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Does banking deregulation affect accounting conservatism?

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ABSTRACT

This study examines the effect of banking competition on borrowing firms' conditional accounting conservatism (i.e., asymmetric timely loss recognition). The context of the study is the staggered passage of the Interstate Banking and Branching Efficiency Act (IBBEA), the deregulation that permits banks to establish branches across state lines and increases bank competition. I find that firms report less conservatively after the passage of the IBBEA in their headquarter states. The effect on conditional conservatism is stronger for firms in states with a greater increase in competition among banks, firms that are more likely to borrow from in-state banks, firms with greater financial constraint, and firms subject to less external monitoring. Additional tests confirm that the decline in conditional conservatism is observed only after the adoption of IBBEA and lasts for two years. The findings indicate that banks tend to "lowball" borrowers when competition arises by relaxing their demand for conservative reporting. Overall, this study highlights the unintended impacts of banking competition on borrowing firms' financial reporting.

1. Introduction

The twentieth century saw the flourishing of the banking industry. Banks no longer focus on regional business only: they have expanded nationwide and compete across state borders. Such expansion is important, in part, because it expands the supply of credit and increases competition among banks. Prior research shows that intensified bank competition leads to beneficial economic outcomes, such as lower loan rates (Jayaratne and Strahan, 1998) and easier access to financing for small firms (Rice and Strahan, 2010). However, intensified bank competition also leads to unintended effects. For instance, banks voluntarily disclose more negative information (Burks et al., 2018) and increase loan loss provisions to deter entry (Tomy, 2019). Investigating the effects of bank competition in the past is crucial for future reference.

This study examines whether bank competition impacts asymmetric timely loss recognition—conditional conservatism—in the financial reporting of borrowing firms. In general, lenders prefer borrowers to recognize bad news more quickly than good news (Franke and Müller, 2019; Khan and Lo, 2018; LaFond and Watts, 2008; Nikolaev, 2010). When lending to a firm, the bank only receives a fixed amount of payment when the firm performs well but will likely receive less, or perhaps nothing, if the firm goes bankrupt. Conditional conservatism helps banks focus on the lower bound of borrowers' earnings distributions (Penalva and Wagenhofer, 2019; Watts, 2003), thus reducing firms' bankruptcy risk, mitigating conflicts of interest between debtholders and shareholders over dividend policy, triggering debt covenant violations more quickly, facilitating transferring control rights to lenders, and lowering the uncertainty of firm performance (Ahmed et al., 2002; Biddle et al., 2020; Christensen and Nikolaev, 2012; Guay and Verrecchia, 2018; Zhang, 2008). However, lenders' demand for conditional conservatism is not static. It changes as their monitoring incentives evolve (Deng et al., 2018; Erkens et al., 2014;

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Gormley et al., 2012; Khurana and Wang, 2015; Tan, 2013). Significant changes in the banking sector, such as increased bank competition, are likely to affect banks' monitoring incentives and thus impact borrowers' financial reporting choices.

In this study, I exploit an important event in the banking industry, the staggered passage of the Interstate Banking and Branching Efficiency Act (IBBEA), which permitted full interstate banking and branching[1]. Expanding across state borders became much more feasible for banks after the passage of the IBBEA[2]. Over fifty years ago, Horvitz (1965) proposed that increasing the entry of new banks would be one of the best ways to stimulate competition in the banking sector. Prior research has considered the passage of the IBBEA as a shock increasing bank competition (Amore et al., 2013; Burks et al., 2018; Chava et al., 2013; Cornaggia et al., 2015; Dou et al., 2018; Favara and Imbs, 2015; Rice and Strahan, 2010; Tomy, 2019; Zarutskie, 2006), with results generally consistent with Horvitz's (1965) views. For example, the number of bank branches and competition among banks increased in each state after the IBBEA (Johnson and Rice, 2008; Rice and Strahan, 2010).

Banks entering new markets must compete with incumbent banks that have developed relationships with local firms over time (Boot, 2000). Borrowing firms have more choices of lenders and thus possess greater bargaining power. Consistent with this argument, Bushman et al. (2016) find that, to remain competitive, banks adopt strategies that may increase their risks, such as lowering loan pricing and relaxing covenants. That is, when competition increases, banks will be increasingly concerned about losing clients; consequently, they may lessen their monitoring of borrowing firms and relax the constraints they impose on borrowers, including their demand for conservative financial reporting. As banks relax their constraints, managers may be more willing to report less conservatively. Conditional conservatism generally results in reduced reported earnings and thus adversely affects managers' compensation (Ahmed et al., 2002; García Lara et al., 2020; Watts, 2003).

However, it is possible that borrowing firms may not decrease their conditional conservatism. First, competition among borrowing firms may also intensify, as the passage of the IBBEA stimulates firms' innovation (Amore et al., 2013; Chava et al., 2013; Cornaggia et al., 2015) and promotes investment in productive projects (Krishnan et al., 2015). Since conditional conservatism is crucial in debt financing and reduces underinvestment (García Lara et al., 2016), firms may have to report conservatively in an effort to secure financing opportunities.

Second, banks will be concerned if their own performance is poor. Stiroh and Strahan (2003) suggest that in the competitive banking industry, there is pressure on all banks and those that do not outperform others have to exit the market. Therefore, banks need to screen their clients carefully, increasing the bank's likelihood of collecting all payments. Moreover, to the extent that banks are already offering "bargains" by lowering loan rates and relaxing covenants (Bushman et al., 2016; Jayaratne and Strahan, 1998), they may believe it is reasonable to continue demanding conservative financial reporting from their clients.

Third, conservative financial reporting can mitigate information asymmetry between new entrants and borrowers (Gormley et al., 2012). As banks enter new states, transactional banking becomes more important, which can be facilitated by increased disclosure and improved financial reporting quality (Breuer et al., 2018).

Finally, firms are subject to monitoring from multiple external parties, such as analysts, equity holders, and public debtholders (Chen et al., 2015; LaFond and Watts, 2008; Lee and Steele, 2019; Ramalingegowda and Yu, 2012). These external parties may not allow firms to report less conservatively even if banks relax their monitoring (Bharath and Hertzel, 2019). Weakened external monitoring of one party can lead to more demands for conservatism from other parties (Shi and You, 2016). Therefore, whether borrowing firms report less conservatively after the passage of the IBBEA is an empirical question.

To measure the change in conditional conservatism after the passage of the IBBEA, I follow prior research (Ettredge et al., 2012; García Lara et al., 2016, 2020; Gong and Luo, 2018; Jayaraman, 2012) and use the *C_SCORE* developed by Khan and Watts (2009) as a proxy for conditional conservatism. I also follow prior research using staggered settings and adopt a difference-in-difference design (Armstrong et al., 2012; Huang et al., 2016; Li et al., 2018). Using a sample of 8,549 firm-year observations over the period 1993–1998, I find that firms report less conservatively after their headquarter states adopt the IBBEA provisions and that the effect is more pronounced for firms headquartered in states that impose fewer restrictions[3]. The passage of the IBBEA in a bordering state does not affect the conditional conservatism in the home state. My results also show that the main result is not driven by an increase in conditional conservatism among the control group, which may make the treatment group appear to be less conservative. The results are robust to alternative measures for conditional conservatism, such as Basu's (1997) interacted measure, Collins et al.'s (2014) decomposing of earnings into accruals and cash flows, and Callen et al.'s (2010) conservatism ratio.

I also find that the decrease in conditional conservatism following the passage of the IBBEA is more pronounced for firms that likely have greater information asymmetry (i.e., smaller and younger firms, firms more likely to borrow and operate in their home states, and firms with less analyst following and institutional ownership). These results indicate that the information asymmetry between these firms and banks seems to be more severe after the IBBEA is enacted. Additional analysis of

[1] The details of the IBBEA can be found at https://www.congress.gov/bill/103rd-congress/house-bill/3841. There are generally four categories of provisions of interstate banking and branching: (1) interstate bank acquisitions; (2) interstate agency operations; (3) interstate branching; and (4) de novo branching.

[2] Johnson and Rice (2008, p. 75) document four general means of geographic expansion in the banking sector: "(1) interstate banking; (2) interstate branching; (3) intrastate banking; and (4) intrastate branching."

[3] The IBBEA allowed states to enact up to four restrictions on interstate banking and branching prior to June 1, 1997. Rice and Strahan (2010, p. 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%."

the dynamic effects of the IBBEA shows that such reduction in conditional conservatism lasts for only two years, suggesting that banks tend to "lowball" clients when they first arrive in the new market and impose more restrictions on clients' financial reporting once they establish a business relationship, otherwise they may face a greater potential downside risk in the long run.

I also conduct robustness tests that show the change in conditional conservatism is causally related to the IBBEA. First, if the decline in conditional conservatism is simply a time trend, then the change in conditional conservatism is likely to precede the passage of the law. However, I find that the change in conditional conservatism appears only after IBBEA adoption. Second, I follow Burks et al. (2018) and conduct placebo tests, using pseudo dates of the passage of the IBBEA in each state. I find no significant change in conditional conservatism in the pseudo post-IBBEA periods.

This study makes several contributions to the literature. First, it shows an unintended consequence of bank competition. Research has documented various effects of bank competition, including a decrease in risk for bank holding companies (Goetz et al., 2016) and an increase in the supply of mortgage loans (Favara and Imbs, 2015). The findings of this study complement the conclusions of Bushman et al. (2016) and show that banks adopt risky strategies when competition increases. My findings should be of interest to regulators, especially given recent government pushes toward deregulation in the financial industry. When bank competition increases after deregulation, regulators need to consider intensifying their monitoring of banks and firms.

Second, my findings add to the scant evidence on the relation between the characteristics of lenders and the reporting practices of borrowers. Several studies employ exogenous shocks that introduce changes in *borrowers'* characteristics to investigate the causal effect of lenders' demand on borrowers' conditional conservatism (Aier et al., 2014; Basu and Liang, 2019; Jayaraman and Shivakumar, 2013). Only a handful have investigated the effects of changes in the banking sector on their borrowers' financial reporting practices (Gormley et al., 2012; Khan and Lo, 2018; Martin and Roychowdhury, 2015; Tan, 2013). 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 and find that foreign bank entry is associated with a higher level of conditional conservatism. However, the authors acknowledge that the findings may not apply to developed countries such as the United States. My study complements theirs by using a different exogenous regulatory change in the U.S. banking industry and provides new insights into the role of the banking sector in shaping corporate financial reporting.

Another closely related study is by Hou et al. (2019), who adopt a similar setting and investigate a similar research question. My study is different from theirs in that I show that banks "lowball" clients by imposing fewer restrictions on clients' financial reporting when banking competition increases, and that banks do not relax their monitoring perpetually. In addition, I discuss various alternative explanations and empirically rule out the possibilities that my findings are driven by a change in the control group instead of the treatment group and by a time trend in conditional conservatism.

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.

2. Institutional background and hypothesis development

2.1. Background and consequences of banking and branching deregulation

2.1.1. Background on banking and branching deregulation

Regulation of interstate banking and branching has long been controversial. Banking deregulation is a result of both economic growth and rivalry between interested parties[4] (Rice and Strahan, 2010). Appendix A shows the timeline of banking and branching deregulation. Briefly, 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 permitted reciprocal interstate banking (Kroszner and Strahan, 1999); however, interstate branching remained greatly restricted.

To meet the increased credit demand 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 an important event in the banking sector, as it aimed to remove the last barriers to full interstate banking and permit full interstate branching. States had four options for whether and when to adopt the provisions[5]: (1) pass legislation before June 1, 1997, to "opt out" of interstate acquisitions of banks[6]; (2) pass legislation to adopt interstate consolidation of subsidiaries and interstate de novo branching; (3) pass legislation to adopt more desirable provisions, such as acquiring single branches without having to acquire the whole bank to enter the market; and (4) adopt provisions earlier than June 1, 1997, or adopt

[4] As summarized in prior research (Blair and Kushmeider, 2006; Rice and Strahan, 2010; Strahan (2003)), small banks seek to shield themselves from competition with large banks. Large banks, on the other hand, have incentives to expand their geographic scope. In addition, national banks are chartered by the federal government, while states receive chartering fees from state banks only. Therefore, states have incentives to prohibit the geographic expansion of banks or to limit competition among banks to enhance their revenue.

[5] See Footnote 1 for the summary of four provisions.

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

additional provisions later (Burks et al., 2018; Johnson and Rice, 2008; Rice and Strahan, 2010; Zarutskie, 2006). In addition, the IBBEA allowed states to enact up to four restrictions on interstate banking and branching prior to June 1, 1997[7]. Appendix B summarizes the effective dates of the IBBEA and the Branching Restrictiveness Index in all states[8].

While certain reciprocal interstate banking was permitted before 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 branches, whereas interstate branching allowed banks to directly establish branches in areas with strong credit demand (Rice and Strahan, 2010). Therefore, it was easier for out-of-state banks to enter the market after states adopted the interstate branching provisions of the IBBEA.

2.1.2. The economic consequences of bank competition

Deregulation in the banking sector fosters competition among banks because it relaxes the barriers to entry into other banking markets (Besanko and Thakor, 1992; Black and Strahan, 2002; Stiroh and Strahan, 2003). The increased competition induced by the IBBEA is consistent with the finding by Horvitz (1965) that one of the best ways to stimulate bank competition is to allow more banks to enter the market. Appendix C shows that the average number of out-of-state branches per state increased from less than 38 in 1994 to 558 in 2005, and the average number of in-state branches decreased from 1,336 in 1994 to 1,045 in 2005. Since 2012, the number of branches has declined due to the rise of technology and online banking services. However, "banks are still opening new branches" (Ensign et al., 2018). Competition among traditional banks and between traditional and alternative lenders is particularly fierce in the current deregulatory period.

Prior researchers document that bank competition in general is related to lower loan rates and more new incorporations (Black and Strahan, 2002; Jayaratne and Strahan, 1998). Given that interstate banking and branching were greatly restricted previously, the passage of the IBBEA created a shock that greatly increased state-level bank competition (Burks et al., 2018; Cornaggia et al., 2015; Rice and Strahan, 2010; Tomy, 2019; Zarutskie, 2006). Researchers have investigated the effect of the IBBEA 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), innovation of firms (Amore et al., 2013; Chava et al., 2013; Cornaggia et al., 2015), voluntary disclosure decisions of banks (Burks et al., 2018), and banks' loan loss provisions (Dou et al., 2018; Tomy, 2019). However, these previous studies have offered little evidence of the impact of bank competition on borrowers' decision-making, particularly financial reporting choices.

2.2. Prior literature on conditional conservatism and hypothesis development

2.2.1. Banks' demand for conditional conservatism from borrowing firms

Lenders face the downside risk in debt contracting (Penalva and Wagenhofer, 2019; Watts, 2003). 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 their debt contracts. Because of this downside risk, lenders require timely disclosure of bad news to take preventive actions (Erkens et al., 2014). Managers, however, tend to withhold bad news in general (Kothari et al., 2009). Thus, lenders set tight covenants to constrain firms' policies, such as those on dividend payout and capital expenditures, and scrutinize their performance closely, protecting lenders' wealth from being transferred to shareholders (Ahmed et al., 2002; Dichev and Skinner, 2002; Nikolaev, 2010; Nini et al., 2009).

Because lenders focus more on the left tail of borrowers' earnings distribution, conditional conservatism becomes particularly important for them. First, conservative reporting provides a lower-bound measure of earnings and helps lenders monitor borrowers' ability to pay (Penalva and Wagenhofer, 2019). Conservatism also constrains managers' opportunism by restricting earnings management and improves firms' investment efficiency (García Lara et al., 2016, 2020). Second, by recognizing bad news promptly, uncertainty about firm performance and firm value is reduced as well (Guay and Verrecchia, 2018). Finally, conditional conservatism also provides an early signal of default risk, so it can trigger debt covenant violations more quickly and facilitate the transfer of control rights to lenders (Christensen and Nikolaev, 2012; Zhang, 2008), reducing lenders' downside risk by allowing them to take protective actions sooner. Consistent with lenders' demand for conditional conservatism from borrowing firms, Franke and Müller (2019) find that firms report more conservatively after private loans are issued. Tan (2013) argues that creditors have a strong demand for information to protect their claims due to the information asymmetry between creditors and borrowers and finds that firms report more conservatively immediately after debt covenant violations.

When lenders' monitoring incentive weakens, there is a decline in borrowers' conditional conservatism. Erkens et al. (2014) document that when lenders can monitor their borrowers more closely through representation on the board of directors, their reliance on conservatism is reduced and firms' conditional conservatism decreases accordingly. Khurana and

[7] See Footnote 3 for the details of the restrictions.

[8] The Branching Restrictiveness Index is added one if states enacted one of the four restrictions denoted in Footnote 3. A higher Index means the state enacted more restrictions when the IBBEA was passed.

Wang (2015) find that when firms have more short-maturity debt, there is less demand for conservatism from lenders, and in this case, firms report less conservatively. Martin and Roychowdhury (2015) argue that when banks invest in credit default swaps (CDS), 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 their investment in CDS. As a result, borrowers' financial reporting is less conservative in the post-CDS period. Deng et al. (2018) show that borrowing firms report less conservatively when lenders' monitoring incentive is undermined after loan sales in the secondary market. Overall, prior literature suggests that conditional conservatism plays an important role in lending decisions and is impacted by changes in lenders' monitoring incentives.

2.2.2. The impact of the IBBEA on borrowing firms' conditional conservatism

Prior studies have employed different shocks that have induced changes in conditional conservatism, such as the passage of state antitakeover laws (Jayaraman and Shivakumar, 2013), the expansion of directors' fiduciary duties (Aier et al., 2014), and director-liability-reduction laws (Basu and Liang, 2019). These settings focus primarily on shocks to borrowers rather than lenders. There is scant evidence for how lenders' characteristics affect their demand for conservative reporting.

A noteworthy change among banks is the increase in competition during the deregulatory period, which is likely to affect their demand for conservative financial reporting. Specifically, banks' bargaining power relative to that of clients may have weakened after the passage of the IBBEA because banks entering new markets must compete with incumbents that have developed relationships with local firms (Boot, 2000). Borrowing firms, on the other hand, have more choices of lenders and thus greater bargaining power. As a result, although banks still prefer conditional conservatism, their weakened bargaining power affects their ability to impose conservative financial reporting on their clients.

Furthermore, to compete with new entrants, incumbent banks are willing to relax their demand for conservative reporting in addition to offering a lower interest rate because they can acquire soft information from local clients (Hollander and Verriest, 2016). Competing banks from other states want to start a business in the new market, so they may have to make similar offers to attract clients. In sum, banks are likely to relax their demand for conservative reporting to retain clients in a more competitive market since borrowers have relatively stronger bargaining power.

Prior literature finds evidence consistent with a decline in bank monitoring after the passage of the IBBEA. Jiang (2016) documents that banks design less restrictive loan contracts after the IBBEA is adopted; specifically, the number of financial covenants is lower, and the covenants are looser. Financial covenants relying on borrowers' accounting information reduce agency costs and help lenders monitor borrowers (Christensen and Nikolaev, 2012). When banks reduce the number of financial covenants and alleviate constraints on borrowers, their monitoring of borrowing firms is less intense. Under less restrictive loan contracts and less monitoring, managers of the borrowing firms may act more in their interest and deliver less conservative financial reporting.

From the supply side, managers have incentives to report less conservatively after the passage of the IBBEA. Conditional conservatism constrains managers' accrual-based earnings management (García Lara et al., 2020), which can reduce the net income upon which managers' compensation and investors' evaluation are based (Ahmed, et al. 2002; Watts, 2003). In addition, when borrowers contract with banks, reporting more conservatively allows borrowing firms to obtain a lower cost of debt (Callen et al., 2016). However, as bank competition intensifies, loan rates are lowered (Degryse and Ongena, 2005; Hauswald and Marquez, 2006; Hollander and Verriest, 2016). Therefore, firms may not have to supply conservative reporting to secure a low cost of debt as they did before.

Following prior research (Amore et al., 2013; Burks et al., 2018; Chava et al., 2013; Cornaggia et al., 2015; Dou et al., 2018; Favara and Imbs, 2015; Rice and Strahan, 2010; Tomy, 2019; Zarutskie, 2006), I use the IBBEA as a shock that increases state-level bank competition[9]. Based on the discussion above, my first hypothesis, stated in the alternative form, is as follows:

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

The effect of the IBBEA may vary across states. As discussed in Section 2.1, not all states fully adopted the provisions when the IBBEA took effect. Furthermore, the law allowed states to enact up to four restrictions on interstate banking and branching before June 1, 1997. The more restrictions a state enacted, the more difficult it was for out-of-state banks to enter. As a result, the increase in bank competition would have been suppressed. 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.

[9] These prior studies do not provide evidence that the passage of the IBBEA is related to firms' prior or intended conditional conservatism in financial reporting. As discussed in Footnote 4, the passage of the IBBEA was intended to resolve the rivalry between small banks and large banks and between states and the federal government, not to change the financial reporting behavior of firms. Therefore, the passage of the IBBEA is not a result of firms' lobbying and the event is likely to be exogenous to firms' financial reporting decisions. It is possible that the passage of the IBBEA is a result of economic improvement. However, such economic improvement means there will be more competition among firms for productive projects. The increase in firm competition is related to an increase in conservatism (Dhaliwal et al., 2014), which militates against my main findings.

However, I may not find a negative relation between bank competition and borrowing firms' conditional conservatism for several reasons. First, as banks start their new business, borrowers in their home states must compete with firms in other states for funds. Moreover, bank competition leads to positive economic outcomes[10] which may offer better investment opportunities for firms and intensify competition among borrowers. To the extent that conditional conservatism reduces underinvestment (García Lara et al., 2016) and that firms report more conservatively when product market competition increases (Dhaliwal et al., 2014), conditional conservatism may not decrease after the passage of the IBBEA.

Second, banks care about their own performance when facing intensified competition[11]. To survive in a competitive market, banks need 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.

Third, given the information asymmetry between banks and borrowers, banks require more conservative reporting when they enter a new market (Gormley et al., 2012). In the setting of the IBBEA, as banks enter other states, transactional banking becomes more important. As discussed in Breuer et al. (2018), a higher level of disclosure and improved financial reporting reduce the information advantage of relationship banking and facilitate transactional banking. Therefore, after the IBBEA is adopted, new entrants may demand conservative financial reporting from borrowers.

Finally, even if banks relax their demand for conservatism, firms are subject to governance from other parties that may not allow them to report less conservatively (Chen et al., 2015; LaFond and Roychowdhury, 2008; LaFond and Watts, 2008; Lee and Steele, 2019; Ramalingegowda and Yu, 2012). Bharath and Hertzel (2019) document that alternative forms of external governance are substitutes. A decrease in one form of external monitoring, such as lower analyst coverage, can lead to stronger demands for conservatism from other external parties due to the increase in information asymmetry (Shi and You, 2016). Overall, whether borrowers' conditional conservatism decreases after the IBBEA is enacted remains an empirical question.

3. Data and research design

3.1. The effect of the IBBEA on conditional conservatism

To measure the impact of the passage of the IBBEA on conditional conservatism, I use the following model:

$$C\_SCORE_t = \beta_0 + \beta_1 POST + \gamma \sum Controls_{t-1} + \alpha_s + \alpha_t + \varepsilon_t \quad (1)$$

In Equation (1), *C_SCORE* is a firm-year measure of conditional conservatism in year *t* following Khan and Watts (2009)[12]. *C_SCORE* is a linear combination of size, market-to-book ratio, and leverage:

$$C\_SCORE_t = \rho_0 + \rho_1 SIZE_t + \rho_2 MTB_t + \rho_3 LEV_t \quad (2)$$

The coefficients to estimate *C-SCORE* are obtained by incorporating size, market-to-book ratio, and leverage into the Basu (1997) estimation regression as follows:

$$\begin{aligned} NI_t = {} & \eta_0 + \eta_1 NEG_t + RET_t(v_0 + v_1 SIZE_t + v_2 MTB_t + v_3 LEV_t) + NEG_t \times RET_t(\rho_0 + \rho_1 SIZE_t + \rho_2 MTB_t + \rho_3 LEV_t) \\ & + (\varphi_1 SIZE_t + \varphi_2 MTB_t + \varphi_3 LEV_t + \varphi_4 NEG_t \times SIZE_t + \varphi_5 NEG_t \times MTB_t + \varphi_6 NEG_t \times LEV_t) + \varepsilon_t \end{aligned} \quad (3)$$

NI is the 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 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). *SIZE* is the market value of common equity. *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. Many prior studies employ *C_SCORE* as the main measure of conservatism and validate that *C_SCORE* captures the variations in the incremental sensitivity of earnings to bad news versus good news (Ettredge et al., 2012; García Lara et al., 2016, 2020; Gong and Luo, 2018; Jayaraman, 2012).

Because the passage of the IBBEA is a staggered event (i.e., different states passed the act at different times), I include a dummy variable *POST* in the model to investigate the effect of the IBBEA on conditional conservatism. *POST* is an indicator variable that equals one if a firm's headquarter state[13] has passed the IBBEA by year *t-1* and zero otherwise[14]. The coefficient on *POST*, β_1, represents the change in conditional conservatism of firms that experienced the passage of the IBBEA in year *t-1*

[10] Bank competition lowers loan rates (Jayaratne and Strahan, 1998), benefits firms' innovation (Amore et al., 2013; Chava et al., 2013; Cornaggia et al., 2015), and promotes investment in productive projects (Krishnan et al., 2015).

[11] Stiroh and Strahan (2003) suggest that banking deregulation increases competitive pressure for both strong and weak banks. When banks perform poorly, they have to exit from the market, and their resources will be transferred to better performers (Jayaratne and Strahan 1998; Stiroh and Strahan 2003).

[12] For easier interpretation of the empirical results, *C_SCORE* is multiplied by 100.

[13] An underlying assumption in this study is that firms borrowed from local banks in their headquarter states before the passage of the IBBEA. Amore et al. (2013) argue that the tendency to borrow from local banks is strong for both private and public firms because the processing of information is more convenient with a closer distance. Moreover, prior studies that measure the distance between borrowers and lenders use headquarters or main offices as the locations of firms (Hollander and Verriest, 2016; Petersen and Rajan, 2002).

[14] For instance, California passed IBBEA in 1995. For firms headquartered in CA, *POST* = 0 in the year and before 1995; *POST* = 1 after the year 1995.

compared to the conditional conservatism of firms that did not experience such a change in bank competition. β_1 is expected to be negative. The model also includes state fixed effects α_s and year fixed effects α_t (Bertrand and Mullainathan, 2003; Tomy, 2019). State fixed effects control for time-invariant differences across states. As shown in Bertrand et al. (2004) and Armstrong et al. (2012)[15], by incorporating *POST*, as well as state and year fixed effects, the model is an effective differences-in-differences design.

This specification is commonly adopted in accounting research that studies the impact of a staggered event (e.g., Armstrong et al., 2012; Huang et al., 2016; Li et al., 2018). Armstrong et al. (2012) study the effect of the staggered passage of state antitakeover laws on the information environment. Huang et al. (2016) investigate whether staggered large tariff reduction in different industries affects management's earnings forecasts decisions. Li et al. (2018) document how staggered adoption of a trade secrets law impacts corporate disclosure. *POST* in Eq. (1) is defined similarly to the key independent variables in these studies. In this study, staggered passage of IBBEA means that the treatment group includes firms headquartered in states that passed IBBEA in year *t-1*, and the control group consists of firms headquartered in states that passed the IBBEA before year *t-1* or will pass the IBBEA after year *t-1*[16].

Following prior literature (Beaver and Ryan, 2005; Callen et al., 2010; García Lara et al., 2011, 2016; Givoly et al., 2007; Gormley et al., 2012; Khan and Watts, 2009; LaFond and Watts, 2008), I control for the demand for conditional conservatism by incorporating a set of firm characteristics. *MTB, LEV,* and *SIZE* are defined as in Eq. (3). *LITIG* is the litigation risk calculated from Kim and Skinner (2012)'s Eq. (4).

As suggested by Hsieh et al. (2019), companies that focus on new business opportunities are facing greater ambiguity and thus exhibit a higher level of conservatism. A higher level of accounting conservatism is associated with more efficient investment (García Lara et al., 2016; Hong et al., 2019), more innovation (Laux and Ray, 2020), and lower risk in operating cash flow (Biddle et al., 2015). Because intensified bank competition reduces firms' risk and volatility (Jiang et al., 2020) and affects their operational strategies, such as risk-taking (Basu et al., 2019) and innovation (Amore et al., 2013; Chava et al., 2013; Cornaggia et al., 2015), these changes in business strategies may affect firms' financial reporting decisions. To account for this bias, I add *ROA*, *CAPX*, *RND*, and *RETVOL* as control variables. *ROA* is the return on assets calculated as net income scaled by total assets. *CAPX* is capital expenditures calculated as total capital expenditures scaled by total assets. *RND* is a firm's R&D expenditures scaled by total assets. *RETVOL* is stock return volatility over the fiscal year calculated using monthly stock returns. Since the distribution of *LITIG* is between zero and one and Table 2 shows that the distributions of control variables are skewed, I use the scaled decile ranks of control variables to estimate Eq. (1) (Ahmed and Duellman, 2013; LaFond and Roychowdhury, 2008). In addition, 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 in 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. The *INDEX* variable in this study 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 among the most restrictive regarding 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 Eq. (1) is replaced with *INDEX* as follows.

$$C\_Score_t = \beta_0 + \beta_1 INDEX + \gamma \sum Controls_{t-1} + \alpha_s + \alpha_t + \varepsilon_t \quad (4)$$

All variables are defined as in Eq. (1). 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 variable *INDEX*, β_1, will be negative.

3.3. Sample selection and descriptive statistics

Appendix B reports the effective dates in all states; the information is obtained from Table 1 of Rice and Strahan (2010). Although a state could adopt additional provisions after the IBBEA took effect, the effect of the law was the greatest when the state first passed the act. Thus, I use only the first date when the state adopted the provisions of the IBBEA. On Jan. 1, 1994, Alaska became the first to adopt the provisions retroactively. 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. I use historical headquarter information collected by Bill McDonald[17]. Sample firms with non-missing historical headquarter information are obtained from COMPUSTAT,

[15] Armstrong et al. (2012, pp. 191-192) discuss the difference-in-difference model in detail.

[16] For instance, California (CA) passed the IBBEA in 1995, Arizona (AZ) passed the IBBEA in 1996, and Arkansas (AR) passed the IBBEA in 1997. Firms headquartered in AZ are the treatment group in 1997, and firms headquartered in CA and AR are the control group in 1997. In 1998, the treatment group includes firms headquartered in AR, and the control group includes the firms headquartered in CA and AZ.

[17] The data are available at https://sraf.nd.edu/data/augmented-10-x-header-data/.

Table 1
Sample Selection and Distribution.

Panel A: Sample Selection

	Number of Observations	Number of Firms
U.S. firms during the fiscal years [1993, 1998] from COMPUSTAT	62,619	13,706
Restrictions:		
After removing observations if the firm's historical headquarter information or the Branching Restrictiveness Index is missing	61,887	13,542
After removing observations if the firm is in the financial industry	47,724	10,454
After removing observations if the firm's stock return and financial data used to calculate C-score are missing	22,621	6,266
After removing observations if the firm's stock return and financial data used to calculate control variables are missing	8,549	2,346
Final Sample	**8,549**	**2,346**

Panel B: Sample Distribution by State

State	Number of Observations	Number of Firms	State	Number of Observations	Number of Firms
AK	1	1	MT	8	3
AL	65	17	NC	110	32
AR	22	6	ND	1	1
AZ	78	22	NE	25	6
CA	1,521	455	NH	49	14
CO	170	60	NJ	458	125
CT	282	71	NM	9	6
DC	15	3	NV	69	22
DE	18	6	NY	729	200
FL	290	103	OH	376	88
GA	170	55	OK	58	15
HI	3	1	OR	110	32
IA	60	13	PA	436	111
ID	31	6	RI	42	9
IL	373	96	SC	24	6
IN	113	27	SD	11	5
KS	44	13	TN	96	28
KY	27	8	TX	454	138
LA	15	5	UT	99	31
MA	621	173	VA	174	43
MD	141	45	VT	2	1
ME	19	5	WA	142	41
MI	215	58	WI	184	42
MN	439	111	WV	5	1
MO	123	37	WY	3	1
MS	19	1			

Panel C: Sample Distribution by Effective Year

Effective Year	Number of States	Number of Observations	Number of Firms
1994	1	1	1
1995	16	3,767	1,074
1996	11	1,604	433
1997	23	3,177	877
Total	**51**	**8,549**	**2,385**

Panel D: Sample Distribution by Fiscal Year

Fiscal Year	Number of Observations	Pre-IBBEA	Post-IBBEA
1993	1,315	1,315	0
1994	1,323	1,323	0
1995	1,376	1,376	0
1996	1,501	936	565
1997	1,530	579	951
1998	1,504	1	1,503
Total	**8,549**	**5,530**	**3,019**

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 (1). Panel B presents the sample distribution by state. Panel C presents the sample distribution by effective year. The total number of firms is slightly higher than in Panel A because some firms changed their headquarter states during the sample period. Panel D presents the sample distribution by the fiscal year of the sample period.

and stock return data are obtained from CRSP. After excluding firms without necessary data to compute variables in Eq. (1) and firms in the financial industry (SIC codes 6000 to 6999), the final sample consists of 8,549 firm-year observations (2,346 unique firms) during the sample period 1993 to 1998, as reported in Table 1, Panel A. The sample period starts one year before the first adoption of IBBEA and ends one year after all states had enacted it to ensure the effect is driven by the passage of the IBBEA.

Table 2 summarizes the descriptive statistics of the sample firms. Panel A reports the summary statistics of the full sample; Panel B and Panel C report the descriptive statistics of sample firms in the pre- and post-IBBEA periods, respectively. *C_SCORE* has a mean of 3.754 before the IBBEA and a mean of 0.651 in the post-IBBEA period. The change in *C_SCORE* indicates preliminary evidence of a decrease in conditional conservatism after the IBBEA. It is also worth noting that there are more firms with negative *C_SCORE* post-IBBEA, indicating that these firms are relatively less conservative after the passage of the IBBEA. This result again provides univariate evidence that increased bank competition weakens banks' monitoring incentives, including their demand for conservative financial reporting.

4. Empirical results

4.1. The effect of the IBBEA on conditional conservatism (H1)

H1 posits that firms are likely to report less conservatively after IBBEA is adopted in their headquarter states. Column (1) of Table 3 presents the multivariate regression results for H1. The coefficient of *POST*, β_1, is negative and statistically significant at the 1% level, indicating that sample firms recognize bad economic news less timely after the IBBEA is adopted in their headquarter states. Specifically, after the passage of the IBBEA, conditional conservatism measured using *C_SCORE* declines by 12.19% (=0.324/2.658) relative to its unconditional mean (2.658) in my sample. The results are consistent with H1 that firms report less conservatively after their states adopt 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, competition among banks in states with fewer restrictions on interstate banking and branching will be greater than in those with more restrictions. Therefore, the negative relation between the IBBEA and conditional conservatism is likely to be more pronounced in states more open to interstate banking and branching.

In Appendix B, Panel A replicates the branching restrictiveness index constructed by Rice and Strahan (2010). Panel B summarizes the Branching Restrictiveness Index. Ten states do not impose any restrictions, while 12 states enact all four restrictions.

Column (2) of Table 3 reports the multivariate regression results for H2. As in Column (1), the coefficient of *INDEX*, β_1, is negative and statistically significant at the 1% level. Specifically, when a state adopted the IBBEA and enacted all four restrictions (e.g., *INDEX* increases from 0 to 1), or a state relaxed one of the four restrictions (e.g., *INDEX* increases from 1 to 2), conditional conservatism measured using *C_SCORE* on average declines by 2.29% (=0.061/2.658) relative to its unconditional mean (2.658) in the sample. The results indicate that firms' conditional conservatism decreases after the passage of the IBBEA, and those headquartered in states more open to interstate banking and branching report less conservatively after the IBBEA is passed than those headquartered in states with more restrictions.

4.3. Are control firms affected when a bordering state passed the IBBEA?

While the main results discussed in Sections 4.1 and 4.2 show that firms report less conservatively after the adoption of the IBBEA, there may be an increase in conditional conservatism of the control group, which makes the treatment group appear to be relatively less conservative. For instance, Utah (UT) passed the IBBEA in 1995 and Arizona (AZ) passed it in 1996. Firms in UT would be the treatment group in 1996 and firms in AZ would be the control group. When banks from AZ were allowed to enter UT but banks from other states were not allowed to enter AZ, borrowers in AZ might have to compete with those in UT for limited available funds, thus increasing their conditional conservatism. In this case, even if firms in UT did not change their conditional conservatism, they might appear to be less conservative than firms in AZ.

To empirically test this alternative explanation, I limit the sample to the control group only and check the change in their conditional conservatism when a bordering state passed the IBBEA. As discussed in Section 3.1, the control group consists of firms headquartered in states that passed the IBBEA before year *t-1* or will pass the IBBEA after year *t-1*. I construct a variable, *POSTBS*, that equals one if a bordering state passed the IBBEA at least 12 months ago and zero if none of the bordering states has passed the IBBEA[18]. I replace *POST* in Eq. (1) with *POSTBS*.

Column (3) of Table 3 reports the multivariate regression results of the test. The coefficient of *POSTBS* is not statistically significant, meaning that the control group does not exhibit an increase in conditional conservatism when their bordering

[18] For example, *POSTBS* = 0 for firms in AZ in 1995 because none of the bordering states passed the IBBEA in 1994, but *POSTBS* = 1 for firms in AZ in 1996 because Utah passed the IBBEA on 6/1/1995.

Table 2
Descriptive Statistics.

	Mean	Std. Dev.	Min	Lower Quartile	Median	Upper Quartile	Max
Panel A: Full Sample (N = 8,549)							
C_SCORE $_t$	2.658	3.227	−8.234	1.638	3.452	4.618	8.476
MTB $_{t-1}$	3.125	2.894	0.487	1.415	2.234	3.683	17.698
LEV $_{t-1}$	0.175	0.161	0.000	0.024	0.144	0.282	0.675
SIZE $_{t-1}$	5.097	1.844	1.629	3.707	4.909	6.414	9.263
LITIG $_{t-1}$	0.251	0.264	0.006	0.058	0.140	0.356	0.996
ROA $_{t-1}$	0.004	0.173	−0.850	−0.006	0.046	0.088	0.260
CAPX $_{t-1}$	0.063	0.056	0.002	0.028	0.048	0.078	0.395
RND $_{t-1}$	0.071	0.102	0.000	0.007	0.033	0.095	0.608
RETVOL $_{t-1}$	0.132	0.070	0.030	0.080	0.117	0.167	0.397
Panel B: Pre-IBBEA Sample (N = 5,530)							
C_SCORE $_t$	3.754	1.882	−5.664	2.680	3.837	5.028	8.476
MTB $_{t-1}$	2.964	2.819	0.487	1.340	2.081	3.432	17.698
LEV $_{t-1}$	0.179	0.159	0.000	0.031	0.150	0.286	0.675
SIZE $_{t-1}$	4.937	1.841	1.629	3.519	4.740	6.216	9.263
LITIG $_{t-1}$	0.240	0.259	0.006	0.055	0.134	0.331	0.996
ROA $_{t-1}$	0.006	0.165	−0.850	−0.004	0.045	0.087	0.260
CAPX $_{t-1}$	0.062	0.055	0.002	0.027	0.048	0.077	0.395
RND $_{t-1}$	0.067	0.096	0.000	0.007	0.031	0.088	0.608
RETVOL $_{t-1}$	0.128	0.069	0.030	0.078	0.113	0.161	0.397
Panel C: Post-IBBEA Sample (N = 3,019)							
C_SCORE $_t$	0.651	4.097	−8.234	−2.981	2.168	3.878	8.476
MTB $_{t-1}$	3.420	3.005	0.487	1.590	2.519	4.037	17.698
LEV $_{t-1}$	0.169	0.163	0.000	0.016	0.133	0.273	0.675
SIZE $_{t-1}$	5.391	1.813	1.629	4.007	5.200	6.678	9.263
LITIG $_{t-1}$	0.272	0.272	0.006	0.065	0.157	0.404	0.996
ROA $_{t-1}$	0.000	0.185	−0.850	−0.009	0.048	0.092	0.260
CAPX $_{t-1}$	0.065	0.057	0.002	0.028	0.049	0.081	0.395
RND $_{t-1}$	0.080	0.111	0.000	0.007	0.037	0.109	0.608
RETVOL $_{t-1}$	0.140	0.071	0.030	0.085	0.125	0.178	0.397

Panel D: Pearson and Spearman Correlations (N=8,549)

	C_SCORE $_t$	*MTB* $_{t-1}$	*LEV* $_{t-1}$	*SIZE* $_{t-1}$	*LITIG* $_{t-1}$	*ROA* $_{t-1}$	*CAPX* $_{t-1}$	*RND* $_{t-1}$	*RETVOL* $_{t-1}$
C_SCORE $_t$	**1.000**	**−0.190**	**0.135**	**−0.055**	**−0.087**	**−0.040**	**−0.021**	**−0.094**	**−0.062**
MTB $_{t-1}$	**−0.098**	**1.000**	**−0.119**	**0.366**	**0.212**	**0.196**	**0.098**	**0.276**	**0.142**
LEV $_{t-1}$	**0.112**	**−0.037**	**1.000**	**0.044**	**−0.025**	**−0.212**	**0.029**	**−0.368**	**−0.150**
SIZE $_{t-1}$	**0.059**	**0.179**	0.015	**1.000**	**0.160**	**0.290**	**0.279**	**−0.062**	**−0.407**
LITIG $_{t-1}$	**−0.086**	**0.170**	**−0.045**	**0.031**	**1.000**	**−0.056**	**0.046**	**0.141**	**0.367**
ROA $_{t-1}$	**0.083**	**−0.248**	**−0.037**	**0.234**	**−0.116**	**1.000**	**0.191**	**−0.133**	**−0.236**
CAPX $_{t-1}$	0.001	**0.038**	**0.028**	**0.147**	**0.044**	**0.088**	**1.000**	**−0.062**	**−0.152**
RND $_{t-1}$	**−0.101**	**0.301**	**−0.285**	**−0.102**	**0.178**	**−0.568**	**−0.064**	**1.000**	**0.297**
RETVOL $_{t-1}$	**−0.090**	**0.226**	**−0.074**	**−0.386**	**0.359**	**−0.316**	**−0.073**	**0.313**	**1.000**

Note: This table presents summary statistics for variables in Equation (1). Panel A reports the descriptive statistics for the full sample. Panel B and Panel C report the descriptive statistics for the 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 the 0.1 level. See Appendix D for variable definitions. All continuous variables are winsorized at 1% and 99%.

states pass the IBBEA. A possible reason could be that banks have known their clients in the home state well before they enter a new market. Thus, firms in the home states may not need to be more conditionally conservative.

4.4. The likelihood of in-state borrowing

Thus far, I have shown that firms report less conservatively after the IBBEA is adopted and that the effect is more pronounced in states more open to interstate banking and branching. An underlying assumption in these tests is that firms borrow from local banks in their headquarter states before the passage of the IBBEA[19]. After the IBBEA is adopted, firms have more options for banks in their home states and thus have greater bargaining power relative to lenders. Therefore, for firms more likely to borrow from in-state banks, the decrease in conditional conservatism should be greater than for firms that have more access to out-of-state financing options.

The first proxy for the likelihood of in-state borrowing is whether a firm is small and young. Small and young firms are likely to borrow from local banks, as indicated in Petersen and Rajan (2002) that the average distance between small firms

[19] See Footnote 13 for more discussions.

Table 3
The Effect of the IBBEA on Conditional Conservatism.

Dependent Variable: C_SCORE_t	**(1)** Coefficient (t-value)	**(2)** Coefficient (t-value)	**(3)** Coefficient (t-value)
POST	**−0.324*** (-4.02)**		
INDEX		**−0.061*** (-2.95)**	
POSTBS			**0.131 (1.32)**
MTB $_{t-1}$	−0.946*** (-11.43)	−0.948*** (-11.45)	−1.149*** (-13.09)
LEV $_{t-1}$	1.171*** (13.95)	1.170*** (13.93)	1.382*** (15.40)
SIZE $_{t-1}$	1.579*** (17.63)	1.576*** (17.58)	1.693*** (17.74)
LITIG $_{t-1}$	−0.184** (-2.13)	−0.183** (-2.11)	−0.106 (-1.15)
ROA $_{t-1}$	0.283*** (3.64)	0.283*** (3.64)	0.373*** (4.48)
CAPX $_{t-1}$	−0.096 (-1.22)	−0.095 (-1.20)	−0.038 (-0.45)
RND $_{t-1}$	−0.210** (-2.25)	−0.210** (-2.24)	−0.229** (-2.31)
RETVOL $_{t-1}$	0.206** (2.15)	0.207** (2.15)	0.204** (2.01)
Constant	1.741*** (13.99)	1.691*** (13.73)	1.626*** (12.14)
# of Observations	8,547	8,547	6,685
Adj. R^2	0.672	0.671	0.660
State and Year FE	Yes	Yes	Yes

Note: This table presents the results of the change in conservatism based on Equation (1). The sample period is from 1993 to 1998. Columns (1) and (2) report the regression results of the full sample, while Column (3) reports the regression results of the control group only. See Appendix D for variable definitions. All control variables are ranked into deciles by year and scaled to be within [0, 1]. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

and their banks was 67.8 miles in 1990–1993[20]. These firms' financial reporting practices are more likely to be affected by the passage of the IBBEA than larger and older firms that probably have more access to out-of-state banks.

I construct two indicator variables, *YOUNG1* and *YOUNG2*, based on the decile ranks of firm age and total assets. To measure firm age more precisely, I use the founding dates of firms provided by Jay Ritter (Loughran and Ritter, 2004)[21]. A firm's asset is defined as the natural logarithm of total assets. Age and total assets are ranked into deciles within each state in the year before the passage of the IBBEA. *YOUNG1* equals one if a firm is in the bottom decile rank of age and the bottom decile rank of total assets and zero if a firm is in the top decile rank of age and the top decile rank of total assets. Because the number of observations using *YOUNG1* is small ($N = 65$), to increase the power of the test, I use *YOUNG2* that equals one if a firm is in the bottom decile rank of age and the bottom decile rank of total assets and zero for all other firms.

Panel A of Table 4 shows the distribution of total assets and age of the bottom and the top decile sample. The average of total assets for the bottom decile is e^2.159, approximately $8.66 million. The average firm age for the bottom decile is 4.75 years. For the top decile, the average of total assets is approximately $1.38 billion (e^7.229) and the average firm age is approximately 67 years.

Column (1) of Panel B, Table 4, reports multivariate regression results for the test. *YOUNG1* × *POST* is the variable of interest. The coefficient of the interaction is negative and significant (=-3.174, *t*-value = -4.26), indicating that small and young firms are more impacted by the passage of the IBBEA than older and larger firms. The results in Column (2) of Panel, Table 4, are similar using the whole sample for the test.

The second proxy I adopt is the geographic concentration of firms' operations in their headquarter states. If a firm's operations are more concentrated in their headquarter state, the firm's financial reporting choices are more likely to be

[20] Petersen and Rajan (2002) measure the distance as the mileage between the firm's main office and the branch that the firm uses the most often. They also document that, for lending relationships beginning in the 1980s, the average distance between small firms and banks is 34 miles.
[21] The data are available at https://site.warrington.ufl.edu/ritter/ipo-data/.

Table 4
Analysis Conditional on In-State Borrowing.

Panel A: Distribution of Total Assets and Age

	Mean	Std. Dev.	Min	Median	Max
Bottom Decile (N = 40)					
Total Assets	2.159	0.323	1.528	2.077	2.679
Age	4.750	1.676	1.000	5.500	6.000
Top Decile (N = 37)					
Total Assets	7.229	0.426	6.570	7.218	7.901
Age	66.973	17.459	43.000	60.000	95.000

Panel B: Regression Analysis
Dependent Variable: C_SCORE_t

	(1) Coefficient	t-value	(2) Coefficient	t-value	(3) Coefficient	t-value
YOUNG1	9.603***	(3.18)				
POST	−0.268	(-0.21)				
YOUNG1 × POST	**−4.981*** **	**(-3.25)**				
YOUNG2			1.847***	(2.98)		
POST			−0.306***	(-3.80)		
YOUNG2 × POST			**−3.174*** **	**(-4.26)**		
CONCEN					0.283**	(2.48)
POST					−0.596***	(-4.22)
CONCEN × POST					**−1.438*** **	**(-7.29)**
MTB_{t-1}	−0.454	(-0.29)	−0.939***	(-11.34)	−0.867***	(-7.92)
LEV_{t-1}	5.773***	(3.45)	1.181***	(14.09)	1.182***	(10.78)
$SIZE_{t-1}$	6.711*	(1.92)	1.569***	(17.51)	1.482***	(12.43)
$LITIG_{t-1}$	−0.774	(-0.62)	−0.180**	(-2.08)	−0.163	(-1.42)
ROA_{t-1}	1.519	(0.82)	0.288***	(3.70)	0.163	(1.55)
$CAPX_{t-1}$	−3.461**	(-2.46)	−0.103	(-1.30)	−0.137	(-1.35)
RND_{t-1}	−0.941	(-0.51)	−0.214**	(-2.29)	−0.238*	(-1.96)
$RETVOL_{t-1}$	−3.053*	(-1.93)	0.200**	(2.09)	0.199	(1.62)
Constant	−4.276	(-1.15)	1.737***	(13.97)	2.061***	(12.02)
# of Observations	65		8,547		5,135	
Adj. R^2	0.808		0.672		0.638	
Controls	Yes		Yes		Yes	
State and Year FE	Yes		Yes		Yes	

Note: This table presents the results of the change in conservatism based on Equation (1), conditional on whether the firm is small and young in the year before the IBBEA and on the percentage of a firm's operations in the headquarter state and. The sample period is from 1993 to 1998. See Appendix D for variable definitions. All control variables are ranked into deciles by year and scaled to be within [0, 1]. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

influenced by in-state banks, rather than out-of-state banks. Therefore, the effect of bank competition on firms' financial reporting would be more pronounced for these firms.

Column (3) of Panel B, Table 4, reports the multivariate regression results for the test. The geographic concentration of firms' operations is measured based on the work of Garcia and Norli (2012), who count state names from firms' 10-K annual reports as a proxy for geographic dispersion of business operations. *CONCEN* is a firm's operation in the headquarter state in the year before the passage of the IBBEA. The interaction of *CONCEN* and *POST* is the variable of interest. The coefficient of *CONCEN* × *POST* is negative and significant (=-1.438, *t*-value = -7.29), indicating that firms operating more in headquarter states report less conservatively in the post-IBBEA period than firms operating less in headquarter states. Overall, the results in Table 4 show that the change in conditional conservatism after IBBEA is more pronounced for firms that are more likely to borrow in their headquarter states.

4.5. Firms' financial constraints

In this section, I investigate whether firms' financial constraints play a role in the relation between IBBEA and conditional conservatism. On the one hand, banks' demand for conservative financial reporting is particularly high for firms with greater financial constraints because banks are exposed to a higher downside risk (Watts, 2003). When borrowers have great financial constraints, it is more costly for banks to relax their demand for conditional conservatism from borrowers even if banks' bargaining power weakens. In this case, the effect of the IBBEA on conditional conservatism will be more pronounced for less financially constrained firms. On the other hand, because banks' demand for conservative reporting is higher for more financially constrained firms, firms that are less dependent on banks are not subject to as much demand for conditional conservatism in the first place. If banks relax their demand after competition increases, the effect would be more obvious for firms with greater financial constraints.

Table 5
Analysis Conditional on Financial Constraints.

Dependent Variable: C_SCORE_t	(1)		(2)	
	Coefficient	t-value	Coefficient	t-value
LEV	1.111***	(5.09)		
POST	−0.129	(-1.39)		
LEV* × *POST	**−1.261*** **	**(-4.85)**		
KZ-INDEX			1.107***	(7.35)
POST			0.456***	(3.58)
KZ-INDEX* × *POST			**−1.604*** **	**(-8.00)**
MTB_{t-1}	−0.941***	(-11.22)	−0.884***	(-10.55)
LEV_{t-1}	0.868***	(7.17)	0.925***	(9.07)
$SIZE_{t-1}$	1.604***	(17.73)	1.633***	(17.88)
$LITIG_{t-1}$	−0.187**	(-2.13)	−0.220**	(-2.51)
ROA_{t-1}	0.300***	(3.83)	0.333***	(4.23)
$CAPX_{t-1}$	−0.098	(-1.22)	−0.103	(-1.30)
RND_{t-1}	−0.174*	(-1.83)	−0.181*	(-1.89)
$RETVOL_{t-1}$	0.178*	(1.83)	0.106	(1.09)
Constant	1.650***	(13.03)	1.256***	(9.09)
# of Observations	8,427		8,393	
Adj. R^2	0.672		0.674	
Controls	Yes		Yes	
State and Year FE	Yes		Yes	

Note: This table presents the results of the change in conservatism based on Equation (1), conditional on firms' financial constraints in the year before the IBBEA. The sample period is from 1993 to 1998. See Appendix D for variable definitions. All control variables are ranked into deciles by year and scaled to be within [0, 1]. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

I employ two measures for firms' financial constraints: *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)[22]. Firms with a higher leverage ratio or a higher KZ-Index are more likely to be financially constrained.

Column (1) of Table 5 reports the multivariate regression results using *Leverage* as the proxy for financial constraint. The coefficient on *LEV* × *POST* in Column (1) is negative and significant (=-1.261, *t*-value = -4.85), indicating that firms with higher leverage ratios report less conservatively after the passage of the IBBEA than firms with lower leverage ratios. In Column (2), the coefficient on *KZ-INDEX* × *POST* is negative and significant (=-1.604, *t*-value = -8.00), meaning that firms with higher KZ-Index report less conservatively after the IBBEA is passed in the headquarter states. Taken together, the results in Table 5 are consistent with the prediction that the effect of the IBBEA on conditional conservatism is more pronounced for firms with greater financial constraints.

4.6. 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. The level of conditional conservatism is affected by monitoring mechanisms. Specifically, firms with stronger governance in place tend to be more conditionally conservative than firms subject to weaker governance (García Lara et al., 2009; Ramalingegowda and Yu, 2012). The effect of the IBBEA on conditional conservatism may be subject to the impact of monitoring mechanisms on conditional conservatism. Following prior research, I use two proxies for monitoring mechanisms: analyst following and institutional ownership. Chen et al. (2015) show that analysts play an important role in corporate governance. Therefore, firms with more analyst following are likely to report more conservatively than firms with less analyst following. Ramalingegowda and Yu (2012) find that firms with higher institutional ownership report more conservatively. With greater analyst following and institutional ownership, firms may not change the level of conditional conservatism, even if banks relax the demand for conservative reporting. Therefore, the decrease in conditional conservatism after the passage of the IBBEA may be more pronounced for firms with less analyst following or lower institutional ownership. In contrast, banks may be willing to relax the demand for firms with more analyst following and institutional ownership, since these firms are subject to less information asymmetry.

Table 6 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 before the IBBEA. Institutional ownership is calculated from Thomson Reuters Stock Ownership. As

[22] 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.

Table 6
Analysis Conditional on External Monitoring.

Dependent Variable: C_SCORE_t				
	(1)		**(2)**	
	Coefficient	t-value	Coefficient	t-value
ANALYST	−0.283***	(-8.90)		
POST	−1.250***	(-13.42)		
ANALYST* × *POST	**0.683*** **	**(18.81)**		
IO			−1.062***	(-7.60)
POST			−1.423***	(-14.03)
IO* × *POST			**2.937*** **	**(17.26)**
MTB_{t-1}	−0.856***	(-10.31)	−0.874***	(-10.15)
LEV_{t-1}	1.130***	(13.62)	1.146***	(13.71)
$SIZE_{t-1}$	1.715***	(13.44)	1.602***	(13.61)
$LITIG_{t-1}$	−0.140	(-1.63)	−0.199**	(-2.30)
ROA_{t-1}	0.289***	(3.75)	0.254***	(3.27)
$CAPX_{t-1}$	−0.066	(-0.83)	−0.099	(-1.25)
RND_{t-1}	−0.226**	(-2.43)	−0.193**	(-2.05)
$RETVOL_{t-1}$	0.181*	(1.90)	0.170*	(1.77)
Constant	1.972***	(15.76)	2.107***	(16.69)
# of Observations	8,415		8,384	
Adj. R^2	0.685		0.683	
Controls	Yes		Yes	
State and Year FE	Yes		Yes	

Note: This table presents the results of the change in conservatism based on Equation (1), conditional on firms' financial constraints in the year before the IBBEA. The sample period is from 1993 to 1998. See Appendix D for variable definitions. All control variables are ranked into deciles by year and scaled to be within [0, 1]. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

with prior tests, the two variables are calculated in the year before the passage of the IBBEA. The positive and significant coefficient on *ANALYST* × *POST* (=0.683, *t*-value = 18.81) in Column (1) indicates that firms with higher analyst following report more conservatively in the post-IBBEA period than firms with lower analyst following. Column (2) reports that the coefficient on *IO* × *POST* is positive and significant (=2.937, *t*-value = 17.26), meaning that compared to firms with lower institutional ownership, firms with greater institutional ownership report more conservatively in the post-IBBEA period. In other words, the results in Table 6 show that the decrease in conditional conservatism is more pronounced for firms subject to less external monitoring.

Taken collectively, the cross-sectional tests in this study show that the increase in bank competition has more pronounced effects on firms subject to seemingly more severe information asymmetry. Breuer et al. (2018) argue that increased disclosure reduces the cost of information acquisition and the advantage of relationship banking, allows more transactional banking, and fosters bank competition. In contrast, my findings indicate that given intensified bank competition, banks may not demand more disclosure even if they do not possess an information advantage when they enter a new market.

4.7. Alternative measures of conditional conservatism

This study uses the *C_SCORE* to measure conditional conservatism. However, there are several other conservatism measures available. In this section, I briefly discuss these alternative conservatism measures and conduct additional tests to evaluate the robustness of my findings to these alternative measures.

4.7.1. The debate on the Basu (1997) conditional conservatism measure

Since Basu (1997), the asymmetric timeliness (AT) measure has been widely used in the literature, but it is also controversial. Dietrich et al. (2007), Givoly et al. (2007), and Patatoukas and Thomas (2011) argue that the AT measure is potentially biased[23], while Ball et al. (2013) and Collins et al. (2014) revise the AT measure to mitigate biases discussed elsewhere[24]. Patatoukas and Thomas (2016) presented the upward bias in the revised AT measures[25] and Dutta and Patatoukas (2017) further develop a conditional conservatism measure to address this bias. However, Harakeh et al. (2019) show that in a time-series

[23] Dietrich et al. (2007) argue that the coefficients of good and bad news in the AT measure can be unequal absent conditional conservatism. Givoly et al. (2007) show that the AT measure is sensitive to factors and policies unrelated to conditional conservatism, indicating potential measurement error. Patatoukas and Thomas (2011) replace current earnings in the AT measure with lagged earnings and find an association between lagged earnings and current conditional conservatism. They conclude that such bias is due to the scale in the dependent variable of the AT measure.

[24] Ball et al. (2013) revise the AT measure by including firm fixed effects. Collins et al. (2014) revise it by removing the cash flow component from earnings. Both studies document that such revisions of the AT measure effectively mitigate biases discussed elsewhere, such as by Givoly et al. (2007) and Patatoukas and Thomas (2011).

[25] Patatoukas and Thomas (2016) show that there is still upward bias in the revised AT measures since the performance variable on the left-hand side of the AT measure, such as earnings or accruals, is related to the return variable on the right-hand side of the measure.

exogenous setting, such as the passage of the Sarbanes-Oxley (SOX) Act, using the AT measure and the measure developed by Dutta and Patatoukas (2017) produce similar inferences.

4.7.2. Basu's (1997) interacted AT measure

Basu's (1997) interacted AT measure has been adopted in many prior studies that investigate the effect of a staggered regulatory change on conditional conservatism (Basu and Liang, 2019; Jayaraman and Shivakumar, 2013; Khurana and Wang, 2019; Manchiraju et al., 2020). The AT measure developed in Basu (1997) is as follows:

$$NI_t = \beta_0 + \beta_1 NEG_t + \beta_2 RET_t + \beta_3 NEG_t * RET_t + \varepsilon_t \quad (5)$$

See Appendix D for variable definitions. 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 versus good news, thus representing the level of conditional conservatism.

I follow prior research and extend Eq. (5) by incorporating *POST* and the Branching Restrictiveness Index *INDEX* in the model as follows:

$$NI_t = \beta_0 + \beta_1 NEG_t + \beta_2 RET_t + \beta_3 NEG_t * RET_t + \beta_4 IV + \beta_5 IV * NEG_t + \beta_6 IV * RET_t + \beta_7 IV * NEG_t * RET_t + \gamma \sum Controls_{t-1} + \alpha_s + \alpha_t + \varepsilon_t \quad (6)$$

IV stands for *POST* or *INDEX*, as defined in Eqs. (3) and (4), respectively. *NI*, *RET*, and *NEG* are defined as in Eq. (5). All control variables as in Eq. (4) are included. Each control variable interacts with *NEG*, *RET*, and *NEG* * *RET*. In Eq. (6), β_7, the coefficient on *IV* * NEG_t * RET_t, represents the change in the incremental timeliness of bad news recognition in the post-IBBEA period. If borrowers' conditional conservatism decreases after the passage of the IBBEA and the effect is more pronounced in states more open to interstate banking and branching, the coefficient on the main interaction variable *IV* * NEG_t * RET_t, β_7, will be negative.

4.7.3. AT measures based on decomposed earnings

Next, I address the concern with cash flow asymmetry in Basu's (1997) conservatism measure. Collins et al. (2014) argue that the asymmetric recognition of good and bad news is not reflected in the recognition of cash flow. Therefore, when using earnings as the dependent variable to measure conditional conservatism, the results can be biased by the noise induced by cash flow asymmetry. Following Collins et al. (2014), I decompose NI_t in Eq. (6) into accruals (ACC_t) and operating cash flow (CFO_t). See Appendix D for variable definitions. Then I estimate Eq. (6) using ACC_t and CFO_t as the dependent variables, respectively. The coefficient on *IV* * NEG_t * RET_t, β_7, is expected to be negative in accruals (ACC_t) but not in operating cash flow (CFO_t).

4.7.4. Conservatism ratio

Because *C_SCORE* is calculated based on the AT measure, to avoid potential issues of the AT measure discussed earlier, I construct another firm-year conservatism measure, Conservatism Ratio (*CR*), following Callen et al. (2010), Callen and Segal (2010), and García Lara et al. (2016). Callen et al. (2010) use the vector autoregressive model (VAR) to estimate unexpected earnings news by regressing current earnings news on stock returns, earnings, and the book-to-market ratio in the previous year. The residual of the model is estimated unexpected earnings news in the current period. Then *CR* is calculated as the unexpected earnings news in the current period divided by total earnings news. *CR* captures the amount of earnings shock incorporated into unexpected earnings news in the current period (Callen et al. 2010). Following Callen et al. (2010), I keep positive *CR* only. Following García Lara et al. (2016), I account for the look-ahead bias by employing a 25-year rolling window when estimating the unexpected earnings news. A greater *CR* indicates that the firm reports more conservatively.

Using *CR* as a dependent variable, I estimate the following regression:

$$CR_t = \beta_0 + \beta_1 IV + \gamma \sum Controls_{t-1} + \alpha_s + \alpha_t + \varepsilon_t \quad (7)$$

The definition of *IV* is the same as in Eq. (6). The coefficient on *IV*, β_1, captures the change in conditional conservatism given an increase in bank competition.

4.7.5. Results using alternative conservatism measures

Table 7 presents the empirical results using alternative conservatism measures. Panel A, Column (1), reports the multivariate regression results of Eq. (6) in which conditional conservatism is measured using Basu's (1997) AT measure. In Columns (2) and (3), I replace NI_t, the dependent variable in Eq. (6), with ACC_t and CFO_t, respectively. Panel B of Table 7 presents the results of estimating Eq. (7). Overall, my results continue to hold using these alternative conservatism measures, except for the augmented Basu (1997) model in which the dependent variable is ACC_t and the effect of the IBBEA is measured using *POST*. The findings also indicate that the effect of the IBBEA on conditional conservatism is more pronounced in states that impose fewer restrictions, consistent with H2.

Table 7
Alternative measures of Conditional Conservatism.

Panel A: Alternative Conservatism Measure following Basu (1997) and Collins et al. (2014)

	(1) DV = NI_t		(2) DV = ACC_t		(3) DV = CFO_t	
	IV = *POST*	IV = *INDEX*	IV = *POST*	IV = *INDEX*	IV = *POST*	IV = *INDEX*
	Coefficient (t-value)		Coefficient (t-value)		Coefficient (t-value)	
NEG_t	−0.065***	−0.067***	−0.040	−0.042	−0.016	−0.018
	(-3.15)	(-3.23)	(-1.50)	(-1.58)	(-0.61)	(-0.72)
RET_t	0.025	0.023	−0.073***	−0.078***	0.089***	0.096***
	(1.33)	(1.18)	(-2.98)	(-3.19)	(3.82)	(4.09)
$NEG_t \times RET_t$	0.144***	0.144***	0.188***	0.204***	−0.033	−0.048
	(2.63)	(2.62)	(2.68)	(2.91)	(-0.50)	(-0.71)
IV	−0.001	−0.001	0.007	0.001	−0.013**	−0.002
	(-0.15)	(-0.89)	(1.16)	(0.51)	(-2.21)	(-1.01)
$IV \times NEG_t$	0.007	0.004*	−0.002	−0.000	0.009	0.003
	(0.89)	(1.70)	(-0.19)	(-0.14)	(0.97)	(1.28)
$IV \times RET_t$	−0.007	0.001	−0.005	0.001	0.001	0.001
	(-0.93)	(0.58)	(-0.52)	(0.23)	(0.08)	(0.20)
$IV \times NEG_t \times RET_t$	**−0.053*** **	**−0.013** **	**−0.028**	**−0.014** **	**−0.023**	**0.001**
	(-2.82)	**(-2.46)**	**(-1.18)**	**(-1.99)**	**(-1.01)**	**(0.19)**
Constant	0.010	0.012	−0.033**	−0.029*	0.042***	0.040***
	(0.423)	(0.99)	(-2.10)	(-1.84)	(2.80)	(2.64)
# of Observations	8,547	8,547	8,531	8,531	8,531	8,531
Adj. R^2	0.288	0.287	0.147	0.150	0.241	0.243
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Alternative Conservatism Measure following Callen et al. (2010) and García Lara et al. (2016)
Dependent Variable: CR_t

	(1) IV=*POST*		(2) IV=*INDEX*	
	Coefficient	t-value	Coefficient	t-value
IV	−0.292**	(-2.03)	−0.073**	(-1.97)
Constant	1.571***	(6.99)	1.541***	(6.94)
# of Observations	5,374		5,374	
Adj. R^2	0.014		0.014	
Controls	Yes		Yes	
State and Year FE	Yes		Yes	

Note: Panel A presents the results of the change in conservatism, using net income (Basu, 1997) and accruals and cash flow (Collins et al., 2014) as dependent variables. Panel B presents the results of the change in conservatism using the conservatism ratio (Callen et al., 2010, García Lara et al., 2016) as the dependent variable. The sample period is from 1993 to 1998. Control variables and their interactions with *NEG* and *RET* are included in Panel A. Control variables as in Table 3 are included in Panel B. See Appendix D for variable definitions. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

4.8. Robustness checks

4.8.1. Dynamic effects of the IBBEA

In this section, I examine the dynamic effects of the IBBEA on conditional conservatism. 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 Eq. (1) is replaced with *IBBEA(-n)* and *IBBEA(n)*. If the decline in conditional conservatism is a time-trend effect or relates to factors other than the passage of the law, then the effects of the IBBEA on conditional conservatism should precede passage. In other words, if the passage of the IBBEA indeed leads to 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. Because most states passed the IBBEA in 1996 and 1997, the sample period in this test is extended to 2001 to capture the dynamic effect of the IBBEA three years after the law was passed. For brevity, I only report the coefficients of the variables of interest. Only the coefficients on *IBBEA(1)* and *IBBEA(2)* are negative and significant at the 1% level, indicating that firms report less conservatively only after the passage of the IBBEA.

Furthermore, the negative and insignificant coefficient on *IBBEA(3)* indicates that the impact of IBBEA on conditional conservatism diminishes after two years since the adoption. This result reveals that banks tend to "lowball" borrowers by relaxing the demand for conservative reporting only during the first two years when they want to attract clients in a more

Table 8
Robustness Tests.

Panel A: Dynamic Effects of IBBEA
Dependent Variable: C_SCORE_t

	[-3, 3]	
	Coefficient	t-value
IBBEA (-3)	−0.029	(-0.30)
IBBEA (-2)	−0.114	(-1.40)
IBBEA (-1)	−0.077	(-0.88)
IBBEA (0)	−0.036	(-0.43)
IBBEA (1)	−0.325***	(-4.05)
IBBEA (2)	−0.324***	(-4.53)
IBBEA (3)	−0.010	(-0.16)
Constant	1.130***	(11.56)
# of Observations	12,941	
Adj. R^2	0.716	
Controls	Yes	
State and Year FE	Yes	

Panel B: Placebo Tests
Dependent Variable: C_SCORE_t

	(1) Coefficient (t-value)	(2) Coefficient (t-value)	(3) Coefficient (t-value)
POST	**0.066**	**−0.058**	**−0.042**
	(0.79)	**(-0.68)**	**(-0.36)**
MTB $_{t-1}$	−0.948***	−0.948***	−0.949***
	(-11.44)	(-11.44)	(-11.44)
LEV $_{t-1}$	1.167***	1.167***	1.167***
	(13.89)	(13.89)	(13.90)
SIZE $_{t-1}$	1.575***	1.576***	1.575***
	(17.56)	(17.58)	(17.57)
LITIG $_{t-1}$	−0.181**	−0.182**	−0.182**
	(-2.08)	(-2.11)	(-2.10)
ROA $_{t-1}$	0.282***	0.280***	0.281***
	(3.63)	(3.60)	(3.62)
CAPX $_{t-1}$	−0.094	−0.094	−0.094
	(-1.19)	(-1.19)	(-1.19)
RND $_{t-1}$	−0.210**	−0.211**	−0.210**
	(-2.24)	(-2.25)	(-2.24)
RETVOL $_{t-1}$	0.205**	0.204**	0.205**
	(2.14)	(2.13)	(2.13)
Constant	1.584***	1.682***	1.670***
	(11.75)	(11.78)	(10.20)
# of Observations	8,547	8,547	8,547
Adj. R^2	0.671	0.671	0.671
State and Year FE	Yes	Yes	Yes

Note: Panel A presents the results on the dynamic effects of the IBBEA on conditional conservatism based on Equation (1). The sample period in this table is from 1993 to 2001. Since most states passed the IBBEA in 1996 and 1997, the sample period in this table is extended to capture the dynamic effect of the IBBEA three years after the act was passed. Control variables as in Table 3 are included. See Appendix D for variable definitions. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.
Note: Panel B presents the results on the change in conservatism based on Equation (1), if the effective date of the IBBEA was N years earlier than the actual effective date, N = 1, 2, and 3 in columns (1), (2), and (3), respectively. The sample period is from 1993 to 1998. All control variables are ranked into deciles by year and scaled to be within [0, 1]. See Appendix D for variable definitions. Standard errors are clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

competitive market. However, banks also face the risk of exiting the market if their own performance is poor (Stiroh and Strahan, 2003). The results in Panel A of Table 8 indicate that the cost of weakened monitoring seems to exceed the benefits in a few years.

The cross-sectional results in Section 4 show that the effect of the IBBEA is more pronounced on small and young firms and, relatedly, firms with fewer analysts following and lower institutional ownership. These firms are likely to have greater information asymmetry before the IBBEA. If banks do not impose more restrictions on borrowers' financial reporting, they

will face a greater potential downside risk in the long run. Therefore, banks eventually tighten the covenants, intensify monitoring, and demand more conservative financial reporting after they have established business relationships in a new market.

4.8.2. Placebo tests

To enhance the credibility of the results, I conduct a placebo test following Burks et al. (2018). In this test, I re-estimate Eq. (1) assuming that the pseudo year of adopting the IBBEA in each state is N (N = 3, 2, and 1) 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 variable *POST* 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* are insignificant, indicating that there is no significant change in conditional conservatism in the pseudo post-IBBEA periods. These results support the argument that the relation between the IBBEA and conditional conservatism is not spurious.

5. Conclusions

Using the staggered passage of the IBBEA as a shock that increases state-level bank competition in the United States, this study investigates the impact of increased bank competition on accounting conservatism in clients' financial reporting. The IBBEA permitted full interstate banking and branching, which had been greatly restricted previously. Banks could expand their business across state borders by acquisitions or de novo branching after a state adopted the IBBEA's provisions. With new banks entering the market, state-level competition among banks increased significantly (Burks et al., 2018; Cornaggia et al., 2015; Dick, 2006; Rice and Strahan, 2010; Zarutskie, 2006).

Prior studies document that information from clients' financial statements is essential to lenders when making lending decisions (Ahmed et al., 2002; Christensen and Nikolaev, 2012; Watts, 2003; Zhang, 2008). The contracting demand from banks also affects their clients' financial reporting practices. Facing significant downside risk, banks require their clients to recognize bad news more promptly. In other words, banks demand conservative financial reporting from borrowers.

An increase in bank competition after deregulation weakens banks' bargaining power in credit negotiations, making them more likely to relax their demand for conservatism. Prior studies find that banks design contracts with fewer and less intense covenants after the passage of the IBBEA (Jiang, 2016; Xia, 2018), so borrowing firms are subject to less monitoring from banks. Therefore, I predicted that firms are likely to report less conservatively after the IBBEA is passed in their headquarter 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 more likely to borrow from in-state banks, firms with higher financial constraint, and firms subject to less external monitoring.

My findings complement those of Gormley et al. (2012) and contribute to the literature on how changes in lenders' characteristics affect borrowers' conditional conservatism. To the extent that the increase in bank competition after deregulation may impact borrowing firms' financial reporting, this study should be of interest to regulators. The findings highlight the necessity of evaluating potential unintended effects of regulatory changes in the banking industry and intensifying monitoring of banks and their clients after deregulation takes effect.

6. Data availability

The data that support the findings of this study are available from the resources identified in the study.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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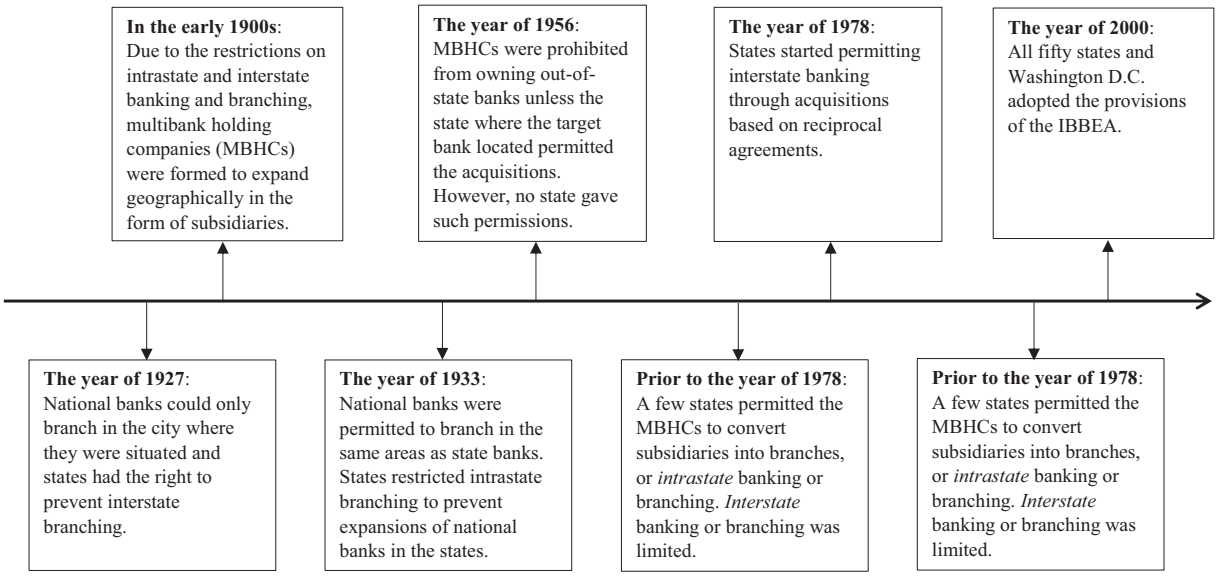
Appendix A. Timeline of banking and branching deregulation



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 passage of the IBBEA by state

Panel A: The Effective Dates of the IBBEA and the Branching Restrictiveness Index

State	Effective Date	Branching Restrictiveness Index	State	Effective Date	Branching Restrictiveness Index
AK	01/01/1994	2	MT	09/29/1995	4
AL	05/31/1997	3	NC	07/01/1995	0
AR	06/01/1997	4	ND	05/31/1997	3
AZ	09/01/1996	3	NE	05/31/1997	4
CA	09/28/1995	3	NH	06/01/1997	4
CO	06/01/1997	4	NJ	04/17/1996	1
CT	06/27/1995	1	NM	06/01/1996	3
DC	06/13/1996	0	NV	09/29/1995	3
DE	09/29/1995	3	NY	06/01/1997	2
FL	06/01/1997	3	OH	05/21/1997	0
GA	06/01/1997	3	OK	05/31/1997	4
HI	06/01/1997	3	OR	07/01/1997	3
IA	04/04/1996	4	PA	07/06/1995	0
ID	09/29/1995	3	RI	06/20/1995	0
IL	06/01/1997	3	SC	07/01/1996	3
IN	06/01/1997	0	SD	03/09/1996	3
KS	09/29/1995	4	TN	06/01/1997	3
KY	06/01/1997	4	TX	08/28/1995	4
LA	06/01/1997	3	UT	06/01/1995	2
MA	08/02/1996	1	VA	09/29/1995	0
MD	09/29/1995	0	VT	05/30/1996	2
ME	01/01/1997	0	WA	06/06/1996	3
MI	11/29/1995	0	WI	05/01/1996	3
MN	06/01/1997	3	WV	05/31/1997	1
MO	09/29/1995	4	WY	05/31/1997	3
MS	06/01/1997	4			

Appendix B (*continued*)

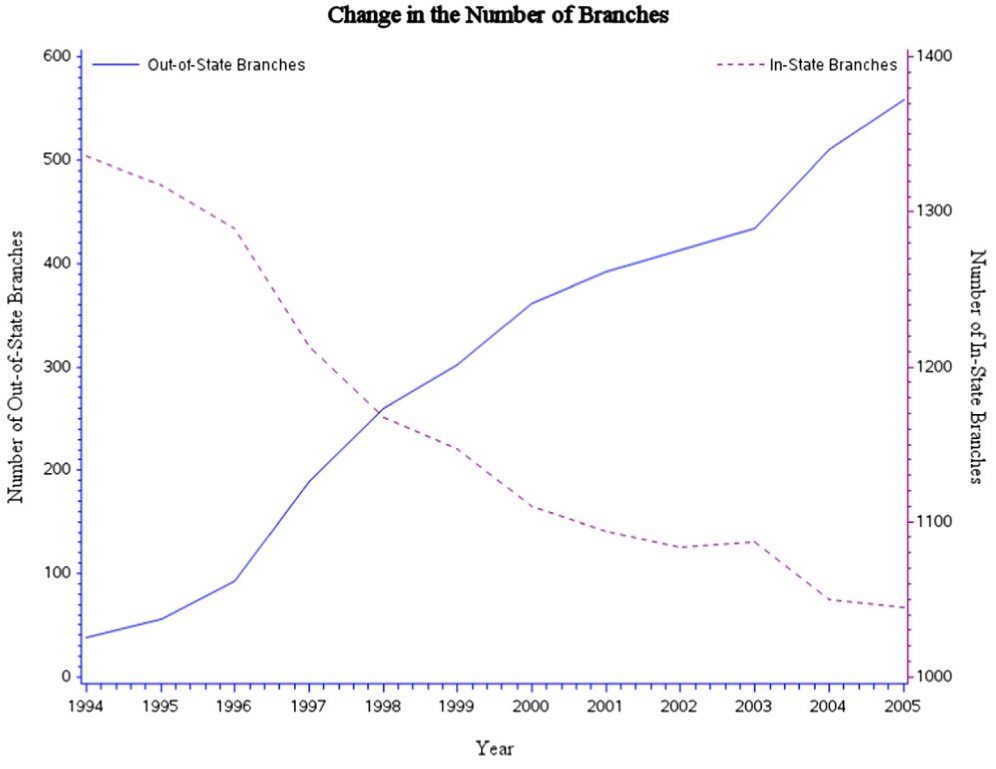
Panel B: Summary of the Branching Restrictiveness Index					
	Index = 0	Index = 1	Index = 2	Index = 3	Index = 4
Number of States	10	4	4	21	12

Note: This table is obtained from Table 1 of Rice and Strahan (2010). Panel A reports the effective dates of the Riegle-Neal Interstate Banking and Branching Efficiency Act (IBBEA) and the branching restrictiveness by state. The Branching Restrictiveness Index is added one if states enacted one of the four restrictions denoted in Footnote 3. A higher Index means the state enacted more restrictions when the IBBEA was passed. Panel B reports the summary of the Branching Restrictiveness Index.

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 in 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 the Federal Deposit Insurance Corporation (FDIC).



Note: This figure is created based on the average number of branches per state. The branch data are obtained from the Summary of Deposits from the Federal Deposit Insurance Corporation (FDIC). See Fig. 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*.
ANALYST	Natural logarithm of the number of analysts following a firm.
CAPX	Capital expenditure scaled by total assets.
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*.
CONCEN	Operation concentration, the percentage of a firm's operation in the headquarter state created by Garcia and Norli (2012).
CR	Conservatism Ratio, calculated following Callen et al. (2010) and Callen and Segal (2010) and corrected for look-ahead bias following García Lara et al. (2016).
C_SCORE	Conditional conservatism measure calculated following Khan and Watts (2009), multiply by 100.
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 Appendix B. *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*.
IO	The percentage of institutional ownership of a firm.
KZ-INDEX	A measure of financial constraint calculated following Kaplan and Zingales (1997) and Lamont et al. (2001).
LEV	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	Market-to-book ratio, calculated as the market value of common equity deflated by the 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 has passed the IBBEA by year *t-1* and zero otherwise.
POSTBS	An indicator variable that equals one if a bordering state passed the IBBEA at least 12 months ago; zero if none of the bordering states has passed the IBBEA.
RET	Cumulative buy-and-hold returns from nine months before the fiscal year end to three months after the fiscal year end.
RETVOL	The return volatility of the fiscal year calculated using CRSP monthly stock returns.
RND	R&D expenditure scaled by total assets.
ROA	Net income scaled by total assets.
SIZE	The market value of common equity.
YOUNG1	An indicator variable that equals one if a firm is in the bottom decile rank of age and the bottom decile rank of the asset; zero if a firm is in the top decile rank of age and the top decile rank of the asset. A firm's age is defined as the number of years since the founding date provided by Jay Ritter. A firm's asset is defined as the natural logarithm of total assets. Age and total assets are ranked within each state in the year before the passage of the IBBEA.
YOUNG2	An indicator variable that equals one if a firm is in the bottom decile rank of age and the bottom decile rank of the asset; zero otherwise. A firm's age is defined as the number of years since the founding date provided by Jay Ritter. A firm's asset is defined as the natural logarithm of total assets. Age and total assets are ranked within each state in the year before the passage of the IBBEA

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