where the firm is headquartered has passed the IBBEA by year *t-1*, and zero otherwise. In Equation (2), β_7, the coefficient on $POST * NEG_t * RET_t$, represents the change in the incremental timeliness of bad news recognition in the post-IBBEA period. I posit that banks reduce their demands for conservative financial reporting from their clients in the presence of an increased competition among banks after the passage of the IBBEA. Therefore, β_7 is expected to be negative.

Following prior literature (Beaver and Ryan 2005; Givoly et al. 2007; Gormley et al. 2012; Khan and Watts 2009; LaFond and Watts 2008), I control for the demands for conditional conservatism by incorporating a set of firm characteristic *MTB*, *LEV*, *SIZE*, and *LITIG*. *MTB* is the market-to-book ratio, calculated as the market value of common equity deflated by the book value of common equity. *LEV* is leverage, calculated as the sum of long-term debt and current debt deflated by total assets. *SIZE* is the market value of common equity. *LITIG* is the litigation risk calculated from Equation (4) of Kim and Skinner (2012). Since the distribution of *LITIG* is between zero and one, and Table 2 shows that the distribution of *MTB*, *LEV*, and *SIZE* are skewed, I use the scaled decile ranks of *MTB*, *LEV*, and *SIZE* to estimate Equation (2) (LaFond and Roychowdhury 2008; Ahmed and Duellman 2013). Each control variable is interacted with *NEG*, *RET*, and *NEG* * *RET*. To control for time-invariant differences across states and years, the model also includes state fixed effects α_s, and year fixed effects α_t. As discussed in prior research (Bertrand et al. 2004; Armstrong et al. 2012), Equation (2) is an effective difference-in-differences design. The staggered passage of the IBBEA means that the treatment group includes firms headquartered in states that passed the IBBEA in year *t-1*, and the control group consists of firms headquartered in states that have passed the IBBEA before year *t-1* or will pass the IBBEA after year *t-1*. Standard errors are clustered at the state level because the passage of the IBBEA occurs at the state level (Cornaggia et al. 2015; Gormley et al. 2012; Petersen 2009).

3.2 Measuring the Increase of Competition among Banks

To measure the restrictiveness of interstate banking and branching across states, I follow Rice and Strahan (2010) and construct a variable *INDEX*. Rice and Strahan (2010) build a Branching Restrictiveness Index that ranges from zero to four. Specifically, they add one to the index if a state imposes one of the four restrictions. My *INDEX* variable is inversely related to the Branching Restrictiveness Index and ranges from zero to five. Specifically, *INDEX* equals zero if a state had not passed the IBBEA by year *t-1*, indicating that the state is the most restrictive on interstate banking and branching. *INDEX* equals one for states that have passed the IBBEA by year *t-1* but enacted all four restrictions. Then, I add one to *INDEX* if states relax one of the four restrictions. Thus, *INDEX* equals five for states that do not impose any restrictions in year *t-1*. *POST* in Equation (2) is replaced with *INDEX* as follows:

$$NI_t = \beta_0 + \beta_1 NEG_t + \beta_2 RET_t + \beta_3 NEG_t * RET_t + \beta_4 INDEX + \beta_5 INDEX * NEG_t + \beta_6 INDEX * RET_t + \beta_7 INDEX * NEG_t * RET_t + \gamma \sum Controls_{t-1} + \alpha_s + \alpha_t + \varepsilon_t \quad (3)$$

All variables are defined as in Equation (2). If the effect of the IBBEA on conditional conservatism is more pronounced in states more open to interstate banking and branching, the coefficient on the main interaction variable $INDEX * NEG_t * RET_t$, β_7, will be negative.

3.3 Sample Selection and Descriptive Statistics

Different states adopted the IBBEA at different times. Appendix B reports the effective dates of the IBBEA in all states obtained from Table 1 of Rice and Strahan (2010). Although states could adopt additional provisions after the IBBEA became effective, the effect of the IBBEA was greatest when the state first passed the Act. Thus, I only keep the first date when the state adopted the provisions of the IBBEA. Alaska was the first to adopt the provisions retrospectively on January 1^{st}, 1994. Twenty-seven states adopted provisions earlier than 1997. As required by the IBBEA, all states adopted at least the minimum provisions by June 1, 1997.

Table 1 details the sample selection process and the sample distribution. Sample firms with non-missing headquarter information are obtained from COMPUSTAT, and stock return data are obtained from CRSP. After excluding firms without necessary data for computing variables in Equation (2) and firms in the financial industry (SIC codes 6000 to 6999), the final sample consists of 17,924 firm-year observations (4,499 unique firms) during the sample period 1993 to 1998, as reported in Table 1, Panel A. The sample period starts one year prior to the first effective date of the IBBEA passage in Alaska and ends in 1998 since all states have passed the IBBEA by 1997.

Table 2 summarizes descriptive statistics of sample firms. Net income (*NI*) has a mean of 0.019 and a median of 0.142, indicating that, in general, sample firms make profits during the sample period. Returns (*RET*), on average, is also positive. The distributions of control variables *MTB*, *LEV*, and *SIZE* are similar. Panel B and Panel C report the descriptive statistics for sample firms in the pre-IBBEA period and the post-IBBEA period, respectively. In general, sample firms in the post-IBBEA period are less profitable and earn fewer returns than sample firms in the pre-IBBEA period.

4. Empirical Results

4.1 The Effect of the IBBEA on Conditional Conservatism (H1)

Hypothesis H1 posits that to attract new clients and retain existing clients, banks are likely to reduce their demand for conservative financial reporting from their clients in the presence of increased competition. Thus, firms headquartered in states where the IBBEA is adopted are expected to report less conservatively in the post-IBBEA period.

Table 3 presents the multivariate regression results for Hypothesis H1. Column (1) of Table 3 reports the basic estimation results of Equation (2) without control variables or fixed effects, Column (2) reports the estimation results with control variables, and Column (3) reports the estimation results of Equation (2) with control variables and fixed effects. The coefficient on the interaction variable $NEG_t * RET_t$, β_3, is positive and statistically significant at the 1% level in all three columns, indicating that sample firms recognize bad economic news in a more timely manner before the IBBEA is adopted in their headquarter state. More importantly, the coefficient on the main interaction variable $POST * NEG_t * RET_t$, β_7, is negative and significant in all three columns. The results are consistent with hypothesis H1 that firms report less conservatively after their states of headquarters adopts the IBBEA.

4.2 Interstate Banking and Branching Restrictions Enacted by States (H2)

In this section, I examine whether the effect of the IBBEA on conditional conservatism is uniform across states. Since the IBBEA allows states to enact restrictions on interstate banking and branching, it will be more difficult for out-of-state banks to enter the market if a state enacts restrictions. Competition among banks in states with fewer restrictions on interstate banking and branching will be greater than in states with more restrictions on interstate banking and branching. Therefore, I predict that the negative relation between the IBBEA and conditional conservatism will be more pronounced in states more open to interstate banking and branching.

Table 4 reports the branching restrictiveness among states and the multivariate regression results for Hypothesis 2. Panel A of Table 4 replicates the Branching Restrictiveness Index constructed by Rice and Strahan (2010). Branching Restrictiveness Index is added one if: (1) states require minimum age of institution for acquisitions; (2) states do not allow de novo Interstate Branching; (3) states do not allow interstate branching by acquisition of single branch or portions of institution; (4) states impose statewide deposit cap on branch acquisitions. Among all the states, 38 states enact Restriction (1), 38 states enact Restriction (2), 33 states enact Restriction (3), and 14 states enact Restriction (4). Panel B summarizes the Branching Restrictiveness Index. Ten states do not impose any banking and branching restrictions, while 12 states enact all four restrictions.

Although Restrictions (1) and (2) are more commonly enacted by states, Johnson and Rice (2008) conclude that Restrictions (3) and (4) are more restrictive. Thus, the banking industry in states that enact Restrictions (3) and (4) will be less competitive, and hence, firms headquartered in these states will exhibit a smaller decline in conditional conservatism after the IBBEA is passed. To empirically test the conjecture, I categorize the sample into three groups: (1) firms headquartered in states that enact both Restrictions (3) and (4); (2) firms headquartered in states that enact either Restriction (3) or (4) but not both; (3) firms headquartered in states that enact neither Restriction (3) nor (4). Panel B of Table 4 shows that most states enact either Restriction (3) or (4) but not both.

Panel C of Table 4 reports the multivariate regression results for Hypothesis 2. As in Table 3, Column (1) of Table 4 reports the baseline estimation results without control variables or fixed effects, Column (2) reports the estimation results after including control variables, and Column (3) reports the estimation results with both control variables and fixed effects. As predicted, the coefficient on $INDEX * NEG_t * RET_t$, β_7, is negative and statistically significant at the 1% level in all three columns. The results indicate that firms headquartered in states more open to interstate banking and branching report less conditionally conservatively after the IBBEA is passed than firms headquartered in states with restrictions on interstate banking and branching. In other words, the effect of the IBBEA on conditional conservatism is more pronounced in states that impose fewer interstate banking and branching restrictions.

Panel D of Table 4 reports the multivariate regression results conditional on the restrictiveness of provisions. Column (1), (2), and (3) report the results of group (1), (2), and (3) respectively. The coefficient on $POST * NEG_t * RET_t$, β_7, is insignificant in Column (1) and is negative and statistically significant at 1% level in Column (2) (=-0.112, t-value=-4.22) and column (3) (=-0.105, t-value=-4.09). The results indicate that firms headquartered in states that enact both Restrictions (3) and (4) do not change the level of conditional conservatism in their financial reporting after the IBBEA is passed. Firms headquartered in states that relax Restrictions (3) and (4) report less conservatively after the IBBEA is passed. The comparison tests show that the difference between the coefficients in Columns (1) and (2), and Columns (1) and (3), are statistically significant. Taken together, the results in Table 4 are consistent with the conjecture that firms headquartered in states with more competitive banking industry report less conservatively.

4.3 Geographic Concentration of Firms' Operations

Thus far, I have shown that firms headquartered in states where the IBBEA is adopted report less conservatively in the post-IBBEA period and the effect of the IBBEA on conditional conservatism is more pronounced in states more open to interstate banking and branching. In this section, I explore the cross-sectional variation of the effect of the IBBEA conditional on the geographic concentration of firms' operations in their headquartered states. If a firm's operations are more concentrated in their headquartered state, it is more likely that the firm's financial reporting choices are largely influenced by banks in its headquartered state rather than out-of-state banks. Therefore, when the competition among banks increases in the firm's headquartered state, the effect of competition on firms' financial reporting would be more pronounced for firms whose operation is more concentrated in the state.

Table 5 reports the multivariate regression results for the test. The geographic concentration of firms' operations is measured based on Garcia and Norli (2012). The authors count state names from firms' 10-K annual reports as a proxy for geographic dispersion of business operations. I sort firms within each state into tercile portfolios based on their concentration of operations in the headquartered states in the year prior to the passage of the IBBEA, and estimate Equation (2) for the bottom (Column 1) and top (Column 2) groups separately. The coefficient on $POST * NEG_t * RET_t$ in Column (1) is negative and insignificant (=-0.043, t-value=-0.77), indicating that firms with less business concentration in headquartered states do not exhibit a significant change in conditional conservatism after the IBBEA is passed. The coefficient in Column (2) is negative and significant (=-0.146, t-value=-4.55), indicating that firms operating more in headquartered states report less conservatively in the post-IBBEA period. The difference between the two coefficients is statistically significant (p-value=0.08). The results in Table 5 are consistent with the prediction that the change in conditional conservatism in the post-IBBEA period is more pronounced for firms whose operations are more concentrated in their headquartered states.

4.4 Banks' Downside Risk

In this section, I investigate whether the downside risk banks face plays a role in the relation between the IBBEA and conditional conservatism. Because of the downside risk, banks demand conservative financial reporting from their clients (Watts 2003). Banks' demand for conservative financial reporting is particularly high for firms more dependent on banks for their external financing, since banks are exposed to higher downside risk if their clients have a high level of debts in place. On one hand, when banks face greater downside risk, it is more costly for them to relax their demand for conditional conservatism from borrowing firms. Therefore, banks may still require conservative financial reporting from clients more dependent on them, even if the credit supply competition increases and banks' bargaining power is weakened. In this case, the effect of the IBBEA on conditional conservatism will be more pronounced for less financially constrained firms. On the other hand, since banks' demands for conservative reporting are higher for more financially constrained firms, if banks relax their demands after the credit supply competition increases, the effect would be more obvious for firms whose banks face higher downside risk. Firms less dependent on banks are not subject to as many demands for conditional conservatism in the first place, so the effect of relaxing demands for conservative financial reporting will not be pronounced among those firms.

Table 6 reports the multivariate regression results for the cross-sectional test. I employ two measures for banks' downside risk: *Leverage* and *KZ-Index*. *Leverage* is the leverage ratio measured as the sum of long-term debt and current debt deflated by total assets. *KZ-Index* is the financial constraint index computed following Kaplan and Zingales (1997) and Lamont et al. (2001)[5]. Firms with a higher leverage ratio or higher KZ-Index are more likely to be dependent on banks. I sort firms within each state into tercile portfolios based on their dependence on banks in the year prior to the passage of the IBBEA, and estimate Equation (2) for the bottom and top groups separately. Columns (1) and (2) report the results using *Leverage* as the partitioning variable. The coefficient on $POST * NEG_t * RET_t$ in Column (1) is negative and insignificant (=-0.027, t-value=-0.94), indicating that the change in conditional conservatism in the post-IBBEA period is not significant for firms with a lower leverage ratio. In Column (2) the coefficient on $POST * NEG_t * RET_t$ is negative and significant (=-0.180, t-value=-4.06), indicating that firms with a higher leverage ratio report less conservatively after the passage of the IBBEA. The comparison test reveals that the difference between the coefficients on $POST * NEG_t * RET_t$ in Columns (1) and (2) is statistically significant (p-value=0.00).

Columns (3) and (4) present the results using *KZ-Index* as the measure for firms' dependence on banks. Similarly, the coefficient on $POST * NEG_t * RET_t$ in Column (3) is

[5] The equation used to calculate the KZ-Index is: $KZ\text{-}Index = -1.002\ CF_{it} / AT_{it-1} - 39.368\ DIV_{it} / AT_{it-1} - 1.315\ CASH_{it} / AT_{it-1} + 3.139\ LEV_{it}$, where CF_{it} is cash flow deflated by lagged assets AT_{it-1}, DIV_{it} is cash dividend deflated by lagged assets AT_{it-1}, $CASH_{it}$ is cash balance deflated by lagged assets AT_{it-1}, and LEV_{it} is the leverage ratio.

negative and insignificant (=-0.027, t-value=-0.94), indicating that firms with lower KZ-Index do not exhibit changes in conditional conservatism in the post-IBBEA period. The coefficient on $POST * NEG_t * RET_t$ in Column (4) is negative and significant (=-0.131, t-value=-2.45), meaning that firms with a higher KZ-Index report less conservatively after the IBBEA is passed in the headquartered states. As shown by the comparison test, the difference between the two coefficients is statistically significant (p-value=0.01). Taken together, the results in Table 6 are consistent with the prediction that the effect of the IBBEA on conditional conservatism is more pronounced for firms with which banks face higher downside risk.

4.5 The Moderating Effect of External Monitoring Mechanisms

In this section, I examine the moderating effect of external monitors on the relation between the IBBEA and conditional conservatism. Prior research documents that the level of conditional conservatism is affected by existing monitoring mechanisms. Specifically, firms with stronger monitoring mechanisms in place tend to be more conditionally conservative than firms subject to weaker monitoring mechanisms (Shi and You 2016; Ramalingegowda and Yu 2012). It is possible that the effect of the IBBEA on conditional conservatism is subject to the impact of existing monitoring mechanisms on conditional conservatism. Following prior research, I use two proxies for monitoring mechanisms: analyst following and dedicated institutional ownership. Shi and You (2016) documents that firms report more conservatively after an exogenous drop in analyst coverage. Thus, firms with a greater analyst following are likely to report more conservatively than firms with a lesser analyst following. With greater analyst following in place, firms may not change the level of conditional conservatism even if banks relax the demand for conservative reporting. Ramalingegowda and Yu (2012) find that firms with higher monitoring institutional ownership report more conservatively. Therefore, I predict that the effect of the IBBEA on conditional conservatism is more pronounced for firms with a lesser analyst following or lower institutional ownership.

Table 7 reports the multivariate regression results for the test. Analyst coverage data are obtained from the I/B/E/S. Analyst following of a firm is calculated as the natural logarithm of one plus the number of analysts who issue earnings forecasts for the firm in the year prior to the IBBEA. For firms without analyst coverage data, analyst following is set to zero. I follow Bushee (2001) and Ramalingegowda and Yu (2012) and calculate the percentage of ownership by dedicated institutional investors. Institutional ownership data are from Thomson Reuters Stock Ownership and the classification of dedicated institutions is provided by Bushee (2001)[6]. Like prior tests, I sort sample firms into tercile portfolios based on their analyst following or dedicated institutional ownership, and estimate Equation (2) for the bottom and top groups separately. The negative and significant coefficient on $POST * NEG_t * RET_t$ (-0.092, t-value=-2.15) in Column (1) indicates that firms with a lesser analyst following report less conservatively in the post-IBBEA period. Consistent with the prediction, firms with a greater analyst following do not change the level of conditional conservatism (0.005, t-value=0.15). The comparison test shows that the difference between the coefficient on $POST * NEG_t * RET_t$ between Column (1) and Column (2) is statistically significant at 10% level. The coefficient on $POST * NEG_t * RET_t$ in Column (3) is negative and significant (=-0.086, t-value=-2.79), indicating that firms with low dedicated institutional ownership report less

[6] http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html

conservatively after the IBBEA is passed. The coefficient on *POST* * NEG_t * RET_t in Column (4) is insignificant. However, the difference between the two coefficients is not statistically significant (p=0.18). The results indicate that dedicated institutional ownership does not have a significant moderating effect on the relation between the IBBEA and conditional conservatism. Taken together, the results in Table 7 support the argument that the effect of the IBBEA on conditional conservatism is more pronounced for firms with lower analyst coverage.

4.6 Robustness Checks

4.6.1 Dynamic Effects of the IBBEA

In this section, I examine the dynamic effects of the IBBEA on conditional conservatism. If the decline in conditional conservatism is simply a time-trend effect that occurs before the passage of the IBBEA, or is induced by other factors before the IBBEA is passed rather than the increase in credit supply competition after the passage of the IBBEA, then the decline in conditional conservatism is likely to be observed prior to the passage of the IBBEA. Following Bertrand and Mullainathan (2003) and Armstrong et al. (2012), I construct indicator variables *IBBEA(-n)* for firms in the states that will adopt the IBBEA in N (N=0, 1, 2, and 3) years and *IBBEA(n)* for firms in the states that have adopted IBBEA for N years. *POST* in Equation (2) is replaced with *IBBEA(-n)* and *IBBEA(n)*. If the decline in conditional conservatism is a time-trend effect or is related to factors other than the passage of the IBBEA, then the effects of the IBBEA on conditional conservatism should precede the passage of the IBBEA. Alternatively, if the passage of the IBBEA leads to less conservative reporting by borrowing firms, then the effects of the IBBEA on conditional conservatism should only be observed after the IBBEA is adopted. In other words, if the passage of the IBBEA indeed causes the reduction in conditional conservatism, then only the coefficients on *IBBEA(n)* (n>0) should be negative and statistically significant.

Panel A of Table 8 presents the multivariate regression results for this test. For brevity, I only report the coefficients of the three-way interaction variables. Only the coefficients on $IBBEA(1) * NEG_t * RET_t$ and $IBBEA(2) * NEG_t * RET_t$ are negative and significant at the 10% level, indicating that firms report less conservatively only after the passage of the IBBEA. Moreover, the negative and insignificant coefficient on $IBBEA(3) * NEG_t * RET_t$ indicates that the IBBEA no longer has an effect on conditional conservatism after two years of its adoption. This result reveals that banks tend to relax their requirement for conservative financial reporting only during the first two years when they want to expand their business and attract clients in a competitive market.

4.6.2 Placebo Tests

In the above section, I have shown that the change in conditional conservatism is causally related to the passage of the IBBEA. To enhance the credibility of the results, I conduct placebo tests following Burks et al. (2017). In this test, I re-estimate Equation (2) assuming that the pseudo year of adopting the IBBEA in each state is N (N=4, 3, and 2) years earlier than the true date of adoption. Since there is no large shock to competition among banks N years before the IBBEA, I predict that the coefficient on the main interaction variable $POST * NEG_t * RET_t$ in the placebo tests should be insignificant. The results in Panel B of Table 8 are consistent with the prediction. In all three columns, the coefficients on $POST * NEG_t * RET_t$ are insignificant, indicating that there is no significant change in conditional conservatism in the pseudo post-IBBEA periods. The results support the argument that the relation between the IBBEA and conditional conservatism is not spurious.

4.6.3 Historical States of Headquarters

In this section, I check whether the main results remain unchanged using historical headquarter information, since COMPUSTAT only reports current information of firms' headquartered states. Historical headquartered state information is parsed by Bill McDonald from 10-Ks on Securities and Exchange Commission (SEC) Electronic Data Gathering, Analysis, and Retrieval system (EDGAR)[7]. Panel C of Table 8 reports the results using historical headquarter information. Column (1) of Panel C reports the estimation results without control variables and fixed effects, Column (2) reports the results with control variables, and Column (3) reports the results with both control variables and fixed effects. The coefficients on $POST * NEG_t * RET_t$ is negative and statistically significant in all three columns, indicating that firms report less conservatively after their states of headquarters adopt the IBBEA. The results in Panel C of Table 8 are similar to the main results in Table 3.

4.7 Measurement Issues

4.7.1 Cash Flow Asymmetry

In this section, I examine the measurement issues of conditional conservatism. First, I address the concern with cash flow asymmetry. Collins et al. (2014) suggest researchers use accrual-based measures to test conditional conservatism. They argue that earnings are composed of accruals and operating cash flow, and the asymmetric recognition of good and bad news is not reflected in the recognition of cash flow.

[7] https://sraf.nd.edu/data/augmented-10-x-header-data/

Therefore, when using earnings as the dependent variable to measure conditional conservatism, results can be biased by the noise induced by cash flow asymmetry. Specifically, Collins et al. (2014) show that cash flow asymmetry varies systematically with predictable firm characteristics in different life-cycle stages. Firms in their early life-cycle stages exhibit greater cash flow asymmetry. Removing the cash flow component from earnings when measuring conditional conservatism effectively mitigate many biases documented in previous research, such as Givoly et al. (2007) and Patatoukas and Thomas (2011). Following Collins et al. (2014), I decompose NI_t in Equation (3) into accruals (ACC_t) and operating cash flow (CFO_t). CFO_t is obtained from the statement of cash flows and ACC_t is calculated as the difference between NI_t and CFO_t. Then, I estimate Equation (3) using ACC_t and CFO_t as the dependent variables, respectively.

Panel A of Table 9 reports the multivariate regression results for the test. The dependent variables are ACC_t and CFO_t in Columns (1) and (2), respectively. The coefficient on $POST * NEG_t * RET_t$ is negative and statistically significant in Column (1) (=-0.075, t-value=-2.39), while the coefficient on $POST * NEG_t * RET_t$ is insignificant in Column (2). The results indicate that the change in conservatism is only observed in accruals but not in operating cash flow. Therefore, the results reported in this study using augmented Basu (1997) measure for conditional conservatism are unlikely to be biased by cash flow asymmetry.

4.7.2 Firm-Specific Measure of Conditional Conservatism

Second, I use an alternative measure of conditional conservatism suggested by Khan and Watts (2009). Khan and Watts (2009) develop a firm-specific measure of conditional conservatism, *C-SCORE,* by substituting *MTB, LEV,* and *SIZE* into Basu

(1997) estimation regression. Following Khan and Watts (2009), I calculate the *C-SCORE* for each firm-year observation and use it as the dependent variable and estimate the following model:

$$C\text{-}SCORE_t = \beta_0 + \beta_1 POST + \beta_2 LITIG_{t-1} + \alpha_s + \varepsilon_t \quad (4)$$

Variable definitions in Equation (4) are the same as in Equation (2). *LITIG* is the only control variable because *MTB*, *LEV*, and *SIZE* are used to calculate the *C-SCORE*. I exclude year-fixed effects in the regression because Panel D of Table 1 shows that the distribution post-IBBEA sample is concentrated in year 1996 to 1998. Specifically, half of the observations in the post-IBBEA sample are in the year of 1998. Therefore, including year-fixed effects in Equation (4) is likely to absorb most of the main effect. The coefficient on *POST*, β_1, captures the change in conditional conservatism after the IBBEA is passed.

Panel B of Table 9 presents the results of estimating Equation (4). Column (1) reports the estimation results of Equation (4) without control variables or fixed effects, Column (2) reports the estimation results with the control variable *LITIG*, and Column (3) reports the estimation results of Equation (4) with the control variable *LITIG* and state-fixed effects. The coefficient on *POST*, β_1, is negative and statistically significant at the 1% level in all three columns, indicating that sample firms recognize bad economic news in a less timely manner after the IBBEA is adopted in their headquartered state.

To control for systematic variation in conditional conservatism over time while mitigating the multicollinearity problem, I replace *POST* in Equation (4) with *INDEX*, add year fixed effects, and estimate the following model:

$$C\text{-}SCORE_t = \beta_0 + \beta_1 INDEX + \beta_2 LITIG_{t-1} + \alpha_s + \alpha_t + \varepsilon_t \quad (5)$$

The variable definition of *INDEX* is the same as in Equation (3). The coefficient on *INDEX*, β_1, represents the change in conditional conservatism after the IBBEA is passed in states with different restrictiveness.

Panel C of Table 9 presents the results of estimating Equation (5). Column (1) reports the estimation results of Equation (5) without control variables or fixed effects, Column (2) reports the estimation results with the control variable *LITIG*, and Column (3) reports the estimation results of Equation (5) with the control variable *LITIG* and fixed effects. The coefficient on *POST*, β_1, is negative and statistically significant at the 1% level in all three columns, indicating that firms report less conservatively after the IBBEA is adopted in their headquartered state, and that firms headquartered in more open states exhibit greater decrease in conditional conservatism after than firms headquartered in less open states. Taken together, the results in Panel B and Panel C of Table 9 show that the main results remain unchanged using *C-SCORE* as the alternative measure of conditional conservatism.

5. Conclusions

Using the staggered passage of the IBBEA as a positive shock to state-level credit supply competition among banks in the U.S., this study investigates the impact of credit supply competition in the banking industry on accounting conservatism in clients' financial reporting. The IBBEA permitted full interstate banking and branching that had been greatly restricted in history. Banks can expand their business across state borders by acquisitions or de novo branching after a state adopts the IBBEA provisions. With new banks entering the market, state-level competition among banks increases significantly and the availability of credit expands.

Prior studies document that information from clients' financial statements is essential to lenders when making lending decisions. The contracting demand from banks also affects their clients' financial reporting practices. Facing significant downside risk, banks require their clients to recognize bad news in a more timely manner. In other words, banks demand conservative financial reporting from their clients.

However, an increase in competition on the credit supply market weakens their bargaining power in the negotiation process. As such, banks are likely to relax their requirement of conservative reporting. Therefore, I predict that firms are likely to report less conservatively after the IBBEA is passed in their headquartered states. Consistent with the prediction, this study documents a negative relation between the passage of the IBBEA and conditional conservatism. The relation is more pronounced in states more open to interstate banking and branching. The decreases in conditional conservatism are concentrated among firms headquartered in states that impose more restrictions on interstate banking and branching, firms that have more operations in their headquartered states, firms with which banks face higher downside risk, and firms with less analyst following.

The findings of the study complement the findings in Gormley et al. (2012) and contribute to the literature on the determinants of conditional conservatism. To the extent that the increase in competition in the banking industry may have negative impacts on firms' financial reporting, the study is particularly of interest to regulators. The findings highlight the necessity of evaluating potential negative effects of regulatory changes in the banking industry and intensifying monitoring of banks and their clients after the IBBEA becomes effective.

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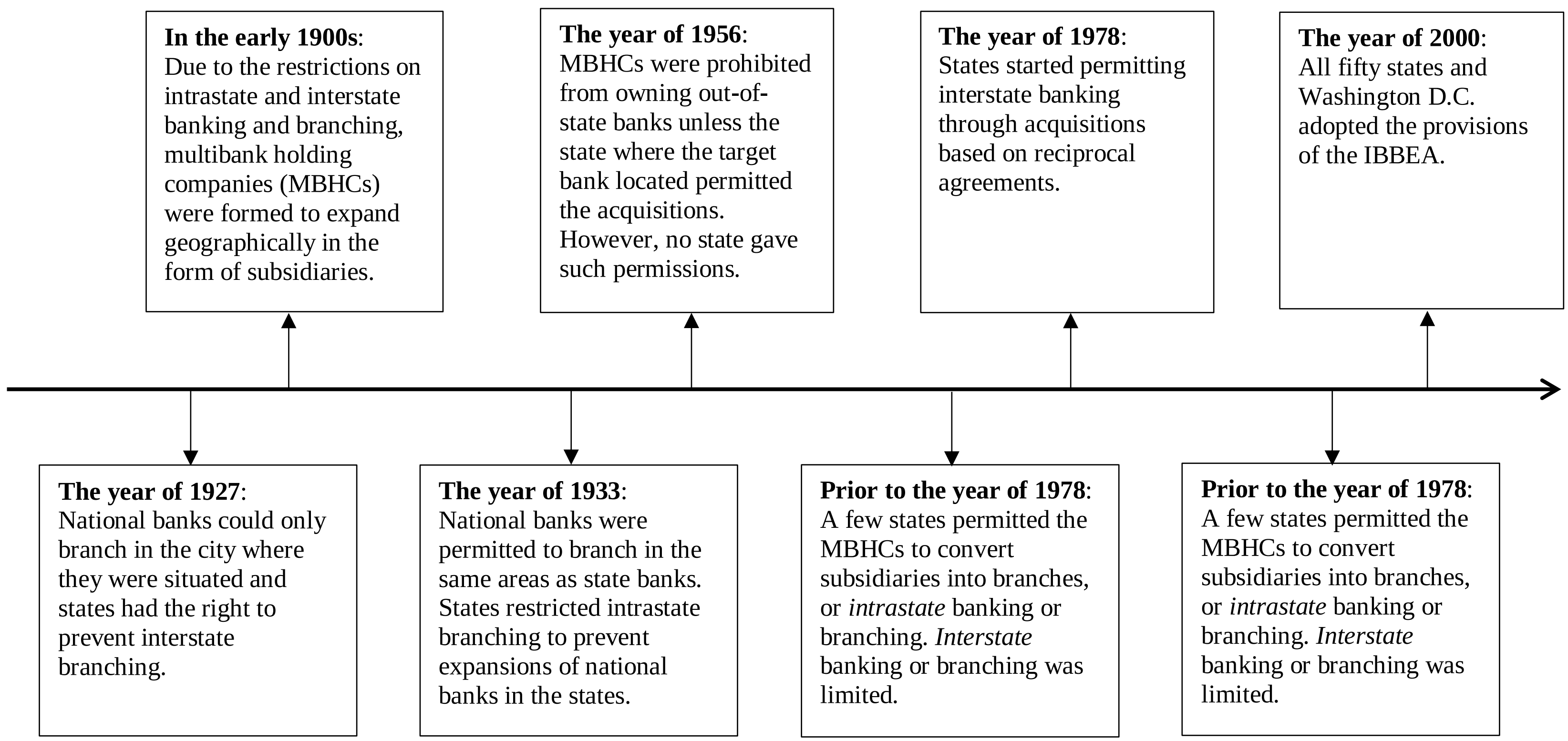
APPENDIX A

TIMELINE OF BANKING AND BRANCHING DEREGULATION



Note: This figure summarizes the history of banking and branching deregulation documented by Kroszner and Strahan (1999), Blair and Kushmeider (2006), Johnson and Rice (2008), and Rice and Strahan (2010).

APPENDIX B

THE EFFECTIVE DATES OF THE IBBEA

State	Effective Date	State	Effective Date
AK	01/01/1994	MT	09/29/1995
AL	05/31/1997	NC	07/01/1995
AR	06/01/1997	ND	05/31/1997
AZ	09/01/1996	NE	05/31/1997
CA	09/28/1995	NH	06/01/1997
CO	06/01/1997	NJ	04/17/1996
CT	06/27/1995	NM	06/01/1996
DC	06/13/1996	NV	09/29/1995
DE	09/29/1995	NY	06/01/1997
FL	06/01/1997	OH	05/21/1997
GA	06/01/1997	OK	05/31/1997
HI	06/01/1997	OR	07/01/1997
IA	04/04/1996	PA	07/06/1995
ID	09/29/1995	RI	06/20/1995
IL	06/01/1997	SC	07/01/1996
IN	06/01/1997	SD	03/09/1996
KS	09/29/1995	TN	06/01/1997
KY	06/01/1997	TX	08/28/1995
LA	06/01/1997	UT	06/01/1995
MA	08/02/1996	VA	09/29/1995
MD	09/29/1995	VT	05/30/1996
ME	01/01/1997	WA	06/06/1996
MI	11/29/1995	WI	05/01/1996
MN	06/01/1997	WV	05/31/1997
MO	09/29/1995	WY	05/31/1997
MS	06/01/1997		

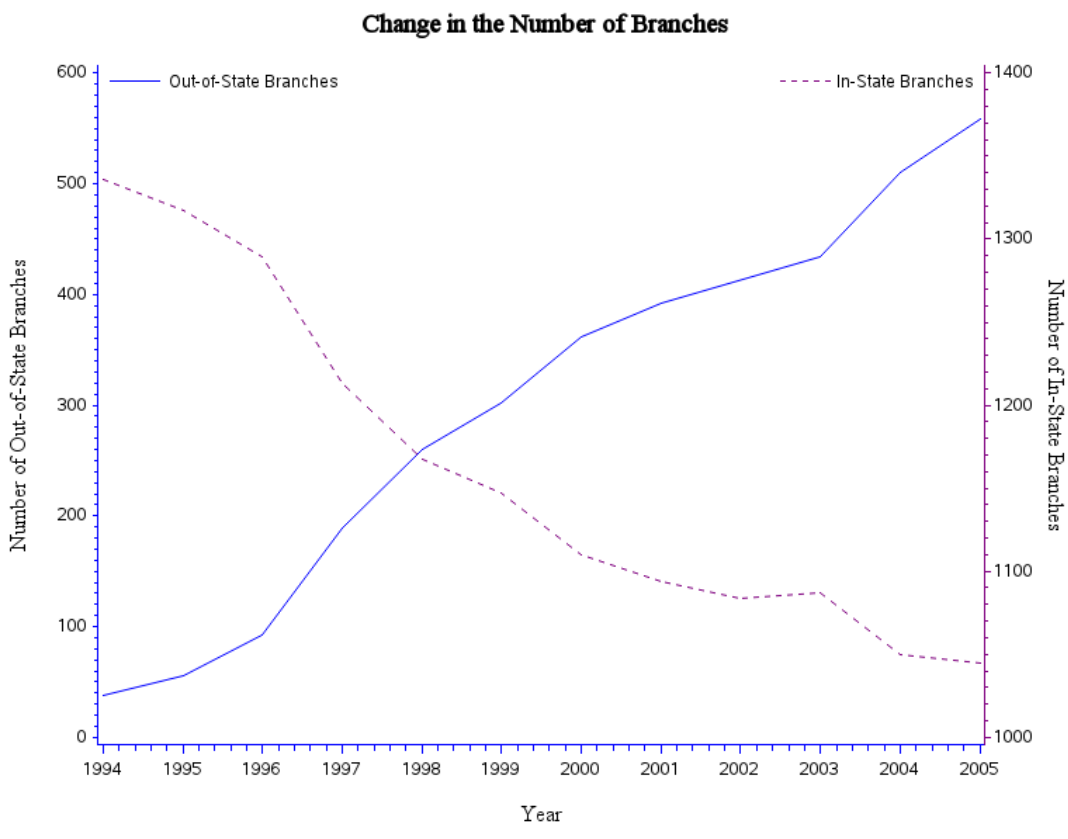
Note: This table is obtained from Table 1 of Rice and Strahan (2010).

APPENDIX C

THE CHANGE IN THE NUMBER OF BRANCHES

Year	1994	1997	2000	2005
	Mean (Std. Dev)			
Number of Out-of-State Branches Per State	37.98 (126.20)	189.54 (464.92)	361.61 (1027.56)	558.00 (1505.84)
Proportion of Out-of-State Branches to Total Branches	0.0298 (0.0747)	0.0882 (0.1252)	0.1366 (0.1856)	0.3488 (0.2074)

Note: This table replicates Table 3 of Johnson and Rice (2008) that summarizes the changes of the number of out-of-state branches per state after the IBBEA was passed. The banking branch data is obtained from the Summary of Deposits from Federal Deposit Insurance Corporation (FDIC).



Note: This figure is created based on the average number of branches per state. The banking branch data is obtained from the Summary of Deposits from Federal Deposit Insurance Corporation (FDIC). See Figure 1 of Johnson and Rice (2008) for more details on the number of branches, banks and bank holding companies after the IBBEA was passed.

APPENDIX D

VARIABLE DEFINITIONS

Variables	Definitions
ACC	The difference between net income and operating cash flow in year *t* deflated by the market value of common equity at the end of year *t-1*.
CFO	Operating cash flow (COMPUSTAT items OANCF – XIDOC) in year *t* deflated by the market value of common equity at the end of year *t-1*.
C-SCORE	Conditional conservatism measure calculated following Khan and Watts (2009).
IBBEA	*IBBEA (-n)* is an indicator variable for firms in the states that will pass IBBEA in n years, and *IBBEA (n)* is an indicator variable for firms in the states that passed IBBEA n years ago.
INDEX	Five minus the Branching Restrictiveness Index in Table 4. *INDEX* equals zero if the state had not passed the IBBEA by year *t-1*, equals one for states that enact all four restrictions by year *t-1*, and equals five for states that enact no restrictions by year *t-1*.
LEV	The scaled decile rank of leverage, calculated as the sum of long-term debt and current debt deflated by total assets.
LITIG	The litigation risk calculated from Equation (4) of Kim and Skinner (2012).
MTB	The scaled decile rank of market-to-book ratio, calculated as market value of common equity deflated by book value of common equity.
NEG	An indicator variable that equals one if *RET* is negative, and zero otherwise.
NI	Net income of year *t* deflated by the market value of common equity at the end of year *t-1*.
POST	An indicator variable that equals one if the state had passed the IBBEA by year *t-1*.
RET	Cumulative buy-and-hold returns from nine months before the fiscal year end to three months after the fiscal year end.
SIZE	The scaled decile rank of market value of common equity.

Table 1. Sample Selection and Distribution

Panel A: Sample Selection

	Number of Observations	Number of Firms
U.S. firms during fiscal year [1993, 1998] with non-missing headquarter information from Compustat	61,972	13,570
Restrictions:		
After removing observations if the firm's permno and stock return data are missing	44,411	10,625
After removing observations if the firm's financial data are missing	22,081	5,751
After removing observations if the firm is in the financial industry	17,967	4,513
After removing observations if the firm is headquartered in major territories	17,924	4,499
Final Sample	**17,924**	**4,499**

Panel B: Sample Distribution by State

State	Number of Observations	Number of Firms	State	Number of Observations	Number of Firms
AK	8	2	MT	15	4
AL	128	31	NC	372	88
AR	96	21	ND	10	3
AZ	266	73	NE	62	16
CA	2,362	658	NH	114	27
CO	420	116	NJ	844	218
CT	398	97	NM	24	7
DC	32	7	NV	147	46
DE	71	17	NY	1,499	366
FL	739	202	OH	752	178
GA	484	126	OK	163	43
HI	31	7	OR	179	46
IA	110	22	PA	798	188
ID	46	9	RI	72	16
IL	879	195	SC	78	21
IN	233	57	SD	36	8
KS	110	27	TN	269	72
KY	99	22	TX	1,806	458
LA	112	28	UT	169	43
MA	961	238	VA	476	114
MD	277	76	VT	29	8
ME	45	8	WA	246	62
MI	436	102	WI	375	78
MN	661	154	WV	19	6
MO	328	77	WY	1	1
MS	37	10			

Table 1. – Continued

Panel C: Sample Distribution by Effective Year

Effective Year	Number of States	Number of Observations	Number of Firms
1994	1	8	2
1995	16	7,883	2,020
1996	11	3,001	742
1997	23	7,032	1,735
Total	**51**	**17,924**	**4,499**

Panel D: Sample Distribution by Fiscal Year

Fiscal Year	Number of Observations	Pre-IBBEA	Post-IBBEA
1993	2,790	2,790	0
1994	2,850	2,850	0
1995	2,904	2,903	1
1996	3,073	1,880	1,193
1997	3,182	1,251	1,931
1998	3,125	0	3,125
Total	**17,924**	**11,674**	**6,250**

Note: Panel A of Table 1 presents the sample selection procedure. The final sample includes firm-year observations with non-missing data for necessary variables in Equation (2). Panel B presents the effective dates of the Riegle-Neal Interstate Banking and Branching Efficiency Act (IBBEA) in 50 states and Washington D.C., and the sample distribution in each state. The effective dates are replicated from Table 1 of Rice and Strahan (2010). Panel C presents the sample distribution by effective year. Panel D presents the sample distribution in each fiscal year of the sample period.

Table 2. Descriptive Statistics

	Mean	Std. Dev.	Min	Lower Quartile	Median	Upper Quartile	Max
Panel A: Full Sample (N=17,924)							
NI_t	0.019	0.142	-0.672	-0.002	0.053	0.084	0.329
RET_t	0.142	0.528	-0.738	-0.187	0.063	0.354	2.364
MTB_{t-1}	2.822	2.789	0.333	1.272	1.962	3.228	17.953
LEV_{t-1}	0.221	0.178	0.000	0.056	0.206	0.348	0.711
$SIZE_{t-1}$	5.134	1.985	1.234	3.654	4.959	6.517	10.141
$LITIG_{t-1}$	0.219	0.251	0.006	0.048	0.114	0.284	0.997
Panel B: Pre-IBBEA Sample (N=11,674)							
NI_t	0.022	0.145	-0.672	0.004	0.056	0.088	0.329
RET_t	0.186	0.516	-0.738	-0.124	0.099	0.376	2.364
MTB_{t-1}	2.700	2.735	0.333	1.217	1.856	3.071	17.953
LEV_{t-1}	0.223	0.176	0.000	0.062	0.208	0.348	0.711
$SIZE_{t-1}$	4.993	1.993	1.234	3.492	4.798	6.381	10.141
$LITIG_{t-1}$	0.214	0.249	0.006	0.047	0.111	0.274	0.997
Panel C: Post-IBBEA Sample (N=6,250)							
NI_t	0.014	0.137	-0.672	-0.013	0.046	0.078	0.329
RET_t	0.062	0.541	-0.738	-0.308	-0.023	0.309	2.364
MTB_{t-1}	3.051	2.874	0.333	1.418	2.179	3.523	17.953
LEV_{t-1}	0.219	0.181	0.000	0.043	0.202	0.348	0.711
$SIZE_{t-1}$	5.396	1.942	1.234	3.957	5.256	6.709	10.141
$LITIG_{t-1}$	0.228	0.255	0.006	0.051	0.118	0.307	0.997

Panel D: Pearson and Spearman Correlations (N=17,924)

	NI_t	*RET_t*	*MTB_{t-1}*	*LEV_{t-1}*	*$SIZE_{t-1}$*	*$LITIG_{t-1}$*
NI_t	**1.000**	**0.386**	**-0.129**	**0.071**	**0.066**	**-0.155**
RET_t	**0.229**	**1.000**	**-0.083**	0.005	**0.022**	**-0.078**
MTB_{t-1}	**-0.033**	**-0.051**	**1.000**	**-0.118**	**0.379**	**0.226**
LEV_{t-1}	-0.007	-0.012	**-0.048**	**1.000**	**0.070**	-0.011
$SIZE_{t-1}$	**0.154**	**-0.049**	**0.190**	**0.048**	**1.000**	**0.161**
$LITIG_{t-1}$	**-0.112**	**-0.027**	**0.181**	**-0.045**	0.002	**1.000**

Note: This table presents summary statistics for variables in the main regression of Equation (2). Panel A reports the descriptive statistics for the full sample. Panel B and Panel C report the descriptive statistics for pre- and post-IBBEA sample, respectively. In Panel D, coefficients below (above) the diagonal presents Pearson (Spearman) correlation. Correlation coefficients in bold are significant at 0.1 level. See Table A2 for variable definitions. All continuous variables are winsorized at 1% and 99%.

Table 3. The Effect of the IBBEA on Conditional Conservatism

Dependent Variable: NI_t						
	(1)		**(2)**		**(3)**	
	Coefficient	**t-value**	**Coefficient**	**t-value**	**Coefficient**	**t-value**
NEG_t	0.001	(0.31)	-0.016	(-1.66)	-0.016	(-1.63)
RET_t	0.008	(1.41)	0.022	(1.50)	0.023	(1.59)
$NEG_t \times RET_t$	0.293***	(18.45)	0.419***	(13.46)	0.415***	(13.38)
POST	-0.001	(-0.10)	-0.000	(-0.08)	0.001	(0.24)
$POST \times NEG_t$	-0.008	(-0.73)	-0.009	(-0.90)	-0.010	(-0.96)
$POST \times RET_t$	-0.003	(-0.60)	-0.002	(-0.45)	-0.009*	(-1.95)
$\boldsymbol{POST \times NEG_t \times RET_t}$	**-0.104*** **	**(-4.32)**	**-0.093*** **	**(-4.32)**	**-0.086*** **	**(-3.76)**
MTB_{t-1}			-0.001	(-0.08)	0.001	(0.13)
$MTB_{t-1} \times NEG_t$			-0.001	(-0.10)	-0.002	(-0.10)
$MTB_{t-1} \times RET_t$			-0.034**	(-2.47)	-0.033**	(-2.35)
$MTB_{t-1} \times NEG_t \times RET_t$			-0.214***	(-6.32)	-0.225***	(-6.96)
LEV_{t-1}			0.005	(0.74)	-0.001	(-0.11)
$LEV_{t-1} \times NEG_t$			0.008	(0.93)	0.007	(0.77)
$LEV_{t-1} \times RET_t$			0.013	(1.04)	0.014	(1.09)
$LEV_{t-1} \times NEG_t \times RET_t$			0.078***	(2.78)	0.085***	(3.06)
$SIZE_{t-1}$			0.031***	(3.37)	0.026***	(2.90)
$SIZE_{t-1} \times NEG_t$			0.023**	(2.41)	0.025**	(2.49)
$SIZE_{t-1} \times RET_t$			0.011	(0.92)	0.019	(1.57)
$SIZE_{t-1} \times NEG_t \times RET_t$			-0.142***	(-3.89)	-0.147***	(-3.92)
$LITIG_{t-1}$			-0.054***	(-4.95)	-0.046***	(-4.86)
$LITIG_{t-1} \times NEG_t$			0.019	(1.51)	0.020*	(1.68)
$LITIG_{t-1} \times RET_t$			0.005	(0.34)	0.001	(0.04)
$LITIG_{t-1} \times NEG_t \times RET_t$			-0.010	(-0.30)	0.004	(0.12)
Constant	0.047***	(18.80)	0.034***	(4.24)	0.013	(1.53)
# of Observations	17,924		17,924		17,924	
Adj. R^2	0.111		0.153		0.165	
State and Year FE	No		No		Yes	

Note: This table presents the results of the change in conservatism based on Equation (2). The sample period is from 1993 to 1998. See Table A2 for variable definitions. All continuous variables are winsorized at 1% and 99%. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4. Analysis Conditional on Branching Restrictions Enacted by States

Panel A: Branching Restrictiveness by State

State	**Branching Restrictiveness Index**	**(1)**	**(2)**	**(3)**	**(4)**	**State**	**Branching Restrictiveness Index**	**(1)**	**(2)**	**(3)**	**(4)**
AK	2	1	1	0	0	MT	4	1	1	1	1
AL	3	1	1	1	0	NC	0	0	0	0	0
AR	4	1	1	1	1	ND	3	0	1	1	1
AZ	3	1	1	1	0	NE	4	1	1	1	1
CA	3	1	1	1	0	NH	4	1	1	1	1
CO	4	1	1	1	1	NJ	1	0	1	0	0
CT	1	1	0	0	0	NM	3	1	1	1	0
DC	0	0	0	0	0	NV	3	1	1	1	0
DE	3	1	1	1	0	NY	2	1	1	0	0
FL	3	1	1	1	0	OH	0	0	0	0	0
GA	3	1	1	1	0	OK	4	1	1	1	1
HI	3	1	1	1	0	OR	3	1	1	1	0
IA	4	1	1	1	1	PA	0	0	0	0	0
ID	3	1	1	1	0	RI	0	0	0	0	0
IL	3	1	1	1	0	SC	3	1	1	1	0
IN	0	0	0	0	0	SD	3	1	1	1	0
KS	4	1	1	1	1	TN	3	1	1	1	0
KY	4	1	1	1	1	TX	4	1	1	1	1
LA	3	1	1	1	0	UT	2	1	1	0	0
MA	1	1	0	0	0	VA	0	0	0	0	0
MD	0	0	0	0	0	VT	2	1	1	0	0
ME	0	0	0	0	0	WA	3	1	1	1	0
MI	0	0	0	0	0	WI	3	1	1	1	0
MN	3	1	1	1	0	WV	1	0	0	0	1
MO	4	1	1	1	1	WY	3	1	1	1	0
MS	4	1	1	1	1						

Panel B: Summary of Branching Restrictiveness Index

	Index=0	**Index=1**	**Index=2**	**Index=3**	**Index=4**	**Enacted both (3) and (4)**	**Enacted (3) or (4) but not both**	**Enacted neither (3) or (4)**
Number of States	10	4	4	21	12	13	20	18

Table 4. – Continued

Panel C: Analysis Conditional on Branching Restrictiveness

Dependent Variable: NI_t	(1)		(2)		(3)	
	Coefficient	**t-value**	**Coefficient**	**t-value**	**Coefficient**	**t-value**
NEG_t	-0.002	(-0.59)	-0.017*	(-1.79)	-0.018*	(-1.82)
RET_t	0.006	(1.22)	0.020	(1.45)	0.021	(1.51)
$NEG_t \times RET_t$	0.271***	(19.46)	0.407***	(13.41)	0.406***	(13.90)
INDEX	-0.001	(-0.33)	-0.001	(-0.42)	-0.001	(-0.58)
$INDEX \times NEG_t$	-0.000	(-0.13)	-0.001	(-0.21)	-0.000	(-0.11)
$INDEX \times RET_t$	0.000	(0.19)	0.001	(0.29)	-0.001	(-0.85)
$\boldsymbol{INDEX \times NEG_t \times RET_t}$	**-0.027*** **	**(-3.69)**	**-0.025*** **	**(-3.80)**	**-0.024*** **	**(-3.77)**
Constant	0.047***	(17.78)	0.034***	(4.30)	0.014	(1.51)
# of Observations	17,924		17,924		17,924	
Adj. R^2	0.109		0.152		0.164	
Controls	No		Yes		Yes	
State and Year FE	No		No		Yes	

Note: This table presents the results of the change in conservatism based on Equation (2), conditional on restrictions enacted by states. Panel A replicates Table 1 of Rice and Strahan (2010) that reports the branching restrictiveness in each state after the IBBEA was passed. The Branching Restrictiveness Index is added one if states enacted one of the four restrictions denoted in Footnote 4. Panel B reports the summary of the Branching Restrictiveness Index. Panel C reports the regression results conditional on interstate restrictions. The sample period is from 1993 to 1998. Control variables *MTB*, *LEV*, *SIZE* and *LITIG* and their interactions with *NEG* and *RET* are included. See Table A2 for variable definitions. All continuous variables are winsorized at 1% and 99%. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4. – Continued

Panel D: Analysis Conditional on Different Enactment of Restrictions

Dependent Variable: *NI* $_t$	(1)		(2)		(3)	
	Coefficient	**t-value**	**Coefficient**	**t-value**	**Coefficient**	**t-value**
NEG $_t$	-0.023	(-0.97)	-0.001	(-0.03)	-0.027*	(-1.70)
RET $_t$	0.041**	(2.00)	0.016	(1.24)	0.022*	(1.78)
NEG $_t$ × *RET* $_t$	0.355***	(5.50)	0.501***	(11.20)	0.359***	(8.25)
POST	0.005	(0.44)	0.006	(0.68)	-0.010	(-1.24)
POST × *NEG* $_t$	-0.014	(-1.01)	-0.012	(-1.09)	-0.003	(-0.27)
POST × *RET* $_t$	-0.028**	(-2.13)	-0.014	(-1.37)	0.004	(0.39)
***POST* × *NEG* $_t$ × *RET* $_t$**	**0.008**	**(0.22)**	**-0.112*** **	**(-4.22)**	**-0.105*** **	**(-4.09)**
Constant	0.079*	(1.89)	0.079**	(2.48)	0.013	(0.27)
# of Observations	3,370		7,153		7,401	
Adj. R^2	0.179		0.179		0.147	
Controls	Yes		Yes		Yes	
State and Year FE	Yes		Yes		Yes	
Chi-Square (1) & (2)		6.22				
p-value		0.01				
Chi-Square (2) & (3)				0.04		
p-value				0.84		
Chi-Square (1) & (3)			4.35			
p-value			0.04			

Note: Panel D of Table 4 presents the results of the change in conservatism based on Equation (2), conditional on the restrictiveness of different provisions. Column (1) reports the regression results based on observations in the states that enact both Restrictions (3) and (4). Column (2) reports the regression results based on observations in the states that enact either Restriction (3) or (4) but not both. Column (3) reports the regression results based on observations in the states that enact neither Restrictions (3) nor (4). The sample period is from 1993 to 1998. Control variables *MTB*, *LEV*, *SIZE* and *LITIG* and their interactions with *NEG* and *RET* are included. See Table A2 for variable definitions. All continuous variables are winsorized at 1% and 99%. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5. Analysis Conditional on Operation Concentration

Dependent Variable: NI_t				
	(1)		(2)	
	Bottom Tercile		Top Tercile	
	Coefficient	**t-value**	**Coefficient**	**t-value**
NEG_t	-0.048	(-1.54)	-0.001	(-0.03)
RET_t	-0.009	(-0.26)	0.041*	(1.73)
$NEG_t \times RET_t$	0.250**	(2.48)	0.437***	(6.58)
POST	0.017	(1.29)	0.006	(0.75)
$POST \times NEG_t$	-0.025	(-0.76)	-0.008	(-0.49)
$POST \times RET_t$	-0.001	(-0.03)	-0.003	(-0.25)
$POST \times NEG_t \times RET_t$	**-0.043**	**(-0.77)**	**-0.146*** **	**(-4.55)**
Constant	0.069***	(4.03)	-4.199***	(-69.56)
# of Observations	3,693		3,703	
Adj. R^2	0.136		0.163	
Controls	Yes		Yes	
State and Year FE	Yes		Yes	
Chi-Square		2.99		
p-value		0.08		

Note: This table presents the results of the change in conservatism based on of Equation (2), conditional on the percentage of a firm's operations in the headquarter state (Garcia and Norli 2012) in the year prior to the IBBEA. The sample period is from 1993 to 1998. Control variables *MTB*, *LEV*, *SIZE* and *LITIG* and their interactions with *NEG* and *RET* are included. See Table A2 for variable definitions. All continuous variables are winsorized at 1% and 99%. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6. Analysis Conditional on Banks' Downside Risk

Dependent Variable: NI_t	Leverage				KZ-Index			
	(1)		(2)		(3)		(4)	
	Bottom Tercile		Top Tercile		Bottom Tercile		Top Tercile	
	Coefficient	**t-value**	**Coefficient**	**t-value**	**Coefficient**	**t-value**	**Coefficient**	**t-value**
NEG_t	-0.007	(-0.46)	-0.010	(-0.32)	-0.008	(-0.58)	-0.031	(-1.12)
RET_t	0.028	(1.13)	0.013	(0.39)	0.023	(1.28)	0.031	(1.05)
$NEG_t \times RET_t$	0.428***	(5.93)	0.473***	(4.98)	0.330***	(7.68)	0.379***	(5.13)
POST	-0.007	(-0.71)	0.009	(0.90)	-0.007	(-1.17)	0.019	(1.56)
$POST \times NEG_t$	-0.019	(-0.99)	0.022*	(1.65)	-0.025*	(-1.73)	0.008	(0.43)
$POST \times RET_t$	-0.002	(-0.19)	-0.002	(-0.20)	-0.008	(-0.90)	0.002	(0.17)
$POST \times NEG_t \times RET_t$	**-0.027**	**(-0.94)**	**-0.180*** **	**(-4.06)**	**0.011**	**(0.51)**	**-0.131** **	**(-2.45)**
Constant	-0.023**	(-2.23)	-3.891***	(-82.18)	0.067***	(5.98)	-0.147***	(-8.06)
# of Observations	5,608		5,689		6,089		5,196	
Adj. R^2	0.169		0.180		0.152		0.158	
Controls	Yes		Yes		Yes		Yes	
State and Year FE	Yes		Yes		Yes		Yes	
Chi-Square	10.60				6.91			
p-value	0.00				0.01			

Note: This table presents the results of the change in conservatism based on Equation (2), conditional on banks' downside risk in the year prior to the IBBEA. The sample period is from 1993 to 1998. Leverage is measured as the sum of long-term debt and current debt deflated by total assets. KZ-Index is the financial constraint index computed following Kaplan and Zingales (1997) and Lamont et al. (2001). Control variables *MTB*, *LEV*, *SIZE* and *LITIG* and their interactions with *NEG* and *RET* are included. See Table A2 for variable definitions. All continuous variables are winsorized at 1% and 99%. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7. Analysis Conditional on External Monitoring

Dependent Variable: NI_t	Analyst Following				Dedicated Institutional Ownership			
	(1)		(2)		(3)		(4)	
	Bottom Tercile		Top Tercile		Bottom Tercile		Top Tercile	
	Coefficient	**t-value**	**Coefficient**	**t-value**	**Coefficient**	**t-value**	**Coefficient**	**t-value**
NEG_t	-0.033**	(-2.52)	0.030	(1.41)	-0.026	(-1.34)	-0.008	(-0.49)
RET_t	0.026	(1.46)	0.022	(1.01)	0.004	(0.28)	0.048***	(3.29)
$NEG_t \times RET_t$	0.373***	(10.36)	0.345***	(3.14)	0.412***	(8.43)	0.335***	(7.21)
POST	0.015	(1.13)	0.002	(0.36)	0.011	(1.05)	0.000	(0.06)
$POST \times NEG_t$	-0.003	(-0.15)	-0.024**	(-2.14)	-0.007	(-0.60)	-0.007	(-0.75)
$POST \times RET_t$	-0.008	(-0.83)	-0.006	(-0.76)	-0.013	(-0.94)	-0.005	(-0.52)
$\boldsymbol{POST \times NEG_t \times RET_t}$	**-0.092****	**(-2.15)**	**0.005**	**(0.15)**	**-0.086*****	**(-2.79)**	**-0.038**	**(-1.50)**
Constant	0.070***	(3.98)	-0.091***	(-7.48)	0.157**	(2.43)	0.036	(0.80)
# of Observations	6,528		6,516		6,255		6,131	
Adj. R^2	0.148		0.161		0.144		0.162	
Controls	Yes		Yes		Yes		Yes	
State and Year FE	Yes		Yes		Yes		Yes	
Chi-Square		2.82				1.84		
p-value		0.09				0.18		

Note: This table presents the results of the change in conservatism based on Equation (2), conditional on external monitoring proxied by the number of analysts following and the percentage of dedicated institutional ownership in the year prior to the IBBEA. The sample period is from 1993 to 1998. Control variables *MTB*, *LEV*, *SIZE* and *LITIG* and their interactions with *NEG* and *RET* are included. See Table A2 for variable definitions. All continuous variables are winsorized at 1% and 99%. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8. Robustness Tests

Panel A: Dynamic Effects of the IBBEA

Dependent Variable: NI_t		
	[-3, 3]	
	Coefficient	**t-value**
IBBEA (-3) × *NEG* $_t$ × *RET* $_t$	-0.035	(-0.54)
IBBEA (-2) × *NEG* $_t$ × *RET* $_t$	-0.030	(-0.61)
IBBEA (-1) × *NEG* $_t$ × *RET* $_t$	-0.012	(-0.24)
IBBEA (0) × *NEG* $_t$ × *RET* $_t$	0.035	(0.66)
IBBEA (1) × *NEG* $_t$ × *RET* $_t$	-0.119**	(-2.42)
IBBEA (2) × *NEG* $_t$ × *RET* $_t$	-0.084*	(-1.94)
IBBEA (3) × *NEG* $_t$ × *RET* $_t$	-0.025	(-0.52)
Constant	0.024**	(2.52)
# of Observations	17,924	
Adj. R^2	0.167	
Controls	Yes	
State and Year FE	Yes	

Panel B: Placebo Tests

Dependent Variable: NI_t						
	(1)		**(2)**		**(3)**	
	Coefficient	**t-value**	**Coefficient**	**t-value**	**Coefficient**	**t-value**
NEG $_t$	-0.019	(-1.40)	-0.015	(-1.12)	-0.016	(-1.64)
RET $_t$	0.027	(1.34)	0.035**	(2.11)	0.028*	(1.99)
NEG $_t$ × *RET* $_t$	0.424***	(11.05)	0.402***	(11.02)	0.400***	(11.96)
POST	0.002	(0.37)	0.009*	(1.88)	-0.001	(-0.11)
POST × *NEG* $_t$	-0.006	(-0.52)	-0.017**	(-2.33)	-0.010	(-1.14)
POST × *RET* $_t$	-0.003	(-0.35)	-0.008	(-1.09)	-0.007	(-1.14)
***POST* × *NEG* $_t$ × *RET* $_t$**	**-0.056**	**(-1.25)**	**-0.035**	**(-1.26)**	**-0.035**	**(-1.63)**
Constant	0.011	(1.12)	0.006	(0.58)	0.011	(1.37)
# of Observations	17,924		17,924		17,924	
Adj. R^2	0.162		0.163		0.162	
Controls	Yes		Yes		Yes	
State and Year FE	Yes		Yes		Yes	

Note: Panel A of Table 8 presents the results on the dynamic effects of the IBBEA on conditional conservatism based on of Equation (2) during the sample period. Panel B of Table 8 presents the results on the change in conservatism based on Equation (2), if the effective date of IBBEA was N years earlier than the actual effective date, N=4, 3, and 2 in columns (1), (2), and (3), respectively. The sample period is from 1993 to 1998. Control variables *MTB, LEV, SIZE* and *LITIG* and their interactions with *NEG* and *RET* are included. See Table A2 for variable definitions. All continuous variables are winsorized at 1% and 99%. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8. – continued

Panel C: Historical Headquartered States

Dependent Variable: NI_t						
	(1)		(2)		(3)	
	Coefficient	**t-value**	**Coefficient**	**t-value**	**Coefficient**	**t-value**
NEG_t	0.001	(0.38)	-0.015	(-1.63)	-0.015	(-1.58)
RET_t	0.008	(1.38)	0.022	(1.51)	0.023	(1.60)
$NEG_t \times RET_t$	0.299***	(18.15)	0.430***	(13.83)	0.422***	(13.90)
POST	-0.001	(-0.12)	-0.001	(-0.10)	0.001	(0.23)
$POST \times NEG_t$	-0.008	(-0.76)	-0.010	(-0.92)	-0.011	(-0.97)
$POST \times RET_t$	-0.003	(-0.59)	-0.002	(-0.35)	-0.009*	(-1.72)
$\boldsymbol{POST \times NEG_t \times RET_t}$	**-0.112*** **	**(-4.42)**	**-0.097*** **	**(-4.37)**	**-0.089*** **	**(-3.78)**
Constant	0.047***	(19.18)	0.033***	(4.23)	0.013	(1.53)
# of Observations	17,909		17,909		17,909	
Adj. R^2	0.112		0.155		0.167	
Controls	No		Yes		Yes	
State and Year FE	No		No		Yes	

Note: Panel C of Table 8 presents the results on the change in conservatism based on Equation (2), using historical headquartered states from 10-Ks. The sample period is from 1993 to 1998. Control variables *MTB*, *LEV*, *SIZE* and *LITIG* and their interactions with *NEG* and *RET* are included. See Table A2 for variable definitions. All continuous variables are winsorized at 1% and 99%. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 9. Measurement Issues for Conditional Conservatism

Panel A: Decompose Earnings

	(1)		(2)	
	Dependent Variable=*ACC* $_t$		Dependent Variable=*CFO* $_t$	
	Coefficient	**t-value**	**Coefficient**	**t-value**
NEG_t	-0.018	(-1.05)	0.003	(0.21)
RET_t	-0.033*	(-1.96)	0.056***	(2.86)
$NEG_t \times RET_t$	0.229***	(5.88)	0.187***	(4.77)
POST	0.002	(0.20)	-0.000	(-0.02)
$POST \times NEG_t$	-0.001	(-0.19)	-0.008	(-1.05)
$POST \times RET_t$	0.010	(0.73)	-0.021	(-1.41)
$\boldsymbol{POST \times NEG_t \times RET_t}$	**-0.075****	**(-2.39)**	**-0.011**	**(-0.44)**
Constant	-0.242***	(-28.38)	0.255***	(26.27)
# of Observations	17,865		17,865	
Adj. R^2	0.093		0.130	
Controls	Yes		Yes	
State and Year FE	Yes		Yes	

Panel B: Alternative Measure of Conditional Conservatism

Dependent Variable: *C-Score* $_t$

	(1)		(2)		(3)	
	Coefficient	**t-value**	**Coefficient**	**t-value**	**Coefficient**	**t-value**
POST	**-0.035*****	**(-9.70)**	**-0.035*****	**(-9.61)**	**-0.039*****	**(-8.76)**
$LITIG_{t-1}$			-0.008***	(-6.21)	-0.007***	(-6.21)
Constant	0.040***	(65.44)	0.042***	(60.92)	0.036***	(11.86)
# of Observations	16,527		16,527		16,527	
Adj. R^2	0.255		0.258		0.286	
State FE	No		No		Yes	

Panel C: Alternative Measure of Conditional Conservatism, Independent Variable=*INDEX*

Dependent Variable: *C-Score* $_t$

	(1)		(2)		(3)	
	Coefficient	**t-value**	**Coefficient**	**t-value**	**Coefficient**	**t-value**
INDEX	-0.009***	(-6.36)	-0.009***	(-6.42)	-0.000*	(-1.91)
$LITIG_{t-1}$			-0.009***	(-7.94)	-0.003***	(-3.91)
Constant	0.036***	(51.42)	0.038***	(62.45)	0.039***	(49.22)
# of Observations	16,527		16,527		16,527	
Adj. R^2	0.165		0.169		0.640	
State and Year FE	No		No		Yes	

Note: Panel A of Table 9 presents the results of the change in conservatism based on Equation (2), using accruals and cash flow components of earnings as dependent variables. Panel B and Panel C of Table 9 present the results of the change in conservatism based on Equation (2), using C-Score as the dependent variable. The sample period is from 1993 to 1998. Control variables *MTB, LEV*, *SIZE* and *LITIG* and their interactions with *NEG* and *RET* are included in Panel A. See Table A2 for variable definitions. All continuous variables are winsorized at 1% and 99%. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.