CREDITOR RIGHTS, CONFLICT OF INTEREST AMONG CREDITORS, AND BORROWERS' ACCOUNTING CONSERVATISM: EVIDENCE FROM ANTI-RECHARACTERIZATION LAWS

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Creditor Rights, Conflict of Interest among Creditors, and Borrowers' Accounting Conservatism: Evidence from Anti-Recharacterization Laws

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ABSTRACT

Using the staggered adoption of anti-recharacterization laws (ARL) as an exogenous shock to creditor rights, I study the effects of creditor rights on borrowers' accounting conservatism. By denying the recharacterization of securitized assets as collateral for secured debt, ARL protects securitization creditors at the expense of non-securitization creditors such as banks. Given the conflict of interest among these creditors, I argue that non-securitization creditors may demand more conservative accounting in response to the decreasing credit rights. Consistent with my prediction, I find an increase in borrowers' reporting conservatism following the adoption of ARL. To further support my demand-side argument, I provide evidence that the effect of ARL on accounting conservatism is stronger for borrowers with higher credit risks. Moreover, I find a stronger effect for borrowers with better corporate governance, suggesting that these borrowers are more likely to meet the demand by supplying accounting conservatism. Overall, my study establishes a link between creditor rights and borrowers' accounting conservatism and adds to the literature by offering new insights from both perspectives of demand and supply of accounting conservatism.

Keywords: Creditor rights; Conflict of interest; Accounting conservatism; Anti-recharacterization; Assets securitization

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1. Introduction

This paper investigates whether borrowers change their accounting conservatism when the creditor rights of non-securitization creditors are constrained. Since studies show that the contractual environment is an important determinant of borrowers' accounting conservatism (Watts and Zimmerman, 1986; Watts, 2003a), an altered contractual environment can potentially induce changes in accounting conservatism (Martin and Roychowdhury, 2015). Creditor rights have a strong impact on the contractual environment including contractual parties' payoffs and incentives (e.g., La Porta et al., 1997, 1998; Esty and Megginson, 2003; Qian and Strahan, 2007; Bae and Goyal, 2009). In this study, the altered contractual environment is a re-allocation of creditor rights between non-securitization (e.g., commercial banks) and securitization creditors. Using the staggered adoption of anti-recharacterization laws (ARL) across different states in U.S., wherein the creditor rights are switched from traditional creditors to securitization creditors, I study the causal effect of creditor rights on accounting conservatism.

Many early studies use cross-country settings to examine the effect of creditor rights on corporate decisions[1]. As a country-level institutional factor, creditor rights can remain unchanged for a long period of time. The international setting is not without problems because it is often hard to fully control for other country-level institutional differences. To circumvent the above limitation, recent literature exploits the staggered adoption of state-level ARL in U.S. as exogenous shocks to creditor rights and documents causal effects of credit risk on innovation (Mann, 2018), plant productivity (Ersahin, 2020), and cash holding and payouts (Favara, Gao, and Giannetti, 2021). I extend these studies to accounting policies in the case of asset

[1] For example, prior literature uses international settings to study the effects of creditor rights on corporate debt financing (Esty and Megginson, 2003; Safavian and Sharma, 2007; Bae and Goyal, 2009; Boubakri and Ghouma, 2010; Benmelech and Bergman, 2011; Cho et al., 2014; Gu and Kowalewski, 2016; Qi, Roth, and Wald, 2017; Moro, Maresch, and Ferrando, 2018), corporate investment (Seifert and Gonenc, 2012; Qi, Roth, and Wald, 2017), cash holding (Kyröläinen, Tan, and Karjalainen, 2013; Yung and Nafar, 2014), and dividend payout policy (Brockman and Unlu, 2009).

securitization and investigate whether credit rights affect accounting conservatism of the borrowers—an important accounting policy that is affected by the contractual environment, which in turn, relies on creditor rights.

Asset securitization can be briefly described as follows: a borrower first sells the assets to a special purpose entity (SPV) which securitizes the assets and sells asset-backed securities to investors (i.e. the creditors). The new borrower of the asset-backed securities becomes the special purpose entity, which is owned by the original borrower.[2] According to the US Bankruptcy Code Chapter 11, collateral underlying secured lending is subject to automatic stay when borrowers file for bankruptcy. However, asset securitization can avoid the automatic stay of underlying assets with the original borrower if the original borrower files for bankruptcy, so the special purpose entity can still stay solvent. This advantage increases the attraction of asset securitization to creditors investing in asset-backed securities. However, this advantage is not guaranteed in the absence of ARL. Without ARL, judges have the discretion to re-characterize the securitized assets as collateral so that the underlying assets are subject to automatic stay with the original borrowers. To prevent such discretion, several states in the US have adopted ARL. The adoption of ARL enhances creditor rights for the buyers of asset-backed securities (hereinafter "securitization creditors") by allowing them to seize the collateral directly and exclusively in the SPV. As a result, the creditor rights of other non-securitization creditors (mainly commercial banks) in the original borrower decrease as the assets that they can repossess decrease. I investigate whether such a transfer of creditor rights from non-securitization creditors to securitization affects the borrower's accounting conservatism.

It is *ex ante* unclear whether and how ARL affect borrowers' accounting conservatism. On the one hand, borrowers' regular or non-securitization creditors, such as banks, may

[2] Asset securitization is a complex transaction involving various parties. I detail the asset securitization process and the background of ARL adoption in subsection 2.1.

demand a higher degree of accounting conservatism following the adoption of ARL.[3] Without ARL, the securitized assets may be recharacterized as part of borrowers' bankruptcy estate. This may be beneficial for all stakeholders including banks, bondholders, and shareholders, as bankrupt borrowers with more valuable assets on hand may have a higher chance to emerge from bankruptcy. Under the ARL regime, borrowers' bankruptcy estate decreases, so does the likelihood of borrowers' emergence from bankruptcy. In response to the additional risk caused by the adoption of ARL for the debtholders, they may demand more conservative accounting because borrowers' accounting conservatism can protect them by recognizing losses more timely and facilitating monitoring effectiveness (Watts, 2003a; Ball and Shivalumar, 2005).

On the other hand, borrowers may become less willing to supply conservative accounting after the adoption of ARL. The major motive for providing conservatism is often to obtain external financing at a lower cost. Asset securitization is an alternative form of financing and the adoption of ARL can decrease its financing costs. This is because ARL enhance the attractiveness of asset securitization by guaranteeing the bankruptcy remoteness and borrowers can enjoy a significantly lower cost of capital (Janger, 2003).[4] Given the increased attractiveness of asset securitization, borrowers are less likely to be dependent on normal sources of financing, such as bank debt and bond issuance. As managers generally prefer less conservative accounting for their own benefits and firms can switch to asset securitization, borrowers may become less responsive to the non-securitization creditors' demand for conservative accounting. To the extent that ARL reduce borrowers' dependence on external financing from non-securitization creditors, one might expect borrowers to be less likely to supply conservative accounting after the adoption of ARL. Given the tension between

[3] Given the conflict of interest among creditors, I distinguish between a borrower's securitization creditors and non-securitization creditors. Securitization creditors refer to the buyers/investors of asset-backed securities. Non-securitization creditors refer to traditional debtholders such as banks and bondholders. For ease of exposition, I use "non-securitization creditors" and "debtholders" interchangeably throughout the paper.

[4] Chu (2020) finds that financing through asset securitization makes leases less attractive following the adoption of ARL.

non-securitization creditors' higher demand for and borrowers' lower supply of accounting conservatism after the adoption of ARL, my exploration is an empirical question.

To test the effect of ARL adoption on borrowers' accounting conservatism, I conduct a difference-in-differences (DID) analysis based on a panel of US-listed firms during the period 1994-2009. Following Ersahin (2020), I define treatment firms as those incorporated or headquartered in the four states that have adopted ARL during my sample period, i.e., Texas, Louisiana, Alabama, and Delaware.[5] Firms incorporated and headquartered in other states are all considered as my control sample. Following prior literature (e.g., Aier, Chen, and Pevzner, 2014; Manchiraju, Pandey, and Subramanyam, 2020), I augment Basu's (1997) asymmetric timeliness model to implement a DID design. I find that, compared with control firms, the reporting conservatism of treatment firms increases significantly following the adoption of ARL. I also show that my results are robust to alternative clustering schemes, fixed effects, samples and approaches for testing accounting conservatism. These results suggest that, following the adoption of ARL, the effect of non-securitization creditors' demand outweighs that of borrowers' lower supply in respect of changing reporting conservatism.

Next, I check the parallel trend and test the dynamic effects of ARL on firms' accounting conservatism. I find no significant difference in the extent of conservatism between treatment and control firms in the pre-period of ARL adoption. This finding lends support to the parallel trend assumption of using the DID model and suggests that the adoption of ARL is exogenous to my sample firms. I also observe the dynamic effects of ARL on firms' accounting conservatism: treatment firms start to increase reporting conservatism in the same year of ARL adoption, and their accounting conservatism remains at a higher level in the subsequent years. To mitigate the concern that confounding factors like local economic shocks may drive my

[5] Ersahin (2020) explains that ARL apply to a borrower's "location" which can refer to its incorporation state and/or headquarters state.

results, I conduct a placebo test by falsely treating the neighboring states of ARL adoption states. These confounding factors are likely to affect firms in the neighboring states as well. If that is the case, I should be able to find similar results. However, I do not find any significant change in accounting conservatism for firms in the falsely treated neighboring states.

My baseline results are consistent with non-securitization creditors' demand for more conservative accounting, because ARL reduce the recoverability of the claim by non-securitization creditors, such as banks, who may monitor borrowers for more conservative accounting. To shed light on this channel, I conduct several cross-sectional tests. First, I focus on the moderating role of borrowers' credit risk. Non-securitization creditors' payoffs motivate them to continuously monitor borrowers' ability to meet the repayment schedule. Non-securitization creditors are generally not concerned about high-quality borrowers because these borrowers can generate stable future cash flow to repay their debt. By contrast, they are more concerned about borrowers with high credit risk. ARL can result in losses for non-securitization creditors only when their borrowers file for bankruptcy. Therefore, given that borrowers with high risk are more likely to default and file for bankruptcy in an extreme case, I expect the effect of ARL on accounting conservatism to be more pronounced for borrowers with higher credit risk. Consistent with my expectation, I find a stronger effect for financially constrained borrowers and borrowers closer to default.

The second cross-sectional test focuses on borrowers' corporate governance. According to the basic economic principle, both demand- and supply-side forces can shape borrowers' accounting practices. While I argue that non-securitization creditors demand for more conservative accounting due to the shock of ARL, the extent of conservatism also depends on borrowers' willingness to meet the demand. I argue that, in response to non-securitization creditors' demand for more conservative accounting, borrowers with better corporate governance are more likely to supply it. Corporate governance is an important mechanism for

implementing accounting conservatism (García Lara, García Osma and Penalva, 2009; Aier, Chen, and Pevzner, 2014). Prior literature suggests that reporting conservatism can benefit not only debtholders but also shareholders by increasing firm value (Watts, 2003). Better-governed firms care more about firm value as a whole and the balance of interest among stakeholders (Gompers, Ishii, and Metrick, 2003; Klock, Mansi, and Maxwell, 2005). Therefore, I expect the effect of ARL on accounting conservatism to be stronger for better-governed firms. Using various measures of corporate governance, I find consistent evidence that better-governed borrowers experience a larger increase in the extent of conservatism following the ARL adoption, suggesting that these borrowers are more likely to meet non-securitization creditors' demand for accounting conservatism.

My study makes several contributions to the literature. First, my study extends the literature that investigates the determinants of accounting conservatism (e.g., LaFond and Roychowdhury, 2008; García Lara, García Osma and Penalva, 2009; Ahmed and Duellman, 2013; Jayaraman and Shivakumar, 213; Cheng, Huang, and Li, 2015; Manchiraju, Pandey, and Subramanyam, 2020). Prior studies on debtholder demand for conservatism mainly document associations between debt contracting characteristics and borrowers' conservatism. Using a unique setting that provides exogenous changes to creditor rights across states in the US, I provide causal evidence that borrowers' financial reporting becomes more conservative when non-securitization creditors' claims are less protected by laws. My study also relates to Martin and Roychowdhury's (2014) study on financial market developments that alter lenders' incentives to monitor borrowers, but in my setting the altered incentives are driven by legal institution. Focusing on these non-securitization creditors' incentives of continuous monitoring and demand for conservative accounting, my study contrasts with and complements prior literature that examines creditors' ex-post intervention (e.g., Tan, 2013; Aghamolla and Li, 2018). A very close paper is Aier, Chen and Pevzner (2014) which examine the effect of

creditor rights increase on accounting conservatism. They find that firms' financial reporting conservatism increased for near insolvent firms after 1991 Delaware court ruling that expands the scope of directors' fiduciary duties to include creditors when a Delaware-incorporated firm is in near insolvency. Different from 1991 Delaware courting ruling increasing directors' obligation to creditors as a whole, the ARL adoption increases non-securitization creditors' right by weakening securitization creditors' right. Aier, Chen and Pevzner (2014) focus on how increased creditor rights affect supply side of conservatism, while I focus on how non-securitization creditors such as commercial bank adjust their demand when their creditor rights decrease. Besides, I could alleviate "Delaware effect" as ARL is passed in several states.

Second, my study adds to the literature on the effect of creditor rights protection. Most of the prior literature in this topic uses cross-country settings to study corporate outcomes, such as corporate debt financing (Esty and Megginson, 2003; Safavian and Sharma, 2007; Bae and Goyal, 2009; Boubakri and Ghouma, 2010; Benmelech and Bergman, 2011; Cho et al., 2014; Gu and Kowalewski, 2016; Qi, Roth, and Wald, 2017; Moro, Maresch, and Ferrando, 2018), corporate investment (Seifert and Gonenc, 2012; Qi, Roth, and Wald, 2017), cash holding (Kyröläinen, Tan, and Karjalainen, 2013; Yung and Nafar, 2014), and dividend payout policy (Brockman and Unlu, 2009). Instead of using cross-country settings, I exploit the staggered adoption of state-level ARL as exogenous shocks to creditor rights to study the impact on borrowers' accounting conservatism. The unique ARL adoption setting helps mitigate the endogeneity problems that prior studies may subject to. My paper also joins a nascent and growing literature that evaluates the implications of ARL adoption (e.g., Chu, 2020; Favara, Gao and Giannetti, 2021; Ghanbari, 2019; Tut, 2019; Attig and Brockman, 2020; Attig and El Ghoul, 2020; Ersahin, 2020).

The rest of the paper is organized as follows. I introduce the background and develop hypotheses in section 2. Section 3 describes my research design and section 4 presents my main

results. In sections 5 and 6, I present my cross-sectional analyses and robustness checks, respectively. I conclude in section 7.

2. Background and hypotheses development

2.1 Anti-recharacterization Laws as exogenous shocks to creditor rights

Asset securitization was first introduced in the1980s and has been a popular financing tool for decades. When borrowers are in need of additional capital, they may choose to raise funds by securitizing their receivables. That is, borrowers sell receivables to a special purpose vehicle that issues asset-backed securities to investors/securitization creditors. Besides the borrowers, securitization creditors and the special purpose vehicle (issuer), the securitization process also involves the trustee, servicer, underwriter, bond lawyer, rating agency, and credit enhancement provider. The trustee is a third party appointed to represent securitization investors' interests; for example, the trustee will hire a servicer to collect payments and handle nonpayment on behalf of the issuer, who will then distribute these payments to investors. The servicer can be the borrower and is typically compensated with service fees. The underwriter assists the issuer to sell bonds to investors, and the rating agency provides credit ratings to the issued bonds. To evaluate the credit quality of a bond, the rating agency will consider various risk factors and the existence of credit enhancements, such as letters of credit provided by guarantors. Figure 1 demonstrates the asset securitization process.

<Insert Figure 1 Here>

In the process of asset securitization, the special purpose vehicle is designed to isolate borrowers' bankruptcy risk so that securitization creditors' interests can remain unaffected even when borrowers file for bankruptcy. To get the benefit of risk isolation, borrowers' receivables must be truly sold to the special purpose vehicle. However, the intended risk isolation design is not guaranteed by the true sale doctrine which governs asset securitization transactions in the absence of ARL. When borrowers file for bankruptcy, the true sale doctrine

gives bankruptcy court judge discretion to determine whether the receivables sold to the special purpose vehicle are a true sale or collateral of secured debt.[6] According to the US Bankruptcy Code Chapter 11, collateral of secured debt is subject to automatic stay when borrowers file for bankruptcy. To facilitate debtors' business continuation, the automatic stay provides a period of time during which all collection activities and repossession of property cannot be pursued by corresponding creditors. During the period of automatic stay, negotiations can take place to resolve debtors' financial difficulties by reaching a feasible reorganization plan.

Bankruptcy judges' discretion over the recharacterization of securitized assets goes against securitization creditors' interests, bond lawyers' opinion of "bankruptcy remoteness", and rating agencies' assumption that there is no need to consider the solvency of the borrower. Owing to the above problems, the securitization industry makes efforts to abolish the true sale doctrine, resulting in the adoption of ARL in several states of the US. Starting from 1997, nine states have enacted these laws: Louisiana and Texas in 1997, Alabama in 2001, Ohio in 2001, North Carolina and Delaware in 2002, South Dakota in 2003, Virginia in 2004, and Nevada in 2005 (Janger, 2003; Kettering, 2008). By denying bankruptcy judges' discretion, ARL significantly enhance the rights of securitization creditors by allowing them to seize the collateral directly and exclusively in the special purpose vehicle.

Following Ersahin's (2020) empirical design, my treatment states include only 4 states, i.e., Texas, Louisiana, Alabama, and Delaware. I do not consider North Carolina and Ohio as treatment states because the ARL adopted by these two states are limited to asset securitization of insured depositary institutions. Besides, I do not consider South Dakota, Virginia, and Nevada as treatment states because they adopted ARL after the 2003 federal court ruling on Reaves Brokerage Company, Inc., v. Sunbelt Fruit & Vegetable Company, Inc. (336 F.3d 410,

[6] For example, in the 1993 court ruling on Octagon Gas Systems, Inc. v. Rimmer (995 F.2d 948 (10th Cir. 1993)), the United States Court of Appeals for the 10th Circuit suggested that the transferred accounts receivable from a seller to a buyer nevertheless constitute property of the seller's bankruptcy estate.

413 (5th Cir. 2003)). In the Reaves case, Sunbelt, a wholesaler of fresh food and vegetables, ceased operations in 2000 and Reaves, a trade creditor, sought recovery from Sunbelt and Fidelity, a securitization creditor in Texas who purchased accounts receivable from Sunbelt. Even though Texas already adopted ARL in 1997 based on which Fidelity defended itself, the federal court ignored the ARL and recharacterized the sale of accounts receivable to secured lending. This ruling partially reverses the enacted ARL by providing a precedent that the federal court can ignore the state-level ARL (Chu, 2020; Ersahin, 2020).

Prior studies argue that the introduction of ARL was mostly driven by the lobbying efforts of the banking and especially the securitization industries (Kettering, 2003). However, the primary purpose that securitization industry lobbying for ARL is to abolish re-characterization on securitization (Janger, 2003; Li et al., 2016), elevate the popularity of asset-backed securities then provide a larger market for structured financial products and earn profits. Those banks involved are large banks whose premier revision effort was an attempt to amend the Bankruptcy Code to render re-characterization irrelevant in securitization industry. Since they failed to achieve the above objective, the securitization industry started working in the state legislatures and tried to spread it across the whole country later. Therefore, it is unlikely that banking engage in lobbying for the adoption of ARL in specific states, which mitigates the concern that the endogeneity of the passing of ARL may bias our results.

2.2 The effect of ARL on borrowers' accounting conservatism

In the context of debt financing, debtholders have an asymmetric payoff with respect to borrowers' net assets (Jensen and Meckling, 1976). Debtholders' continuous monitoring on borrowers may be the most traditional and effective way of protecting their claims, and it is especially true for monitoring experts like banks (Fama, 1985; James, 1987).[7] Taking

[7] Financial innovation in recent decades provides alternative ways for debtholders to manage credit risk, e.g., they may choose to transfer credit risk via credit default swaps or collateralized debt obligation (Ashcraft and Santos, 2009; Wang and Xia, 2014).

advantage of the lender-borrower relationship and their in-house financial expertise, banks gain superior information access and processing abilities (Sharpe, 1990; Diamond, 1991; Datta, Iskandar-Datta and Patel, 1999; Mester, Nakamura, and Renault, 2007; Ma, Stice, and Williams, 2019). For example, banks may make a site visit to their borrowers and/or request timely financial statements (Martin and Roychowdhury, 2015; Chen, 2016; Minnis and Sutherland, 2017). In particular, Chen (2016) finds that banks respond timely to borrowers' ongoing misreporting, suggesting that banks gain proprietary information before the information is publicly released. This finding highlights the effectiveness of banks' continuous monitoring. Prior studies also show that non-securitization creditors like banks rely on public financial reporting to monitor even though they have private information access to borrowers. For example, Minnis and Sutherland (2017) find that banks indeed request financial statements based on a dataset that records banks' ongoing requests of information from small commercial borrowers . Furthermore, they show that bankers use other information such as tax returns to confirm information reported in financial statements through their informal interviews with bankers.

The adoption of ARL motivates debtholders to monitor borrowers more actively and closely throughout the whole course of lending relationship. As in my earlier discussion, ARL preclude the recharacterization of securitized assets which otherwise can be recharacterized as collateral of secured loans. Collateral of secured loans is subject to automatic stay in the case of borrowers' bankruptcy, so that debtholders may be able to seize these assets. By contrast, with ARL in effect, securitization creditors can seize these assets exclusively. In other words, ARL protect securitization creditors at the expense of debtholders' claims. When borrowers file for bankruptcy, ARL reduce the assets that debtholders can repossess. To avoid or mitigate such potentially enlarged bad debt losses, debtholders such as banks are likely to exert more monitoring efforts after the adoption of ARL. With closer monitoring on borrowers, banks can

better assess credit risks and react more promptly. Intensified monitoring by banks can also discipline borrowers' managers for risk-taking behavior (Saunders and Song, 2018).

To facilitate monitoring effectiveness, debtholders are expected to demand more conservative financial reporting. Reporting conservatism requires borrowers to recognize losses more timely than profits, thus ensuring that their net assets are underestimated. Net assets under conservative accounting provide a lower bound for the measurement of borrowers' repayment ability. Debtholders are concerned about whether the borrowers' current value of net assets is enough to cover their debt, and they use this lower bound measure during the life of their debt to monitor borrowers (Watts, 2003a). A large body of literature suggests that accounting conservatism is an efficient monitoring mechanism for protecting debtholders' interests (e.g., Watts and Zimmerman, 1986; Watts, 2003a; Ball and Shivalumar, 2005; Beatty, Weber, and Yu, 2008; Zhang, 2008; Gormley, Kim, and Martin, 2012; Tan, 213; Aier, Chen, and Pevzner, 2014; Donovan, Frankel, and Martin, 2015). In particular, Gormley, Kim, and Martin (2012) find that in response to lenders' demand for more conservative accounting, borrowers recognize loss more timely, and lenders value this change in accounting conservatism.

In summary, because the adoption of ARL reduces the recoverability of debtholders' claims, I expect debtholders to monitor borrowers more closely and demand higher reporting conservatism which can facilitate their monitoring effectiveness. In response to debtholders' demand, borrowers may adjust their accounting practices accordingly (Gormley, Kim, and Martin, 2012). In addition, increased scrutiny from lenders would substantially constrain borrowers' ability to delay loss recognition, even though they may be incentivized to do so to boost short-term managerial compensation or to avoid covenant violations. In fact, conservative accounting can accelerate debt covenant violations (Zhang, 2008; Nikolaev, 2010), but these violations might be less of a concern under the ARL regime. Borrowers may

expect debtholders to be less intransigent in debt renegotiations because the increased asymmetry in debtholders' payoffs reduces their bargaining power. Considering all the above arguments, I therefore expect borrowers to be more conservative in accounting following the adoption of ARL.

However, there are also other reasons to expect an opposite or no effect of ARL adoption on borrowers' accounting conservatism. First of all, borrowers may become less willing to supply conservative accounting. One important motive for providing conservative reporting is to facilitate external financing and improve investment efficiency (Ahmed et al., 2002; Wittenberg-Moerman, 2008; Zhang, 2008; García Lara, García Osma and Penalva, 2011, 2016; Laux and Ray, 2020). In theory, the debt contracting demand of conservatism applies to both types of creditors---securitization and non-securitization creditors. Their demand for conservatism changes after the adoption of ARL. On the one hand, asset securitization is an alternative form of financing and ARL adoption enhances its advantage over other ways of financing (Tut, 2019; Favara, Gao and Giannetti, 2021; Chu, 2020; Ersahin, 2020). Because securitization creditors are protected by the bankruptcy-remote mechanism of ARL, they would not have any preferences for accounting conservatism. On the other hand, non-securitization creditors such as commercial banks demand more conservatism due to such potentially enlarged bad debt losses from bankruptcy. With this alternative form of financing, borrowers might become less dependent on traditional way of debt financing, leading to borrowers' lower willingness to supply conservative accounting. In this case, whose demand dominates my findings to some extent depends on which source of financing is more important for a borrower. If the borrower relies more on securitized debts than bank lending, then it is less likely to impress the non-securitization creditors by providing conservatism accounting numbers. Therefore, it is possible that borrowers become less conservative in their financial reporting following the adoption of ARL.

In addition, managers in the borrowing firms might be reluctant to change the current accounting practices because of the potential conflicts of interest. By alleviating agency problems, conservative accounting can benefit most of a company's stakeholders (e.g., Zhang, 2008; García Lara, García Osma and Penalva, 2009; Hui, Klasa, and Yeung, 2012), but increasing conservatism may go against managers' personal interests. By delaying the recognition of gains relative to losses, increasing conservatism in financial reporting is likely to reduce managers' current compensation or bonuses which are usually linked to current-period earnings (Leone, Wu, and Zimmerman, 2006). Conservative accounting can also restrict managers' ability to gain private benefits from daily operations and project investments (Watts, 2003; Ball and Shivakumar, 2005; Kravet, 2014; Caskey and Laux, 2017). Therefore, for the sake of their own interest, managers might not want to adjust accounting conservatism according to other stakeholders' demand. Therefore, it is possible that the adoption of ARL does not significantly change borrowers' accounting practices.

The above tensions illustrate the importance of empirically examining the impact of ARL adoption on borrowers' accounting conservatism. Even though the net effect is often ambiguous, I tend to believe that borrowers will react in response to debtholders' demand due to the importance of debt financing. I therefore state my central hypothesis in the alternative form as follows:

H1. *The adoption of ARL leads to an increase in borrowers' accounting conservatism.*

2.3 Cross-sectional hypotheses

To enhance the arguments concerning my central hypothesis, I develop several cross-sectional hypotheses. First, I focus on the potential moderating effect of borrowers' credit risk on the relation between ARL adoption and accounting conservatism. As I argued in my central hypothesis, debtholders are expected to monitor borrowers more closely and demand higher reporting conservatism because the adoption of ARL endangers their claims; otherwise, they

will lose more in the case of borrowers' insolvency. Following this line of reasoning, one might naturally expect debtholders to pay more attention to borrowers with higher repayment risk.

Debtholders' asymmetric payoffs motivate them to continuously monitor borrowers' ability to meet the repayment schedule. Debtholders are generally not concerned about high-quality borrowers because these borrowers can probably generate stable future cash flow to repay their debt. By contrast, debtholders are more concerned about borrowers with high credit risk (Diamond, 1991; Minnis and Sutherland, 2017). ARL can result in losses for debtholders only when their borrowers have difficulty in making repayments (Janger, 2003; Ghanbari, 2019). Therefore, given that high-risk borrowers are more likely to default and file for bankruptcy in an extreme case, I expect the positive effect of ARL on accounting conservatism, if any, to be more pronounced for borrowers with higher credit risk. Accordingly, I state this hypothesis as follows:

H2. *The positive effect of ARL on accounting conservatism, if any, is stronger for firms with higher credit risk.*

My second cross-sectional hypothesis explores the potential moderating role of borrowers' corporate governance. According to the basic economic principle, both demand- and supply-side forces can shape borrowers' accounting practices. While debtholders demand more conservative accounting due to the shock of ARL, the extent of conservatism also depends on borrowers' willingness to meet debtholders' demand. As I argued in my central hypothesis, the availability and attractiveness of asset securitization might reduce borrowers' dependence on traditional debt financing, resulting in less incentives for borrowers to provide conservative reporting. The conflict of interest induced by agency problems also makes firm managers more reluctant to respond to debtholders' demand for conservative reporting.

Strong corporate governance can potentially mitigate the above problems. As an efficient contracting mechanism, conservative accounting increases firm value by constraining

managers' opportunistic behavior (Watts, 2003). The increased firm value can benefit not only debtholders but also shareholders and other stakeholders. Better-governed firms should care more about firm value as a whole and the balance of interest among stakeholders (Gompers, Ishii, and Metrick, 2003; Klock, Mansi, and Maxwell, 2005; García Lara, García Osma and Penalva, 2009). Therefore, I posit that borrowers with better corporate governance are more likely to meet debtholders' demand for conservative reporting. Accordingly, I expect the positive effect of ARL on accounting conservatism, if any, to be stronger for better-governed firms. I state this hypothesis as follows:

H3. *The positive effect of ARL on accounting conservatism, if any, is stronger for firms with better corporate governance.*

3. Research design

3.1 Empirical model

My primary measure of accounting conservatism is the asymmetric timeliness of earnings with respect to good vs. bad news (e.g., Basu, 1997; Ball, Kothari, and Robin, 2000; Ahmed and Duellman, 2007; LaFond and Watts, 2008; Ramalingegowda and Yu, 2012). Following Aier, Chen and Pevzner (2014) and Martin and Roychowdhury (2015), I use the following expanded Basu (1997) model which incorporates my DID design to test the effect of ARL adoption on accounting conservatism:

$$NI_{i,h,k,t} = \beta_0 + \beta_1 ARL_{h,k,t} \times NEG_{i,t} \times RET_{i,t} + \beta_2 ARL_{h,k,t} \times NEG_{i,t} + \beta_3 ARL_{h,k,t} \times RET_{i,t} + \beta_4 ARL_{h,k,t} + \beta_5 NEG_{i,t} \times RET_{i,t} + \beta_6 NEG_{i,t} + \beta_7 RET_{i,t} + Controls \times NEG_{i,t} \times RET_{i,t} + Controls \times NEG_{i,t} + Controls \times RET_{i,t} + Controls + f_i + y_t + \varepsilon_{i,t}, \quad (1)$$

where the dependent variable, $NI_{i,h,k,t}$, is the income before extraordinary items of firm i—headquartered in state h and incorporated in state k—in year t, scaled by its market value of

equity at the beginning of the year. $RET_{i,t}$ is the twelve-month compounded stock returns of firm *i* over the fiscal year *t*. $NEG_{i,t}$ is an indicator variable which equals 1 if $RET_{i,t}$ is negative and 0 otherwise. In Eq. (1), β_7 captures timeliness of earnings with respect to good news and β_5 captures incremental timeliness with respect to bad news compared to good news, i.e., my primary measure of accounting conservatism.

To test the effect of the staggered adoption of ARL on accounting conservatism, I create a dummy variable, $ARL_{h,k,t}$, and include it and its interaction terms with $NEG_{i,t}$, $RET_{i,t}$, and $NEG_{i,t}\times RET_{i,t}$ into Eq. (1). Following Ersahin (2020), $ARL_{h,k,t}$ is defined according to each sample firm's historical information on headquarters state (*h*) and incorporation state (*k*). Specifically, $ARL_{h,k,t}$ equals 1 for firms located or incorporated in Texas or Louisiana over the period 1997 to 2003, firms located or incorporated in Alabama over the period 2001 to 2003, and firms located or incorporated in Delaware over the period 2002 to 2003, and 0 otherwise. The coefficient on the three-way interaction term, $ARL_{h,k,t} \times NEG_{i,t}\times RET_{i,t}$, is of my interest, capturing the effect of ARL on borrowers' conditional conservatism. On the basis of my hypothesis that conservatism increases after the adoption of ARL, I expect β_1 to be significantly positive.

In Eq. (1), I include firm fixed effects (f_i) to control for time-invariant unobservable firm characteristics and year fixed effects (μ_t) to account for time-variant macroeconomic factors. In addition, I control for firm size ($SIZE_{i,t-1}$), market-to-book ($MTB_{i,t-1}$), and leverage ($LEV_{i,t-1}$) to ensure that these firm characteristics are not driving my results. $Size_{i,t-1}$ is the natural logarithm of total assets of firm *i* at the beginning of fiscal year *t*. $MTB_{i,t-1}$ is the sum of total assets and market value of equity minus book value of equity for firm *i* at the beginning of fiscal year *t*, divided by total assets at the beginning of the fiscal year. $LEV_{i,t-1}$ is total liabilities divided by total assets for firm *i* at the beginning of fiscal year *t*. Consistent with prior literature (e.g., LaFond and Roychowdhury, 2008; Ahmed and Duellman, 2013; Cheng

et al, 2015), I use scaled decile rank for all of the control variables when estimating Eq. (1). The scaled decile rank is determined by first ranking observations each year into ten groups from zero to nine, and then scaling the ranking by ten so that the rank variable falls within the zero-to-one interval. I summarize the definitions of all variables in Appendix A, and I winsorize all continuous variables at the 1st and 99th percentiles.

3.2 Sample and data

Panel A of Table 1 describes construction of the sample used to test my main hypothesis. My sample starts with 118,624 firm-year observations for all public firms available in the Compustat database from 1994 to 2009. I first exclude 27,169 observations for non-US firms and 20,491 observations in financial industries (SIC: 6000–6999). To determine whether a firm is located or incorporated in a state that has passed ARL, my empirical methodology requires data on the historical state of incorporation and location. As Compustat only provides the latest state of incorporation and location, which may result in measurement errors, I collect historical information on headquarters and incorporation states from SEC 10K filings in EDGAR. As SEC 10K filings provide historical state information starting from 1994, my sample begins with 1994. I end my sample period with 2009 to avoid including observations too far away from my ARL shock. I also need to drop observations for firms changing their location states or incorporation states during my sample period following Gormley and Matsa (2016) to alleviate concern of mistakenly classifying a firm into the treatment or control group. Therefore, I drop 9,179 observations with missing historical information on states or firms having changed headquarters or incorporation states during my sample period.[8]

Furthermore, I drop 3,568 observations with negative book value of equity and 2,574 observations with less than 6 months of data in the Center for Research in Security Prices

[8] In an untabulated test, I show that my inferences remain unchanged if I include those firms having changed headquarters or incorporation states during my sample period.

(CRSP) database over the fiscal year to calculate annual returns. I exclude additional 1,475 firm-year observations with missing data on firm size, leverage, market-to-book, and earnings for baseline regression. Finally, after requiring firms in my sample having at least two observations so that Ball et al.'s (2013a) procedure is meaningful, my final sample consists of 53,391 firm-year observations representing 6,962 unique firms.

Panel B shows the sample distribution over the period 1994 to 2009. There are 11 percent of firm-year observations with *ARL* equal to 1.

<Insert Table 1 Here>

3.3 Descriptive statistics

Table 2 reports the descriptive statistics for the whole sample. In Panel A, I present the mean, standard deviation (SD), 25th percentile (Q1), median, and the 75th percentile (Q3) of each variable used in my baseline regression. During my sample period from 1994 to 2009, while the mean of net income divided by the beginning market value of equity (*NI*) is negative (-0.029), the median of this variable is positive (0.036), indicating left skewness of earnings which is consistent with Basu (1997) and Ball et al. (2000). The mean of *TREAT* (an indicator equals 1 for firms located or incorporated in Texas, Louisiana, Alabama or Delaware, and 0 otherwise) is 0.626, suggesting that my treatment group accounts for 62.6 percent of my sample, while the mean of *ARL* (an indicator equals 1 for firms located or incorporated in Texas or Louisiana during the period 1997–2003, firms in Alabama during the period 2001–2003, and firms in Delaware during the period 2002–2003, and 0 otherwise.) is 0.109, meaning that about 10.9 percent of my sample are in the effective period of ARL. The mean (median) for the 12-month buy-and-hold return ending at the fiscal year is 0.134 (0.015). The mean value of *NEG* (an indicator variable that takes a value of 1 if *RET* is negative and 0 otherwise) is 0.486, indicating that about 48.6 percent of observations have a negative *RET*. The mean (median) beginning natural logarithm of total assets (*SIZE*) is 5.255 (5.089), suggesting that mean

(median) value of total assets in my sample is USD1,487 (162) million. The mean (median) value of market-to-book ratio (*MTB*) is 2.174 (1.526). The mean (median) value of leverage (*LEV*) is 0.196 (0.160). Panel B reports the Pearson correlation matrix among variables in my baseline regression.

<Insert Table 2 Here>

4. Main results

4.1 Baseline results

In my main hypothesis, I argue that whether and how ARL affect borrowers' accounting conservatism is an empirical question. On the one hand, debtholders may demand more conservative accounting in response to the ARL adoption which reduces the recoverability of their debt. On the other hand, borrowers' incentives to supply reporting conservatism may decrease following the adoption of ARL because asset securitization becomes a more attractive form of financing, resulting in less dependence on debt financing. Therefore, the net effect of ARL adoption depends on which side dominates the other. To test my main hypothesis, I estimate a DID model as specified in Eq. (1) where the dependent variable is earnings scaled by market value of equity (*NI*), and my interest centers on the three-way interaction term (*ARL*× *NEG*×*RET*).

Table 3 presents my main results. I start my analyses with a subsample of firms that are headquartered or incorporated in my 4 treatment states (*TREAT*=1), i.e., Texas, Louisiana, Alabama and Delaware. By focusing on these treatment firms, I can observe their time-series variation in accounting conservatism and avoid the influence of control firms. I present results on these treatment firms in the first two columns. I find that, compared with the pre-ARL adoption period, treatment firms become more conservative in their financial reporting in the post-ARL adoption period. This is not a DID design but the finding is consistent with my

expectation. In addition, results in these two columns show that this finding is not affected by adding control variables.

In columns (3) and (4), I use the full sample which includes both treatment and control firms. In column (3), I estimate a simplified model that excludes all control variables and their interaction terms, since one might argue that these firm-level control variables may be also affected by the adoption of ARL, which may result in estimation bias. Consistent with prior literature studying asymmetric timeliness of earnings (e.g., Basu, 1997; Khan and Watts, 2009), the coefficient on the two-way interaction term (*NEG*×*RET*) is significantly positive, suggesting that earnings reflect bad news more quickly than good news. Moreover, consistent with my central hypothesis, the coefficient on the three-way interaction term (*ARL*×*NEG*×*RET*) is positive and significant at the 1% level.

In column (4), I estimate my baseline DID model, as specified in Eq. (1), which controls for firm size (*SIZE*), market to book ratio (*MTB*), financial leverage (*LEV*), and their effects on asymmetric timeliness of earnings. I continue to find a significantly positive coefficient on the three-way interaction term (*ARL*×*NEG*×*RET*), suggesting that firms' financial reporting becomes more conservative after the adoption of ARL. Economically, the incremental increase in asymmetric timeliness for treatment firms relative to control firms is about 20% of the conservatism level of treatment firms before the ARL adoption.[9] Collectively, my results imply that debtholders' demand outweighs borrowers' reluctance to supply, and therefore I find that the net effect of ARL on accounting conservatism is positive.

<Insert Table 3 Here>

[9] Following Martin and Roychowdhury (2015), the economic significance is calculated as the coefficient on the three-way interaction term (*ARL*×*NEG*×*RET*) divided by the coefficient on the two-way interaction term (*NEG*×*RET*), i.e., 0.074/0.371=19.95%. This magnitude is comparable with that of Martin and Roychowdhury (2015) who document that firms reduce the level of accounting conservatism by 20.30% following the onset of CDS trading.

4.2 Parallel trend and dynamic effects

To test the parallel trend assumption of my DID model, I augment the baseline specification by including a series of relative year dummies to test the dynamic effects of ARL adoption. The relative year dummies are defined according to each treatment state's adoption year. Taking firms located or incorporated in Texas as an example, because the ARL adoption year of Texas is 1997, I define a dummy variable ARL^{-2} (ARL^{-1}) as equaling 1 for these firms' observations in 1995 (1996) and 0 otherwise. Similarly, the dummy variable ARL^{0}, (ARL^{1}, ARL^{2}) equals 1 for these firms' observations in 1997 (1998, 1999) and 0 otherwise. To cover all other post-adoption years, I define another dummy variable ARL^{3+} as equaling 1 for these firms' observations in 1999 or later years but not after 2003 and 0 otherwise.

Table 4 presents results of this test. In columns (1) and (2), I focus on treatment firms only. Using the accounting conservatism level of treatment firms in year t-3 and earlier years as a benchmark, I do not observe significant changes in accounting conservatism in year t-2 or t-1. However, I do observe that these treatment firms begin to increase their conservatism level in the year of ARL adoption, and their conservatism remains at a higher level in later years. These dynamic effects are important findings because they suggest that treatment firms adjust their accounting practices in response to the adoption of ARL.

To quantify the net effect of ARL adoption by comparing treatment with control firms, I use the full sample in columns (3) and (4). I find that these relative year dummies before the ARL adoption are insignificant. These results suggest that treatment and control firms are comparable prior to the ARL adoption. The lack of trends in the pre-adoption period also suggests that the ARL adoption is exogenous to my sample firms so that they cannot change their accounting behavior in advance. Therefore, these insignificant results lend support to the parallel trend assumption of using the DID model. Focusing on year dummies in the post-adoption period, I find that firms' financial reporting becomes more conservative immediately

after the adoption of ARL and continues to show a higher extent of conservatism in the following 3 years or more.

<Insert Table 4 Here>

4.3 Placebo test

To mitigate the concern that confounding factors like local economic shocks may drive my results, I conduct a placebo test by falsely treating the neighboring states of ARL adoption states. Specifically, I follow Ersahin's (2020) approach to falsely treat the neighboring states of Texas, Louisiana, Alabama and Delaware. Taking Texas as an example, I falsely assume that the neighboring states of Texas adopted ARL at the same time as Texas (i.e., in 1997). In the case of Texas, the neighboring states include three states (i.e., Arkansas, New Mexico and Oklahoma) that have not adopted ARL. According to this false assumption, I can reconstruct my key variable of interest (*ARL*). By construction, this test can alleviate the concern that my results are driven by regional economic shocks overlapped with the ARL adoption events. While regional shocks may spill over into neighboring states, the effects of ARL are restricted to the adoption states. An insignificant coefficient on the reconstructed three-way interaction term (*ARL*×*NEG*×*RET*) would validate my main findings.

I present the results of this placebo test in Table 5. Consistent with my expectation, I find insignificant results in column (1) in which the four states that truly adopted ARL are considered as control states. To construct a cleaner control group, I further drop the four states that truly adopted ARL (i.e., Texas, Louisiana, Alabama and Delaware). In column (2), I continue to find an insignificant coefficient on the three-way interaction term (*ARL*×*NEG*×*RET*). Taken together, results from my placebo tests validate my main findings by suggesting that it is the ARL adoption, rather than regional economic shocks, that leads to firms' more conservative financial reporting.

<Insert Table 5 Here>

5. Cross-sectional analyses

So far I have established the causal link between creditor rights and borrowers' conservative accounting. I argue that debtholders may demand more conservative accounting in response to the shock of credit rights: given that ARL endanger the recoverability of debtholders' claims, debtholders will monitor borrowers more closely and conservative accounting can increase the effectiveness of their monitoring. Consistent with this channel, I show that borrowers' financial reporting becomes more conservative following the adoption of ARL. In this section, I conduct cross-sectional analyses to provide further support to this channel.

5.1 The role of borrowers' credit risk

First, I exploit the variation in debtholders' incentives to monitor as captured by borrowers' credit risk. Debtholders' asymmetric payoffs motivate them to continuously monitor borrowers' ability to meet the repayment schedule. While debtholders are generally not concerned about high-quality borrowers because these borrowers can generate stable future cash flow to repay their debt, they are more concerned about borrowers with high credit risk. ARL can result in losses for debtholders only when their borrowers file for bankruptcy. Given that high-risk borrowers are more likely to default and file for bankruptcy in an extreme case, I expect the effect of ARL on accounting conservatism to be more pronounced for borrowers with higher credit risk.

To test this prediction, I use various measures of credit risk to capture debtholders' incentives to monitor borrowers. First, I employ Merton's (1974) distance to default to measure borrowers' credit risk. I follow Vassalou and Xing's (2004) procedure to construct the distance to default (*DTD*) which is an inverse measure of credit risk: the higher the value of *DTD*, the lower the probability of default. Second, I use two commonly used indexes of financial

constraints to capture borrowers' credit risk. Specifically, I construct Whited and Wu's (2006) index (*WWINDEX*) and Hadlock and Pierce's (2010) size-age index (*SAINDEX*). Using these credit risk measures, I classify firms with higher risk (*HIGHRISK*=1) as those with a *DTD* lower than the industry-year median, with a *WWINDEX* higher than the industry-year median, or with a *SAINDEX* higher than the industry-year median. Then I interact *HIGHRISK* with the three-way interaction term (*ARL*×*NEG*×*RET*) to test the role of borrowers' credit risk (H2).

Table 6 presents the results of testing H2. In column (1), I show the results of using Merton's (1974) distance to default (*DTD*) to measure borrowers' credit risk. Consistent with my expectation, I find a significantly positive coefficient on the four-way interaction term (*HIGHRISK*×*ARL*×*NEG*×*RET*), suggesting that the positive effect of ARL adoption on borrowers' accounting conservatism is stronger for firms that are closer to default. In columns (2) and (3), I respectively use Whited and Wu's (2006) *WWINDEX* and Hadlock and Pierce's (2010) *SAINDEX* to identify firms with high vs. low credit risk. Again, I continue to find that the coefficient on the four-way interaction term is significantly positive, suggesting that the effect of ARL is stronger for firms that are more financially constrained. Taken together, using various measures of credit risk, I find consistent results to support my hypothesis. These results suggest that debtholders pay more attention to high-risk borrowers, thus providing corroborative evidence to my main argument that debtholders monitor borrowers more closely and demand more conservative accounting following the adoption of ARL.

<Insert Table 6 Here>

5.2 The role of corporate governance

Second, I focus on borrowers' corporate governance. According to the basic economic principle, both demand- and supply-side forces can shape borrowers' accounting practices. While debtholders demand more conservative accounting due to the shock of ARL, the extent of conservatism also depends on borrowers' willingness to meet debtholders' demand. Prior

literature suggests that reporting conservatism can benefit not only debtholders but also shareholders by increasing firm value. Better-governed firms care more about firm value as a whole and the balance of interest among stakeholders. Therefore, borrowers with better corporate governance are more likely to meet debtholders' demand. Accordingly, I expect the effect of ARL on accounting conservatism to be stronger for better-governed firms.

To test this prediction, I use various measures of corporate governance. First, I use Gompers, Ishii, and Metrick's (2003) governance index (*GINDEX*) which aggregates the incidence of 24 antitakeover provisions. *GINDEX* is an inverse measure of corporate governance, i.e., the lower the value of the index, the stronger the shareholder rights. Second, I consider the borrowing firms' external governance based on the quality of their financial report auditors. A large body of literature suggests that Big N auditors are of higher quality and provide a higher level of scrutiny (e.g., DeAngelo, 1981; Khurana and Raman, 2004; DeFond, Erkens, and Zhang, 2016). Using these corporate governance measures, I classify firms with good governance (*GOODGOV*=1) as those with a *GINDEX* lower than the industry-year median or that have engaged a Big N auditor. Then I interact *GOODGOV* with the three-way interaction term (*ARL*×*NEG*×*RET*) to test the role of corporate governance (H3).

Table 7 presents the results of testing H3. In column (1), I rely on Gompers et al.'s (2003) *GINDEX* to divide my sample into two groups: firms with high vs low corporate governance.[10] Consistent with my expectation, I find that the coefficient on the four-way interaction term (*GOODGOV*×*ARL*×*NEG*×*RET*) is significantly positive, suggesting that firms with better corporate governance experience a larger increase in accounting conservatism following the adoption of ARL. In column (2), I focus on borrowers' external governance as captured by Big N auditors. The significantly positive coefficient on the four-way interaction

[10] The regression sample size in column (1) of Table 7 decreases significantly because of the data availability issue.

term suggests that the effect of ARL on accounting conservatism is stronger for firms with high-quality auditors. Taken together, these results are consistent with my hypothesis. These results suggest that borrowers with better corporate governance are more likely to meet debtholders' demand for conservative accounting.

<Insert Table 7 Here>

6. Additional analyses

In this section, I conduct a series of supplementary analyses to check the robustness of my empirical results.

6.1 Alternative clustering and fixed effects

In Table 8, I present a series of robustness checks for my baseline finding. In my baseline regression, standard errors are one-way clustered at the location state level because my setting of ARL shock is also at the state level. As an alternative specification, I use two-way clustering to account for both cross-sectional and time-series dependence in my panel data set (Gow et al., 2010). As shown in column (1) of Panel A, I continue to find a significantly positive coefficient on the three-way interaction term when the regression standard errors are two-way clustered by location state and fiscal year. In columns (2) and (3), I show that my main results hold when standard errors are clustered by incorporation state and two-way clustered by incorporation state and year. In columns (4) and (5), when using one-way clustering by firm and two-way clustering by firm and year, the coefficient on the three-way interaction term remains significantly positive. Therefore, my results are less likely to be affected by different clustering schemes.

In Panel B, I check if my results are sensitive to more granular fixed effects. In my baseline specification, I include firm and year fixed effects. Year fixed effects absorb time-variant macroeconomic factors that can potentially affect firm performance and accounting

behavior. In column (1), I replace the year fixed effects with location state-year fixed effects to account for local economic shocks, i.e., I control for more disaggregated state-year fixed effects. In column (2), I consider the incorporation state-year fixed effects. In both columns, I get qualitatively the same results as my baseline specification. In column (3), I consider the time-variant economic factors to vary across industries by including industry-year fixed effects. Again, I find similar results. In short, my results are robust to alternative fixed effects.

6.2 Alternative ways of defining the ARL dummy

In my main analysis, I identify treatment firms by taking into consideration each sample firm's headquarters state and incorporation state. Here I check if my results hold when I define the ARL dummy solely on the basis of the borrower's headquarters or incorporation state. As shown in column (1) of Panel C, when I only consider the treatment effect from the adoption of ARL in borrowers' headquarters states, I continue to find a significant increase in borrowers' accounting conservatism. In column (2), the significantly positive coefficient on the three-way interaction term suggests that the effect solely from the adoption of ARL in incorporation states is also statistically significant. Therefore, my main results are not driven by either treatment effect from borrowers' headquarters or incorporation states.

In my baseline specification, I focus on the four states that adopted ARL before 2003 when the federal court ruling substantially reduced the relevance of ARL. Because of this court ruling, I only assign a value of 1 to the ARL dummy for treatment firms' observations in the period from the adoption year to 2003. As a robustness check, I intentionally ignore the 2003 court ruling, i.e., I consider all seven states that have adopted ARL as my treatment states (i.e., Texas, Louisiana, Alabama, Delaware, South Dakota, Virginia, and Nevada) and assign a value of 1 to the ARL dummy for all treatment firms' observations in the post-ARL period. In column (3), I show that my results hold if I ignore the 2003 court ruling, albeit the magnitude is slightly smaller than that in my baseline model.

6.3 Alternative samples

Next, I check if my results are robust to alternative samples and I present results in Panel D. To further deal with the 2003 federal court ruling that substantially reverses the adoption of ARL, I alternatively focus on a sample ending in 2003. Column (1) shows that my results are essentially the same in this shorter sample period. In my baseline analysis, my sample includes all the US states, four of which are treatment states (Texas, Louisiana, Alabama and Delaware) that adopted ARL before 2003. Some states adopted ARL after 2003 (e.g., South Dakota, Virginia, and Nevada) but I use them as control states as if they have never adopted ARL because the federal court ruling in 2003 substantially reduced the relevance of these state-level laws. As a robustness check, I redo my baseline analysis by dropping these states that adopted ARL after 2003.[11] Column (2) shows that excluding these states does not change my inferences.

In column (3), I check if my results are solely driven by firms incorporated in Delaware, as a large number of firms have chosen to incorporate in Delaware. Excluding firms incorporated or headquartered in Delaware does significantly reduce my testing sample. However, I continue to find a significantly positive coefficient on the three-way interaction term of my interest. Manchiraju, Pandey and Subramanyam (2020) document that the adoption of universal demand laws alters firms' accounting practices. Coincidently, Texas, one of my treatment states, adopted both ARL and the universal demand laws in 1997.[12] To deal with this potential confounding effect, I redo my analysis by dropping firms headquartered or incorporated in Texas. Results in column (4) show that my results are not driven by the confounding effects of universal demand laws.

[11] In addition, I get similar results if I further drop North Carolina and Ohio in which the adopted ARL are limited to sales made by insured depositary institutions.

[12] My other three treatment states (Louisiana, Alabama and Delaware) are not affected by the universal demand laws.

6.4 Alternative ways of measuring accounting conservatism

Panel E presents results of using alternative ways to measure accounting conservatism. First, following Khan and Watts (2009), I construct a firm-year score of asymmetric timeliness as follows:

$$NI_{i,t} = \beta_0 + \beta_1 NEG_{i,t} \times RET_{i,t} + \beta_2 NEG_{i,t} + \beta_3 RET_{i,t} + \varepsilon_{i,t} \quad (2)$$

$$GSCORE_{i,t} = \beta_3 = \mu_0 + \mu_1 MV_{i,t} + \mu_2 MTB_{i,t} + \mu_3 LEV_{i,t} \quad (3)$$

$$CSCORE_{i,t} = \beta_1 = \gamma_0 + \gamma_1 MV_{i,t} + \gamma_2 MTB_{i,t} + \gamma_3 LEV_{i,t} \quad (4)$$

Replacing β_1 and β_3 in Eq. (2) with the expression of Eq. (3) and (4), I obtain:

$$NI_{i,t} = \beta_0 + \left(\gamma_0 + \gamma_1 MV_{i,t} + \gamma_2 MTB_{i,t} + \gamma_3 LEV_{i,t}\right) \times NEG_{i,t} \times RET_{i,t} + \beta_2 NEG_{i,t} + \left(\mu_0 + \mu_1 MV_{i,t} + \mu_2 MTB_{i,t} + \mu_3 LEV_{i,t}\right) \times RET_{i,t} + (\delta_1 MV_{i,t} + \delta_2 MTB_{i,t} + \delta_3 LEV_{i,t} + \delta_4 MV_{i,t} \times NEG_{i,t} + \delta_5 MTB_{i,t} \times NEG_{i,t} + \delta_6 LEV_{i,t} \times NEG_{i,t}) + \varepsilon_{i,t} \quad (5)$$

All variables are defined earlier as in Eq. (1). I can obtain a firm-specific conservatism measure (*CSCORE*) by applying the estimated coefficient from Eq. (5) into Eq. (4). Eq. (5) is estimated by each fiscal year. By construction, a larger value of *CSCORE* indicates greater conditional conservatism.

Second, I use Ahmed et al.'s (2002) measure of unconditional conservatism (*CONACC*), which is also used by Ahmed and Duellman (2007, 2013). *CONACC* is defined as the income before extraordinary items less cash flows from operations plus depreciation expense, scaled by average total assets, and then averaged over the previous three years, multiplied by negative one. A larger value of *CONACC* suggests greater unconditional conservatism. Results in Panel E show that *CSCORE* and *CONACC* increase after the adoption of ARL, which is consistent with my baseline results.

<Insert Table 8 Here>

7. Conclusion

Using the staggered adoption of state-level ARL as an exogenous shock to creditor rights, I examine how the change in creditor rights shapes firms' accounting behavior. Specifically, I focus on borrowing firms' financial reporting conservatism. Based on a large sample of US firms during the period 1994–2009, my DID analysis reveals that firms headquartered or incorporated in treatment states become more conservative in financial reporting immediately after the adoption of ARL. These results support debtholders' demand for more conservative accounting in response to the ARL adoption which reduces the recoverability of their debt.

In my cross-sectional tests, I first show that the effect of ARL is stronger for borrowers with higher credit risk. This finding provides further support to the argument of debtholders' demand because debtholders are likely to monitor high-risk borrowers more closely. I also show that the effect of ARL is stronger for borrowers with better corporate governance, suggesting that better-governed firms are more likely to meet debtholders' demand. Collectively, my study establishes a causal link between creditor rights and accounting conservatism, and adds to the literature by offering new insights from both perspectives of demand and supply.

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Appendix A: Variable definitions and data sources

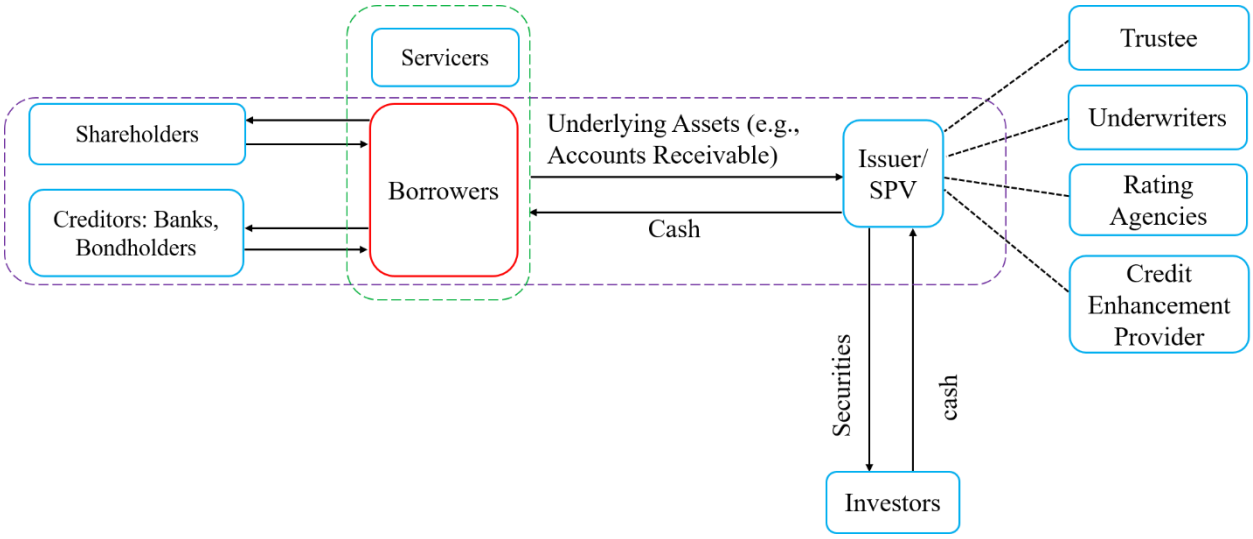
Variable	Definition	Data Source
Variables used in the main regression:		
NI	Income before extraordinary items in fiscal year t scaled by beginning market value of equity.	Compustat
TREAT	An indicator that equals 1 for firms headquartered or incorporated in my treatment states, i.e., Texas, Louisiana, Alabama or Delaware, and 0 otherwise.	Janger (2003)
ARL	An indicator that equals 1 for firm-year observations headquartered or incorporated in Texas or Louisiana during the period 1997–2003, in Alabama during the period 2001–2003, in Delaware during the period 2002–2003, and 0 otherwise.	Janger (2003)
ARL^{x}	An indicator variable that equals 1 for firms whose headquarters or incorporation state will adopt ARL in *x* year(s) ($x<0$) or have adopted ARL for *x* year(s) ($x>=0$) and 0 otherwise.	Janger (2003)
RET	The twelve-month buy-and-hold stock return of firm i over the fiscal year t.	CRSP
NEG	An indicator that equals 1 if *RET* of firm i over fiscal year t is negative and 0 otherwise.	CRSP
SIZE	The natural logarithm of total assets of firm i at the beginning of fiscal year t.	Compustat
MTB	The sum of total assets and market value of equity minus book value of equity of firm i, scaled by total assets at the beginning of fiscal year t.	Compustat
LEV	The ratio of total debt to total assets for firm i at the beginning of fiscal year t.	Compustat
Other variables: (in alphabetical order)		
BIGN	An indicator variable that equals one if the auditor of firm i is one of the Big N international accounting firms.	Compustat
CSCORE	The firm-specific asymmetric timeliness scores for firm i in fiscal year t developed by Khan and Watts (2009).	Compustat & CRSP
CONACC	The income before extraordinary items less cash flows from operations plus depreciation expense of firm i in fiscal year t, deflated by average total assets over year t-1 and t, and averaged over the previous three years, multiplied by negative one.	Compustat
CFO	The cash flows from operations for firm i in fiscal year t divided by average total assets.	Compustat
DTD	Merton's (1974) distance to default measure.	Compustat & CRSP
GINDEX	The anti-takeover index for firm i in fiscal year t, which equals the sum of 24 governance provisions.	ISS
MV	The natural logarithm of market value of equity for firm i in year t.	Compustat
RDAD	The ratio of total research and development expense plus advertising expense to total sales for firm i in fiscal year t.	Compustat
SAINDEX	Hadlock and Pierce's (2010) size-age index of financial constraints for firm i in fiscal year t, calculated as $-0.737\times SIZE+0.043\times SIZE^{2}-$	Compustat

	0.040×*AGE*, where *SIZE* is the natural logarithm of total assets capped at $4,500 million; *AGE* is the number of years firm i has been included in Compustat up to fiscal year t, capped at 37.	
SG	The annual growth rate in total sales from fiscal year t-1 to t.	Compustat
STDREV	The standard deviation of the natural log of revenues for firm i measured from fiscal year t–5 to year t–1.	Compustat
WWINDEX	Whited and Wu's (2006) financial constraint index for firm i in fiscal year t, calculated as -0.091×*OCF*-0.062×*DIVPOS*+0.021×*TLTD*-0.044×*LNTA*+0.*102*×*ISG*-0.035×*SG*, where *OCF* is operational cash flow in fiscal year t scaled by lagged total assets; *DIVPOS* is an indicator variable which equals one if firm i has a positive dividend payout in fiscal year t; *TLTD* is long-term debt scaled by total assets in fiscal year t; *LNTA* is the natural logarithm of total assets in fiscal year t; *SG* is firm i's sale growth in fiscal year t; *ISG* is two-digit industry average sale growth in fiscal year t ;	Compustat

This table summarizes the definitions of the variables used in the paper.

Appendix B: Figures and Tables

Figure 1 Illustration of Borrowers' Asset Securitization



The figure illustrates the borrowers' asset securitization process. In the process, borrowers or servicers sell the underlying assets (e.g., accounts receivable) to the issuer/SPV. The Issuer/SPV sells the securities as a bond to the investor. The trustee, as a third party, represents the investors' interests in securitization. The underwriter assists the sale, while the rating agency rates the security. Credit enhancement agencies provide the credit enhancement on securities.

Table 1 Sample description

Panel A: Sample selection procedure

Steps:	# of observations
All Compustat observations from fiscal year 1994 to 2009	118,624
Less: Non-US companies	(27,169)
Financial industries (SIC: 6000-6999)	(20,491)
Firms with missing historical information on or having changed the headquarters or incorporation state	(9,179)
Observations with negative book value of equity	(3,568)
Observations with less than 6 months of data to calculate annual returns	(2,574)
Observations with missing values of baseline regression variables	(1,475)
Firms with only one observation during my sample period	(777)
Final sample	53,391

Panel B: Sample distribution

Fiscal year	# of Obs	% of Obs	# of Treatment with *ARL*=1	% of Treatment with *ARL*=1
1994	3,534	6.62	0	0.00
1995	3,932	7.36	0	0.00
1996	4,171	7.81	0	0.00
1997	4,332	8.11	400	9.23
1998	4,143	7.76	381	9.20
1999	3,830	7.17	353	9.22
2000	3,650	6.84	334	9.15
2001	3,472	6.50	334	9.62
2002	3,207	6.01	2,076	64.73
2003	2,978	5.58	1,915	64.30
2004	2,882	5.40	0	0.00
2005	2,833	5.31	0	0.00
2006	2,743	5.14	0	0.00
2007	2,651	4.97	0	0.00
2008	2,624	4.91	0	0.00
2009	2,409	4.51	0	0.00
Total	53,391	100.00	5,793	10.85

This table presents the sample description. Panel A reports steps of sample selection. Non-US companies and firms in financial industries (SIC 6000-7000) are excluded. Observations with missing historical state information or having changed the headquarters or incorporation state are dropped. Observations with negative book value of equity are dropped. Firm-year observations with less than 6 months of data to calculate annual returns are dropped. Observations for firms with missing data on earnings, firm size, leverage and market-to-book are dropped. Firms with only one observation during the sample period are deleted. After applying the selection criteria, the final sample has 53,391 firm-year observations. Panel B reports the sample distribution.

Table 2 Descriptive statistics

Panel A: Summary statistics

Variable	N	Mean	SD	Q1	Median	Q3
NI	53,391	-0.029	0.219	-0.052	0.036	0.072
TREAT	53,391	0.626	0.484	0.000	1.000	1.000
ARL	53,391	0.109	0.311	0.000	0.000	0.000
NEG	53,391	0.486	0.500	0.000	0.000	1.000
RET	53,391	0.134	0.721	-0.297	0.015	0.358
SIZE	53,391	5.255	2.014	3.770	5.089	6.593
MTB	53,391	2.174	1.882	1.125	1.526	2.419
LEV	53,391	0.196	0.185	0.014	0.16	0.329

Panel B: Correlation matrix

	NI	*TREAT*	*ARL*	*NEG*	*RET*	*SIZE*	*MTB*	*LEV*
NI								
TREAT	**-0.060**							
	<.0001							
ARL	**-0.049**	**0.270**						
	<.0001	**<.0001**						
NEG	**-0.210**	**0.018**	**-0.008**					
	<.0001	**<.0001**	**0.067**					
RET	**0.148**	**0.007**	**0.038**	**-0.653**				
	<.0001	**0.099**	**<.0001**	**<.0001**				
SIZE	**0.187**	**0.061**	**0.045**	**-0.104**	**-0.020**			
	<.0001	**<.0001**	**<.0001**	**<.0001**	**<.0001**			
MTB	**0.038**	**0.069**	**-0.053**	**0.123**	**-0.113**	**-0.185**		
	<.0001	**<.0001**	**<.0001**	**<.0001**	**<.0001**	**<.0001**		
LEV	-0.006	**-0.012**	**0.030**	**-0.023**	-0.005	**0.295**	**-0.287**	
	0.199	**0.004**	**<.0001**	**<.0001**	0.228	**<.0001**	**<.0001**	

This table presents summary statistics of my regression variables. In Panel A, I present the mean, standard deviation (SD), 25th percentile (Q1), median, and the 75th percentile (Q3) of the variables. In Panel B, I present the Pearson correlation for each pair of variables in the main tests. P-value is presented below each correlation coefficient. Significant correlations are indicated in bold ($p < 0.10$, two-tailed test). *NI* is defined as the income before extraordinary items of firm i in fiscal year t divided by beginning market value of equity. *TREAT* is an indicator that equals 1 for firms headquartered or incorporated in Texas, Louisiana, Alabama or Delaware, and 0 otherwise. *ARL* is an indicator that equals 1 for firm-year observations headquartered or incorporated in Texas or Louisiana during the period 1997–2003, in Alabama during the period 2001–2003, in Delaware during the period 2002–2003, and 0 otherwise. *RET* is the twelve-month buy-and-hold stock returns of firm i over fiscal year t. *NEG* is an indicator that equals 1 if RET of firm i over fiscal year t is negative and 0 otherwise. *SIZE* is the natural logarithm of total assets of firm i at the beginning of fiscal year t. *MTB* is the sum of total assets and market value of equity minus book value of equity of firm i, scaled by total assets at the beginning of fiscal year t. *LEV* is the ratio of total debt to total assets for firm i at the beginning of fiscal year t. Variable definitions are summarized in Appendix A. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 3 Baseline analysis (H1)

	TREAT = 1		Full sample	
Dep. Var. = *NI*	(1)	(2)	(3)	(4)
ARL×NEG×RET	**0.135*****	**0.063*****	**0.128*****	**0.074*****
	(6.39)	**(4.01)**	**(6.14)**	**(4.42)**
ARL×NEG	-0.013*	-0.014**	-0.011*	-0.013**
	(-1.88)	(-2.22)	(-1.68)	(-2.35)
ARL×RET	-0.024***	-0.013**	-0.030***	-0.022***
	(-3.32)	(-2.38)	(-4.05)	(-4.21)
ARL	0.035***	0.028***	0.030***	0.026***
	(6.06)	(4.08)	(5.72)	(4.85)
NEG×RET	0.078***	0.395***	0.083***	0.371***
	(3.34)	(13.56)	(3.98)	(17.56)
NEG	-0.009**	-0.017	-0.009***	-0.008
	(-2.43)	(-1.44)	(-2.72)	(-1.03)
RET	0.005	0.004	0.009***	0.021**
	(1.34)	(0.43)	(2.96)	(2.09)
SIZE×NEG×RET		0.130***		0.111***
		(3.78)		(3.82)
SIZE×NEG		0.049***		0.041***
		(3.69)		(3.90)
SIZE×RET		0.031**		0.022
		(2.49)		(1.65)
SIZE		0.116***		0.114***
		(5.74)		(6.46)
MTB×NEG×RET		-0.601***		-0.584***
		(-18.96)		(-28.45)
MTB×NEG		-0.018		-0.025***
		(-1.65)		(-3.03)
MTB×RET		0.006		-0.004
		(0.61)		(-0.38)
MTB		0.233***		0.225***
		(14.20)		(19.26)
LEV×NEG×RET		0.014		0.037
		(0.28)		(0.81)
LEV×NEG		0.001		-0.003
		(0.06)		(-0.35)
LEV×RET		0.006		-0.003
		(0.60)		(-0.32)
LEV		-0.011		-0.008
		(-0.99)		(-0.76)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	33,433	33,433	53,391	53,391
adj. R^2	0.345	0.461	0.342	0.452

This table presents my baseline results of testing the effect of ARL adoption on borrowers' accounting conservatism (H1). In Columns (1) and (2), I focus on firms headquartered or incorporated in my treatment states (i.e., Texas, Louisiana, Alabama and Delaware), while in Columns (3) and (4), I focus on the full sample which includes both treatment and control states. To test H1, I augment Basu's (1997) model of asymmetric timeliness of earnings. The dependent variable (*NI*) is defined as earnings before extraordinary items in year t scaled by beginning market value of equity. *ARL* is an indicator that equals 1 for firm-year observations headquartered or incorporated in Texas or Louisiana during the period 1997–2003, in Alabama during the period 2001–2003, in Delaware during the period 2002–2003, and

0 otherwise. *RET* is the twelve-month buy-and-hold stock returns over the firms' fiscal year t. *NEG* is an indicator that equals 1 if firms' *RET* is negative and 0 otherwise. Control variables (*SIZE*, *MTB*, and *LEV*) are measured at the beginning of the year. Variable definitions are summarized in Appendix A. All continuous variables are winsorized at the 1st and 99th percentiles. The model includes firm and year fixed effects. Presented in the parentheses below each coefficient is the t-value based on standard errors clustered by firms' headquarters state. Constant terms are estimated but omitted for presentation. ***, ** and * represent significance at the 1%, 5% and 10% levels, respectively.

Table 4 Parallel trend and dynamic effects

	TREAT = 1		Full sample	
Dep. Var. = *NI*	(1)	(2)	(3)	(4)
NEG×RET×ARL^{-2}		**0.036**		**0.042**
		(0.91)		**(1.08)**
NEG×RET×ARL^{-1}	**0.046**	**0.050**	**0.059**	**0.063**
	(0.81)	**(0.95)**	**(1.02)**	**(1.16)**
NEG×RET×ARL^{0}	**0.044***	**0.047****	**0.057****	**0.059*****
	(1.86)	**(2.27)**	**(2.41)**	**(2.74)**
NEG×RET×ARL^{1}	**0.105***	**0.118****	**0.107****	**0.116****
	(1.87)	**(2.07)**	**(2.09)**	**(2.28)**
NEG×RET×ARL^{2}		**0.114*****		**0.124*****
		(5.08)		**(5.29)**
NEG×RET×ARL$^{>=2}$	**0.050*****		**0.065*****	
	(3.89)		**(4.18)**	
NEG×RET×ARL$^{>=3}$		**0.034*****		**0.051*****
		(3.52)		**(4.80)**
NEG×RET	0.392***	0.388***	0.368***	0.365***
	(14.09)	(13.69)	(16.72)	(16.59)
NEG	-0.018	-0.020*	-0.009	-0.010
	(-1.66)	(-1.78)	(-1.19)	(-1.29)
RET	-0.001	0.001	0.019*	0.020**
	(-0.08)	(0.07)	(1.94)	(2.09)
ARL dummies×*NEG*	Yes	Yes	Yes	Yes
ARL dummies×*RET*	Yes	Yes	Yes	Yes
ARL dummies	Yes	Yes	Yes	Yes
Control variables×*NEG*×*RET*	Yes	Yes	Yes	Yes
Control variables×*NEG*	Yes	Yes	Yes	Yes
Control variables×*RET*	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	33,433	33,433	53,391	53,391
adj. R^2	0.462	0.463	0.453	0.453

This table examines the parallel trend and dynamic effects of the adoption of ARL. In Columns (1) and (2), I focus on firms headquartered or incorporated in my treatment states (i.e., Texas, Louisiana, Alabama and Delaware), while in Columns (3) and (4), I use the full sample which includes both treatment and control states. To test the parallel trend and dynamic effects of ARL adoption, I replace *ARL* with a series of relative year dummies (*ARL* ^{x}). *ARL* ^{x} equals 1 for firms whose headquarters or incorporation states will adopt ARL in *x* year(s) (*x*<0) or have adopted ARL for *x* year(s) (*x*>=0) and 0 otherwise. Variable definitions are summarized in Appendix A. All continuous variables are winsorized at the 1st and 99th percentiles. The model includes firm and year fixed effects. Presented in the parentheses below each coefficient is the t-value based on standard errors clustered by firms' headquarters state. Constant terms are estimated but omitted for presentation. ***, ** and * represent significance at the 1%, 5% and 10% levels, respectively.

Table 5 Placebo test

Dep. Var. = *NI*	Falsely treat neighboring states of TX, LA, AL, and DE (1)	Further exclude firms headquartered or incorporated in TX, LA, AL, and DE (2)
ARL*×*NEG*×*RET	**0.058**	**0.058**
	(1.48)	**(1.48)**
ARL×*NEG*	-0.031***	-0.033***
	(-2.68)	(-2.80)
ARL×*RET*	-0.013	-0.012
	(-1.31)	(-1.23)
ARL	0.023***	0.025***
	(3.81)	(4.08)
NEG×*RET*	0.379***	0.381***
	(17.44)	(16.90)
NEG	-0.010	-0.005
	(-1.17)	(-0.69)
RET	0.016	0.016
	(1.54)	(1.45)
Control variables×*NEG*×*RET*	Yes	Yes
Control variables×*NEG*	Yes	Yes
Control variables×*RET*	Yes	Yes
Control variables	Yes	Yes
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
N	53,391	47,818
adj. R^2	0.451	0.460

This table presents results of placebo test. I consider neighboring states of Texas, Louisiana, Alabama or Delaware as my treatment states. Specifically, *ARL* equals one for firms located or incorporated in Arkansas, New Mexico, Oklahoma and Mississippi during the period 1997 to 2003; *ARL* equals one for firms located or incorporated in Tennessee, Georgia, and Florida during the period 2001 to 2003; *ARL* equals one for firms located or incorporated in Maryland Pennsylvania and New Jersey during the period 2002 to 2003. In Column (1), I falsely treat the neighboring states but include the 4 states that truly adopted ARL (i.e., Texas, Louisiana, Alabama and Delaware). In Column (2), I falsely treat the neighboring states and exclude the 4 states that truly adopted ARL. Variable definitions are summarized in Appendix A. All continuous variables are winsorized at the 1st and 99th percentiles. The model includes firm and year fixed effects. Presented in the parentheses below each coefficient is the t-value based on standard errors clustered by firms' headquarters state. Constant terms are estimated but omitted for presentation. ***, ** and * represent significance at the 1%, 5% and 10% levels, respectively.

Table 6 The role of credit risk (H2)

Dep. Var. = *NI*	Distance to Default (1)	WW Index (2)	SA Index (3)
HIGHRISK×ARL×NEG×RET	**0.104*****	**0.065***	**0.128*****
	(3.00)	**(1.94)**	**(4.43)**
HIGHRISK×ARL×NEG	0.030**	0.010	0.000
	(2.18)	(0.95)	(0.03)
HIGHRISK×ARL×RET	-0.018	-0.028**	-0.039***
	(-1.33)	(-2.08)	(-3.05)
HIGHRISK×ARL	-0.013	-0.004	0.013
	(-0.97)	(-0.31)	(1.31)
HIGHRISK×NEG×RET	0.029*	0.052***	-0.039**
	(1.74)	(2.92)	(-2.42)
HIGHRISK×NEG	0.001	0.011*	0.012***
	(0.21)	(1.99)	(2.80)
HIGHRISK×RET	-0.039***	-0.015***	0.016***
	(-6.52)	(-2.79)	(2.84)
HIGHRISK	-0.010**	-0.029***	-0.036***
	(-2.58)	(-6.81)	(-8.77)
ARL×NEG×RET	-0.008	0.027	-0.008
	(-0.40)	(1.04)	(-0.39)
ARL×NEG	-0.020***	-0.015***	-0.013**
	(-3.17)	(-2.84)	(-2.23)
ARL×RET	-0.005	-0.000	0.006
	(-1.03)	(-0.05)	(0.64)
ARL	0.022***	0.024***	0.017***
	(4.99)	(4.72)	(3.08)
NEG×RET	0.321***	0.306***	0.401***
	(9.25)	(8.67)	(16.10)
NEG	-0.016*	-0.021**	-0.019**
	(-1.96)	(-2.07)	(-2.45)
RET	0.053***	0.037***	0.008
	(3.46)	(3.54)	(0.91)
Control variables×*NEG×RET*	Yes	Yes	Yes
Control variables×*NEG*	Yes	Yes	Yes
Control variables×*RET*	Yes	Yes	Yes
Control variables	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
N	40,753	53,391	53,391
adj. R^2	0.448	0.455	0.453

This table presents the results of testing the role of credit risk (H2). In Column (1), *HIGHRISK* equals 1 if the firms' distance to default (*DTD*) in year t is below the industry-year median and 0 otherwise. In Columns (2) and (3), *HIGHRISK* is defined based on Whited and Wu's (2006) *WWINDEX* and Hadlock and Pierce's (2010) *SAINDEX*, respectively: it equals 1 if the index is above the industry-year median and 0 otherwise. Variable definitions are summarized in Appendix A. All continuous variables are winsorized at the 1st and 99th percentiles. Presented in the parentheses below each coefficient is the t-value based on standard errors clustered by firms' headquarters state. Constant terms are estimated but omitted for presentation. ***, ** and * represent significance at the 1%, 5% and 10% levels, respectively.

Table 7 The role of corporate governance (H3)

Dep. Var. = *NI*	G-Index (1)	Big N Auditor (2)
GOODGOV×NEG×RET×ARL	**0.172****	**0.102*****
	(2.31)	**(3.22)**
GOODGOV×ARL×NEG	0.042**	0.018
	(2.57)	(0.60)
GOODGOV×ARL×RET	-0.043	-0.022
	(-1.24)	(-1.34)
GOODGOV×ARL	-0.003	0.015
	(-0.29)	(0.67)
GOODGOV×NEG×RET	-0.064**	0.040*
	(-2.41)	(1.88)
GOODGOV×NEG	-0.012**	0.014*
	(-2.13)	(1.79)
GOODGOV×RET	0.010	-0.002
	(1.17)	(-0.23)
GOODGOV	-0.004	0.003
	(-0.74)	(0.49)
ARL×NEG×RET	-0.016	-0.017
	(-0.23)	(-0.56)
ARL×NEG	-0.043***	-0.030
	(-3.36)	(-1.08)
ARL×RET	-0.021	-0.003
	(-0.91)	(-0.19)
ARL	0.019***	0.013
	(2.68)	(0.70)
NEG×RET	0.472***	0.351***
	(7.27)	(13.82)
NEG	0.034***	-0.016*
	(3.18)	(-1.80)
RET	0.037	0.021*
	(1.28)	(1.75)
Control variables×*NEG×RET*	Yes	Yes
Control variables×*NEG*	Yes	Yes
Control variables×*RET*	Yes	Yes
Control variables	Yes	Yes
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
N	17,603	53,391
adj. R^2	0.406	0.452

This table presents the results of testing the role of corporate governance (H3). In Column (1), *GOODGOV* equals one if G-index (*GINDEX*) in fiscal year t is below the median of all firms in the same state and year. In Column (2), *GOODGOV* equals one if a firm has engaged a Big-N auditor (*BIGN*) in fiscal year t. Variable definitions are summarized in Appendix A. All continuous variables are winsorized at the 1st and 99th percentiles. Presented in the parentheses below each coefficient is the t-value based on standard errors clustered by firms' headquarters state. Constant terms are estimated but omitted for presentation. ***, ** and * represent significance at the 1%, 5% and 10% levels, respectively.

Table 8 Robustness checks

Panel A: Alternative clustering

Dep. Var. = *NI*	Two-way clustered by headquarters state and year (1)	One-way cluster by incorporation state (2)	Two-way cluster by incorporation state and year (3)	One-way clustered by firm (4)	Two-way clustered by firm and year (5)
ARL×NEG×RET	**0.074*****	**0.074*****	**0.074*****	**0.074*****	**0.074****
	(3.07)	**(9.23)**	**(4.05)**	**(3.91)**	**(2.73)**
ARL×NEG	-0.013	-0.013***	-0.013**	-0.013	-0.013
	(-1.47)	(-6.85)	(-2.21)	(-1.59)	(-1.14)
ARL×RET	-0.022**	-0.022***	-0.022**	-0.022***	-0.022*
	(-2.77)	(-5.05)	(-2.94)	(-2.87)	(-2.08)
ARL	0.026***	0.026***	0.026***	0.026***	0.026***
	(4.83)	(6.15)	(4.42)	(4.10)	(4.10)
NEG×RET	0.371***	0.371***	0.371***	0.371***	0.371***
	(11.98)	(22.08)	(15.17)	(15.17)	(11.01)
NEG	-0.008	-0.008	-0.008	-0.008	-0.008
	(-0.89)	(-1.63)	(-1.29)	(-0.97)	(-0.86)
RET	0.021*	0.021**	0.021*	0.021***	0.021*
	(1.87)	(2.01)	(2.03)	(2.70)	(1.88)
Control variables×*NEG×RET*	Yes	Yes	Yes	Yes	Yes
Control variables×*NEG*	Yes	Yes	Yes	Yes	Yes
Control variables×*RET*	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
N	53,391	53,391	53,391	53,391	53,391
adj. R^2	0.452	0.452	0.452	0.452	0.452

Panel B: Alternative fixed effects

Dep. Var. = *NI*	(1)	(2)	(3)
ARL×NEG×RET	**0.078*****	**0.080*****	**0.078*****
	(4.31)	**(4.54)**	**(4.19)**
ARL×NEG	-0.013**	-0.014**	-0.008
	(-2.32)	(-2.50)	(-1.55)
ARL×RET	-0.021***	-0.020***	-0.022***
	(-4.11)	(-4.16)	(-4.10)
ARL	0.025***	0.035***	0.021***
	(3.67)	(5.98)	(4.07)
NEG×RET	0.371***	0.372***	0.371***
	(17.50)	(17.37)	(17.01)
NEG	-0.009	-0.009	-0.007
	(-1.11)	(-1.14)	(-0.89)
RET	0.020**	0.020*	0.018*
	(2.07)	(2.00)	(1.97)
Control variables×*NEG×RET*	Yes	Yes	Yes
Control variables×*NEG*	Yes	Yes	Yes
Control variables×*RET*	Yes	Yes	Yes
Control variables	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Headquarters state × year fixed effects	Yes		
Incorporation state × year fixed effects		Yes	
Industry × year fixed effects			Yes
N	53,373	53,307	52,855
adj. R^2	0.451	0.450	0.478

Panel C: Alternative ways of defining the ARL dummy

Dep. Var. = *NI*	Solely based on headquarters state (1)	Solely based on incorporation state (2)	Ignoring the 2003 federal court ruling (3)
ARL×NEG×RET	**0.044*****	**0.068*****	**0.044*****
	(3.50)	**(3.44)**	**(3.06)**
ARL×NEG	-0.006	-0.020***	-0.005
	(-0.93)	(-2.69)	(-1.10)
ARL×RET	-0.007	-0.024***	-0.023***
	(-1.22)	(-4.66)	(-3.73)
ARL	0.019***	0.026***	0.022***
	(3.21)	(3.92)	(3.64)
NEG×RET	0.380***	0.372***	0.368***
	(17.34)	(17.80)	(17.14)
NEG	-0.010	-0.008	-0.008
	(-1.24)	(-0.99)	(-1.00)
RET	0.016	0.021**	0.024**
	(1.50)	(2.12)	(2.50)
Control variables×*NEG×RET*	Yes	Yes	Yes
Control variables×*NEG*	Yes	Yes	Yes
Control variables×*RET*	Yes	Yes	Yes
Control variables	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
N	53,391	53,391	53,391
adj. R^2	0.451	0.452	0.452

Panel D: Alternative samples

Dep. Var. = *NI*	Restricting the sample period to 1994-2003 (1)	Excluding states adopted ARL after 2003 (SD, VA, and NV) (2)	Excluding Delaware (3)	Excluding Texas (adopted universal demand laws in 1997) (4)
ARL×NEG×RET	**0.062*****	**0.077*****	**0.069*****	**0.072*****
	(3.98)	**(4.58)**	**(3.32)**	**(3.80)**
ARL×NEG	-0.012*	-0.013**	-0.014	-0.013**
	(-1.91)	(-2.25)	(-1.42)	(-2.10)
ARL×RET	-0.020***	-0.025***	-0.008	-0.021***
	(-3.25)	(-5.08)	(-0.84)	(-3.78)
ARL	0.027***	0.025***	0.014	0.025***
	(3.85)	(4.71)	(0.86)	(4.30)
NEG×RET	0.358***	0.368***	0.342***	0.368***
	(15.93)	(16.68)	(13.38)	(15.82)
NEG	-0.014	-0.011	0.002	-0.008
	(-1.32)	(-1.52)	(0.18)	(-0.98)
RET	0.034***	0.020**	0.042***	0.021**
	(3.52)	(2.39)	(3.27)	(2.06)
Control variables×*NEG×RET*	Yes	Yes	Yes	Yes
Control variables×*NEG*	Yes	Yes	Yes	Yes
Control variables×*RET*	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	37,154	50,512	21,677	52,347
adj. R^2	0.473	0.452	0.424	0.452

Panel E: Alternative specification to test the effect of ARL on accounting conservatism

Dep. Var. =	(1) *CSCORE*	(2) *CSCORE*	(3) *CONACC*	(4) *CONACC*
ARL	**0.012*****	**0.006*****	**0.010*****	**0.010****
	(3.72)	**(3.60)**	**(2.75)**	**(2.67)**
SIZE		-0.030***		-0.030***
		(-18.53)		(-20.41)
MTB		-0.031***		0.001*
		(-41.64)		(1.87)
LEV		0.392***		0.029***
		(79.05)		(4.38)
LIT		-0.004		0.009
		(-0.85)		(1.15)
SG		-0.001		-0.008***
		(-1.38)		(-6.95)
RDAD		0.000		-0.003**
		(0.44)		(-2.35)
CFO		0.026***		-0.033***
		(8.39)		(-7.11)
STDREV		0.002		-0.017***
		(1.23)		(-3.50)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	48,755	48,755	51,815	51,815
adj. R^2	0.669	0.773	0.530	0.552

This table presents robustness checks for my baseline results. Panel A reports the results of using different clustering methods and Panel B reports results of using alternative fixed effects. In Panel C, I define *ARL* in alternative ways. In the first (second) column, ARL equals one for firms headquartered (incorporated) in Texas or Louisiana during the period 1997–2003, in Alabama during the period 2001–2003, and in Delaware during the period 2002–2003, and 0 otherwise. In Column (3), I ignore the 2003 federal court ruling and consider all seven states that have adopted ARL as my treatment states (i.e., Texas, Louisiana, Alabama, Delaware, South Dakota, Virginia, and Nevada). Panel D reports results based on alternative samples. Panel E reports results using alternative measures of conservatism, including a firm-year-level conservatism score (*CSCORE*) proposed by Khan and Watts (2009) in Columns (1)-(2) and an unconditional conservatism measure (*CONACC*) proposed by Ahmed et al. (2002) in Columns (3)-(4). Variable definitions are summarized in Appendix A. All continuous variables are winsorized at the 1st and 99th percentiles. Presented in the parentheses below each coefficient is the t-value based on standard errors clustered by firms' headquarters state (except Panel A). Constant terms are estimated but omitted for presentation. ***, ** and * represent significance at the 1%, 5% and 10% levels, respectively.