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Abstract

We exploit an influential 1991 Delaware court ruling to examine simultaneously two types of conservatism that play important roles in resolving creditor-owner agency conflicts: contracting conservatism and reporting conservatism. The ruling expanded managerial fiduciary duties in favor of creditors for Delaware-incorporated firms in the vicinity of insolvency. In those firms, following the ruling, debt contracts are less likely to include conservative adjustments to accounting numbers used for covenant compliance (i.e., contracting conservatism decreases), while public financial reporting becomes more conservative (i.e., reporting conservatism increases). The decrease in contracting conservatism is concentrated in firms that exhibit a greater increase in reporting conservatism, suggesting that reporting conservatism is more cost effective in resolving agency conflicts. In addition, the substitution effect is more pronounced in firms facing greater business uncertainty and firms with greater board independence.

1. Introduction

We examine the relation between two applications of accounting conservatism: the preparation of financial statement numbers in accordance with generally accepted accounting principles (GAAP), and the writing of contracts that use such numbers. Financial reporting conservatism encompasses the asymmetric timeliness of loss recognition over gain recognition in accounting earnings. Contracting conservatism means the use of reported numbers in contracts in a conservative manner – e.g., in debt agreements, giving more weight to losses over gains, or more weight to tangible over intangible assets – when measuring covenant compliance. Since both approaches can be used to resolve creditor-owner agency conflicts, the question as to whether they are complements or substitutes has been a matter of debate and, given the endogenous nature of reporting and contracting, difficult to address empirically.

Starting with Basu (1997), an extensive stream of accounting literature has examined conservatism in financial reporting. Watts (2003) proposes that lenders' demand for conservatively reported accounting numbers is an important driver for the prevalence of reporting conservatism observed in practice. Schipper (2005) and Guay and Verrecchia (2006), however, note that an alternative approach for lenders to protect their interests is to write their contracts more conservatively without relying on management-provided conservative financial numbers.

Each mechanism has its relative strength and weakness. On the one hand, financial reports are updated more frequently than contracts and therefore can reflect new information in a timelier manner. However, lenders may not want to rely entirely on reported accounting numbers even if they are prepared conservatively, because of an "enforcement" problem: borrowers are responsible for the preparation of financial statements and lenders cannot be certain that these financial statements will continue to be prepared with the lenders' desired level of conservatism after the loans have been provided. On the other hand, lenders can directly require adjustments in contracts

to accommodate their demand for conservatism. However, writing such contracts also imposes costs because lenders cannot foresee all future eventualities and thus cannot pre-specify very precisely all desired contracting adjustments.

Disentangling the relation between contracting conservatism and reporting conservatism is challenging because, in equilibrium, both mechanisms are at work simultaneously. Existing studies so far have provided mixed evidence (e.g., Beatty et al. 2008).[1] We exploit an influential 1991 Delaware (DE) court ruling by the Delaware Court of Chancery in the *Credit Lyonnais v. Pathé Communications* case to investigate this issue. The ruling was widely viewed as having shifted the balance of fiduciary duties of management towards creditors and away from shareholders, and can be considered as an exogenous shock to the creditor-owner agency conflict.[2] As a result of the shock, contracting parties are likely to re-evaluate the relative effectiveness of each mechanism and possibly adjust each type to a new level. We are therefore able to provide more causal inferences about the relation between the two conservative approaches by observing adjustments made following the ruling.

We argue that the "enforcement" problem of voluntary conservative reporting decreases after the ruling because officers and directors are bound by their expanded fiduciary duties to act in the interests of creditors (rather than doing so voluntarily), which include supplying conservative financial reports. From the lenders' perspective, the reporting channel becomes more cost effective than contracting in resolving the agency conflicts and, therefore, we expect to see an

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[1] "Depending on the setting, the authors' evidence suggests that covenant adjustments may be a substitute for financial reporting conservatism, a complement to financial reporting conservatism, or bear no relation with examined measures of financial reporting conservatism" (Guay 2008, p. 176, from his discussion of Beatty et al. 2008).

[2] Before the 1991 *Credit Lyonnais* case, the prevailing view on managerial fiduciary duties was that officers and directors of solvent corporations owed fiduciary duties to shareholders but not to creditors. If the firm became insolvent, the fiduciary duties would extend to corporate creditors, who could then sue for breach of fiduciary duties if the officers and directors did not act in their interest. The 1991 *Credit Lyonnais* ruling changed this by explicitly requiring directors to act in creditors' interests even if the firm is solvent but is in the "vicinity of insolvency".

increase in reporting conservatism accompanied by a decrease in contracting conservatism following the ruling (i.e., substitution occurs while a new equilibrium is formed).

To test our hypothesis, we hand-collect 278 credit agreements and borrowers' financial statement information from the period surrounding the *Credit Lyonnais* ruling. We simultaneously examine changes in: (1) contracting conservatism as revealed by debt contract features, and (2) reporting conservatism in GAAP financial statements produced by borrowers.

We first investigate whether and how contracting conservatism changes following the ruling. We examine the use of income escalators (provisions in loan contracts which require changes in net worth to reflect losses in full, but only partially for gains and profits), the exclusion of acquired intangibles when calculating net worth, and the use of contractual adjustments to GAAP numbers which result in greater conservatism (for example, stipulating the exclusion of extraordinary gains but not extraordinary losses). We find significantly lower use of income escalators, tangible net worth covenants, and conservative definitions for affected firms than for unaffected firms after the ruling. This result suggests that contracting parties rely less on contracting conservatism following a shift in fiduciary duties favoring creditors.

We then examine changes in reporting conservatism. Our analysis is an extension of Aier et al. (2014) who also examine changes in conservative financial reporting following the *Credit Lyonnais* ruling. Aier et al. (2014) measure conservatism with a general composite measure of decile rankings of firm-year observations based on averages of three reporting conservatism proxies (negative non-operating accruals, skewness of earnings vs. cash flows, and unrecorded balance sheet reserves). We first replicate their results using the composite conservatism measure in our sample, and then expand their analysis by focusing on specific accounting choices, namely discretionary accruals and negative special items. The discretionary accruals models that we

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implement allow us to examine changes in accounting that are not driven by changes in firms' investing behaviors following *Credit Lyonnais* (Becker and Strömberg 2012).[3]

Consistent with our prediction, we find a higher composite conservatism score, more negative discretionary accruals, and more negative special items in affected firms after the ruling, indicating greater conservatism in the reported numbers. The economic magnitudes of these changes are significant. The results provide insights into mechanisms supporting the debt contracting explanation for conservatism posited in Watts (2003) and complement the results of Aier et al. (2014). Our findings suggest that officers and directors, facing a greater duty of care and loyalty towards creditors, report more conservative accounting numbers in the firm's public filings, thus demonstrating that they are acting in creditors' interest.

When we interpret our results on contracting and reporting conservatism jointly, our findings are consistent with the conjecture that reporting conservatism becomes relatively more cost effective than contracting conservatism in resolving the agency conflicts following a shift in fiduciary duties towards creditors. We observe a substitution of contracting conservatism by reporting conservatism as creditors gain more power.

We further validate the substitution effect by directly exploring the interaction between the two types of conservatism. We examine changes in reporting conservatism around the ruling and find that the substitution effect is more pronounced for firms that have experienced an increase in reporting conservatism after the ruling.

To further understand the substitution effect and its underlying cause, we conduct cross-sectional analyses. The substitution effect should vary with the degree to which the *Credit*

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[3] Becker and Strömberg (2012) document that firms affected by the *Credit Lyonnais* ruling (i.e., DE-incorporated firms close to financial distress) issue more equity and make greater capital and R&D investments, and there is lower volatility in their accounting and market returns after the ruling was passed.

Lyonnais ruling helped resolve or mitigate the *ex-ante* enforcement problem. We consider *ex-ante* firm characteristics that reflect the relative strengths and weaknesses of the two conservatism mechanisms: business uncertainty and board independence. *Ex-ante* uncertainty imposes costs on lenders since it makes writing complete contracts more difficult and subsequent renegotiation more likely (Roberts and Sufi 2009; Roberts 2015; Nikolaev 2018). Lenders to firms facing greater business uncertainty stand to gain more from the improved alignment of interests arising from *Credit Lyonnais* since they can reduce their reliance on relatively costly mechanisms involving contracting conservatism. We expect therefore more substitution of contracting by reporting conservatism in affected firms with greater *ex-ante* uncertainty. Board independence, on the other hand, is related to the effectiveness of monitoring of the financial reporting process (e.g., Beasley 1996; Klein 2002), and firms with more independent boards report more conservatively (Ahmed and Duellman 2007). Since the increase in reporting conservatism is concentrated (i.e., the enforcement problem is more successfully mitigated) in firms with more independent boards (Aier et al. 2014), we expect the substitution effect to be more pronounced in firms with greater board independence. Our cross-sectional analyses indicate that the substitution effect is indeed more pronounced in affected firms with greater *ex-ante* business uncertainty and more independent boards.

The evidence in this paper sheds light on how contracting parties trade-off the two different types of conservatism to achieve an equilibrium. In contrast to Beatty et al. (2008), whose conclusions lean towards complementarity (despite their mixed overall results), our study documents substitution, reflecting adjustments to the two mechanisms during transition (i.e., changing from an old equilibrium to a new one as a result of the ruling). Furthermore, our cross-sectional analyses suggest that substitution may not apply in all scenarios, but is more likely

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to occur in firms facing greater *ex-ante* business uncertainty (where writing a complete contract is more costly) and with greater board independence (where the enhanced fiduciary duties resulting from the ruling are more likely to be followed).

This paper makes at least two substantial contributions to the literature. First, it contributes to the debate about whether contractually negotiated conservative features of debt contracts provide an adequate mechanism to satisfy lenders' demand for conservatism. Our findings suggest that lenders do take into account the degree of reporting conservatism provided by borrowing firms in their GAAP financial statements and adjust their use of contractually negotiated conservative adjustments accordingly.

Second, our paper contributes to the literature on agency conflicts between shareholders and debtholders, and the relative strength of the fiduciary duty that management owes to these parties. Building on the investing and financing angles studied by Becker and Strömberg (2012) and the initial evidence in Aier et al. (2014), we examine the impact of changes in fiduciary duties on specific accounting and contracting choices.

The rest of the paper is laid out as follows. Section 2 develops hypotheses. Section 3 describes the sources of data, research design and presents main results. Sections 4 and 5 provide additional and robustness analyses. Section 6 concludes.

2. Hypothesis development

Delaware court ruling: Credit Lyonnais vs. Pathé Communications

The *Credit Lyonnais* ruling was reached on December 30, 1991 and generated a great deal of press coverage and commentaries by legal experts. Prior to the case, the general understanding under Delaware law was that managers and directors did not owe fiduciary duties to creditors prior

to insolvency. The significance of *Credit Lyonnais* is that it expanded the scope of directors' fiduciary duties to parties other than shareholders even before the corporation becomes insolvent.[4]

The case arose out of the leveraged buyout of MGM, which was primarily financed by Credit Lyonnais. The newly formed private company had immediate trouble meeting its financial obligations. Only five months after the acquisition, MGM secured an additional credit line of $145 million from Credit Lyonnais. At the same time, Pathé Communications (MGM's controlling shareholder) and Credit Lyonnais entered into a corporate governance agreement that permitted Credit Lyonnais to gain control through the nomination of MGM directors. Credit Lyonnais exercised its rights under this agreement and appointed a new CEO and directors.

The corporate governance agreement also allowed Pathé to regain voting rights of the controlling shares of MGM and potentially remove bank-designated directors if the debt could be sufficiently paid down. In an attempt to do so, Pathé sought to sell MGM's interest in an overseas subsidiary. MGM's directors did not approve the sale, and Pathé sued claiming that the new management and directors were in breach of the fiduciary duty they owed to Pathé by favoring creditors' interest.

In December 1991, the court ruled that the new management "was not disloyal in not immediately facilitating whatever asset sales were in the financial best interest of the controlling shareholders" (section ** of the ruling). The court held that "the new management was appropriately mindful of the potential differing interests between the corporation and its controlling shareholder. At least where a corporation is operating in the vicinity of insolvency, a

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[4] Although technically the ruling referred to all stakeholders, the focus in subsequent discussions and studies by the press, legal scholars, lawyers, and other legal cases is almost always on creditors. For example, see Geyer vs. Ingersoll Publications 1992; Weaver vs. Kellogg 1997; and Medlin vs. Wells Fargo Bank 2007. For the text of the ruling, see Memorandum Opinion, Civ. A. No. 12150, Court of Chancery of Delaware, New Castle County. For further institutional details surrounding the case, see Becker and Strömberg (2012).

board of directors is not merely the agent of the residue risk bearers, but owes its duty to the corporate enterprise" (section *34 of the ruling).[5]

The ruling came as a surprise to the business and legal community and immediately triggered widespread media coverage.[6] It is therefore reasonable to view the ruling as an exogenous shock to managerial fiduciary duties. Despite uncertainty about the exact definition of "vicinity" of insolvency in the ruling, it is commonly viewed that the ruling has shifted the balance of power towards creditors in distressed situations (Becker and Strömberg 2012). Since the ruling only applied to Delaware-incorporated firms in the vicinity of insolvency, it enables us to employ a difference-in-difference identification strategy and compare outcomes of affected and unaffected firms before and after 1991.

Empirical predictions

The conflict between the general providers of capital to firms – lenders and owners – has been well acknowledged in the corporate finance literature for decades (e.g., Jensen and Meckling 1976; Myers 1977). Becker and Strömberg (2012) note that this conflict can be particularly acute in times of financial distress when management, acting on behalf of shareholders, has the incentive to underinvest (since value gains will go to lenders first) or, when they do invest, opt for riskier projects (to increase the probability of owners receiving a gain after paying off lenders). Becker and Strömberg (2012) utilize the *Credit Lyonnais* case as an exogenous shock to the creditor-owner conflict. Their study predicts and documents that affected firms

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[5] Footnote 55 of the ruling states that the possibility of insolvency can expose creditors to risks of opportunistic behavior if the fiduciary duties of directors are owed to shareholders only. It states that "in managing the business affairs of a solvent corporation in the vicinity of insolvency, circumstances may arise when the right […] course to follow for the corporation may diverge from the choice that the stockholders […] would make if given the opportunity to act" (footnote 55 of *Credit Lyonnais* ruling).

[6] According to Becker and Strömberg (2012), 24 newspapers covered the case and ruling on the day of or day following the ruling and, within the next three months, the case was covered more than 60 times by the press.

(i.e., those incorporated in Delaware and closer to insolvency) invest more, but at the same time experience less volatile cash flows, compared to unaffected firms. This behavior is consistent with the predictions of the classic models since it reflects a reduced tendency by firms to invest for the benefit of shareholders at the expense of lenders.

The results in Becker and Strömberg (2012) suggest that the *Credit Lyonnais* case may also be useful to better understand how accounting numbers are prepared and used in lending agreements. The conflict between lenders and owners affects not only investing behavior, but also financial reporting (Watts and Zimmerman 1986). While lenders negotiate contracts to protect themselves, they must still delegate the financial reporting function to the borrowing firm's management. Thus, not only is there an agency problem related to investment, lenders also risk distortion of financial reporting by management.

The accounting literature has considered two alternative mechanisms that can be used to mitigate such a conflict. One alternative (reporting conservatism) is to pressure management into a reporting system that aligns with the payoff function of lenders, e.g., a model where bad news is communicated on a more timely basis than good news, since lenders are less concerned about the upside which will belong to the owners. Ball (2001) and Watts (2003), among others, have made this argument to explain the format that accounting principles have followed historically, with asymmetries in the form of timely loss recognition (e.g., impairments) and non-recognition of contingent gains. Empirically, a long line of literature followed Basu (1997) to demonstrate this behavior in practice, especially in debt contracting (e.g., Ahmed et al. 2002; Ball and Shivakumar 2005; Nikolaev 2010; Tan 2013; Wittenberg-Moerman 2008; Zhang 2008).

In contrast, other scholars (e.g., Schipper 2005; Guay and Verrecchia 2006) have argued that, to counter management's incentives to bias upwards, lenders could also write contracts more

conservatively (contracting conservatism). For example, when measuring covenant compliance, debt agreements may give more weight to losses over gains, or more weight to tangible over intangible assets. Beatty et al. (2008) document the prevalence of such contracts in practice.

The use of these two conservatism mechanisms in resolving agency problems in debt contracting has been debated in the literature. The strength of the financial reporting approach is that financial reports are updated more frequently than contracts and can reflect new information on a timelier basis and more effectively. For example, public filings are updated at least every fiscal quarter in the U.S., whereas loan contracts are written only when initiated or renegotiated. Moreover, the measurement rules in loan contracts are predetermined and cannot fully incorporate new information after they have been written. However, reporting conservatism has an "enforcement" problem because it is the manager who supplies reporting conservatism and lenders cannot be certain that they will continue to receive the desired reporting conservatism after they have provided the loans. The strength of contracting conservatism, on the other hand, is that lenders can directly incorporate in the contract conservative modifications to covenants meeting their specific requirements. However, it is costly to write such contracts because lenders cannot foresee all future eventualities. Since it is inefficient and costly to design a contract that transfers decision rights either too frequently or too infrequently (e.g., Leuz 2001; Guay 2008), adjustments in the contract are usually kept very crude (i.e., incomplete contracts), which cannot accurately reflect bad news on a timely basis.

Beatty et al. (2008) have examined the relation between reporting and contracting conservatism using a cross-sectional association analysis. While their evidence leans towards a complementary relation, their results overall are mixed as covenants that ignore intangibles when calculating net worth are not associated with conservative reporting. The inconclusive findings are

likely due to endogeneity embedded in their cross-sectional association analysis, since reporting and contracting choices are made simultaneously in an equilibrium.

The *Credit Lyonnais* ruling provides an opportunity to tackle this difficult question. We exploit the ruling to extend Beatty et al. (2008) in a quasi-experimental setting in which changes in creditor-owner agency conflicts are exogenous (Becker and Strömberg 2012). Contracting parties are likely to re-evaluate the relative effectiveness of each mechanism in response to the exogenous change in agency conflicts and form a new equilibrium with each mechanism adjusting to its appropriate level. We are therefore able to provide more causal inferences about the true interdependent relation between the two conservative approaches by observing adjustments made to these approaches after the ruling.

Aier et al. (2014) also utilize *Credit Lyonnais* but focus on the financial reporting angle only. They find that reporting conservatism, as measured using decile rankings of firm-year observations based on averages of three conservatism proxies (negative non-operating accruals, skewness of earnings vs. cash flows, and unrecorded balance sheet reserves), increases following the ruling for Delaware distressed firms only. We first replicate Aier et al.'s (2014) results in our debt contract sample and then expand on those results by examining specific accounting choices, namely, negative accruals and special items. More importantly, we link the reporting change to the contracting change studied by Beatty et al. (2008). This linkage is left unexplored by Aier et al. (2014), and yet, as previously noted, accounting scholars have been divided on how such an interaction would manifest empirically.

We conjecture that the "enforcement" problem of voluntary conservative reporting decreased after the ruling because managers were bound by their expanded fiduciary duties to act in the interests of creditors, which include supplying conservative financial reports. From the

lenders' perspective, the reporting channel becomes more cost effective than contracting in resolving the agency conflicts and, therefore, we expect to see an increase in reporting conservatism along with a decrease in contracting conservatism following the ruling (i.e., substitution occurs while a new equilibrium is formed). We state our first hypothesis regarding both reporting and contracting conservatism as follows:

> HYPOTHESIS 1 (Changes in reporting and contracting conservatism): *After the Credit Lyonnais ruling, firms incorporated in Delaware and in the vicinity of insolvency exhibit more reporting conservatism and less contracting conservatism compared to firms incorporated in other states or Delaware firms not in the vicinity of insolvency.*

The substitution effect of reporting conservatism for contracting conservatism following the ruling is likely to vary cross-sectionally with *ex-ante* firm characteristics reflecting the relative strengths and weaknesses of the two conservatism mechanisms. Our cross-sectional tests are based on two constructs expected to affect the substitution between the two types of conservatism, one related to contracting and the other to financial reporting. Specifically, we consider factors likely to affect the enforcement costs of contracting conservatism and the effectiveness of lender monitoring making use of financial reporting conservatism. First, in relation to contracting conservatism, we consider variations in borrowers' business volatility. Prior research shows that exogenous uncertainty is an important driver of debt contract renegotiation (Roberts and Sufi 2009; Roberts 2015; Nikolaev 2018). High uncertainty makes it difficult to write complete initial contracts because of the large number of contingencies that cannot all be anticipated. Since officers and directors of borrowing firms are bound by greater fiduciary duties favoring creditors following *Credit Lyonnais*, lenders to firms facing higher uncertainty can reduce reliance on relatively costly

mechanisms involving contracting conservatism. Thus, we expect the substitution effect to be more pronounced for firms with greater *ex-ante* uncertainty. Stated formally:

> HYPOTHESIS 2A (moderating effect of *ex-ante* uncertainty): *The substitution of reporting conservatism for contracting conservatism following the Credit Lyonnais ruling is more pronounced in firms with greater ex-ante uncertainty.*

Next, in relation to reporting conservatism, we examine the cross-sectional effect of board independence. Prior studies document that more independent boards of directors are more effective in monitoring the financial reporting process (Beasley 1996; Klein 2002) and, in particular, firms with more independent boards report more conservatively (Ahmed and Duellman 2007). Since the increase in financial reporting conservatism following *Credit Lyonnais* is concentrated in firms with more independent boards (Aier et al. 2014), we expect the substitution effect to be more pronounced for firms with greater board independence. Stated formally:

> HYPOTHESIS 2B (moderating effect of board independence): *The substitution of reporting conservatism for contracting conservatism following the Credit Lyonnais ruling is more pronounced in firms with greater board independence.*

3. Data and analyses

Sample selection

To test Hypothesis 1 about contracting conservatism, we hand-collect a sample of loan contracts disclosed by borrowing firms in various SEC filings. For tractability, we restrict loan contracts to the years 1989 to 1994 (three years before and after the ruling). We describe the data collection procedures in more detail in Appendix 2. The loan contracts sample consists of 278

private loan contracts.[7] We extract data on debt covenant terms, noting any conservative modifications to accounting numbers involving asymmetric treatments of gains and losses, as well as the treatment of intangible assets in net worth measures. To examine borrowing firms' reporting conservatism, we also collect financial statement data from Compustat/CRSP corresponding to what would have been available on the dates the loan contracts are signed. We obtain data on board of directors from Compact Disclosure.

Empirical strategy

We examine contracting and reporting conservatism using the following difference-in-difference regression specification:

$$Y_{it} = \beta_0 + \beta_i \textit{Afterlaw}_t \times \textit{Affected}_i + \gamma \textit{Affected}_i + \delta' \mathbf{X}_{it} + \textit{Industry FE} + \textit{Year FE} + \varepsilon_{it} \qquad (1)$$

Y refers to the various proxies for contracting and reporting conservatism, *i* indexes firms and *t* indexes time (in years). *Afterlaw* is an indicator variable equal to one for the years 1992 to 1994, and zero otherwise. *Affected* is also an indicator variable, equal to one if the firm is DE-incorporated and close to insolvency, and zero otherwise.[8] $\mathbf{X}_{it}$ represents a vector of control variables. *Industry FE* denotes industry fixed effects, *Year FE* year fixed effects, and ε_{it} the residual. As in Becker and Strömberg (2012), we do not include a separate *Afterlaw* indicator as it

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[7] To alleviate concerns about generalizability of the results documented using this sample, we note that the loan contracts were selected randomly (please refer to Appendix 2 for details). Furthermore, for all results on reporting conservatism reported in the paper which use this sample, we also conduct the same analyses using the entire Compustat universe (untabulated) since the variables used in reporting conservatism tests can be collected for all firms on Compustat. Our conclusions on reporting conservatism continue to hold in the general sample.

[8] Following Becker and Strömberg (2012), our main measure of proximity to insolvency is Merton's distance-to-default measure. Merton's (1974) model uses the market value of a firm's equity in calculating default risk. We construct the distance-to-default measure following Vassalou and Xing (2004), who employ Merton's model to estimate the value of contingent claims on the firm's assets. We classify firms with Merton measure below the median value in 1990 (i.e., immediately before the ruling) as being closer to insolvency.

is absorbed in the year fixed effects. In all our regressions, we cluster standard errors at the state of incorporation and year level, whenever possible.

Our identification strategy benefits from the fact that the *Credit Lyonnais* ruling set a precedent for DE-incorporated firms but had no prejudicial power for other firms. The impact and scope of the ruling continue to be debated and interpreted in subsequent Delaware cases.[9] Our identification draws the juristic distinction between DE-incorporated firms and non-DE-incorporated firms. To the extent that other state jurisdictions may incorporate the results of Delaware rulings, this works against finding a significant treatment effect.

Proxies for contracting and reporting conservatism

Proxies for contracting conservatism

We use three proxies for contracting conservatism: the use of income escalators, tangible net worth covenants (Beatty et al. 2008) and conservative definitions of net income, cash flows, or net worth (Li 2010). The typical income escalator clause stipulates changes to net worth calculations which incorporate future profits only partially and future losses in full. The first dependent variable, *D(Escal)*, is an indicator variable equal to one if the net worth covenant includes an income escalator, and zero otherwise. The second dependent variable, *D(Tang)*, is an indicator equal to one if the definition of net worth used in contracts includes only tangible assets, and zero otherwise. The third dependent variable, *D(ConsDef)*, is an indicator equal to one if the definition of income, cash flow or net worth in a debt contract includes specific line items (e.g., extraordinary items) for losses but not for gains, and zero otherwise.

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[9] Later rulings in Production Resources (2004) and Gheewalla (2007) introduced partial reversals of *Credit Lyonnais* by removing creditors' ability to sue directors and officers for fiduciary duty breaches (Becker and Strömberg 2012). When we examine changes in both contracting and reporting conservatism following these rulings, we do not observe reversals of our main results.

Proxies for reporting conservatism

We also construct three proxies to examine changes in reporting conservatism following the *Credit Lyonnais* ruling: a composite conservatism score as used in Aier et al. (2014) (*Crank*), discretionary accruals, and negative special items. *Crank* is a composite measure of decile rankings of firm-year observations based on averages of three conservatism proxies (negative non-operating accruals, skewness of earnings vs. cash flows, and unrecorded balance sheet reserves). We use *Crank* to confirm Aier et al.'s (2014) finding in our sample of firms with loan contracts.

To shed further light on how reporting conservatism might have changed, we examine two accounting choices made by managers: discretionary accruals (*DA-DD*) and special charges (*SPI/AT*). We use a discretionary accrual measure based on the cross-sectional Dechow and Dichev (2002) model, as modified by McNichols (2002) and Francis et al. (2005). We estimate the following model by industry-year using all available firm-years from Compustat/CRSP over our sample period 1989 to 1994, requiring at least 20 observations (industries are based on the Fama-French 48 industries classification):

$$TA_{it} = \alpha_0 + \alpha_1 CFO_{it-1} + \alpha_2 CFO_{it} + \alpha_3 CFO_{it+1} + \alpha_4 \Delta Rev_{it} + \alpha_5 PPE_{it} + \varepsilon_{it} \qquad (2)$$

In equation (2), TA_{it} is total accruals of firm i in year t (measured as earnings before extraordinary items and discontinued operations minus operating cash flows (from continuing operations) taken directly from the statement of cash flows, scaled by average total assets). *CFO* is operating cash flow (measured as the sum of net income, depreciation and amortization, and changes in current liabilities, minus changes in current assets, scaled by average total assets); ΔRev_{it} is the annual change in revenues scaled by average total assets; and PPE_{it} is property, plant, and equipment scaled by average total assets. The residuals from equation (2) represent

discretionary accruals not associated with operating cash flows that cannot be explained by change in revenue and level of PPE.

We also examine income-decreasing special charges, such as impairments and restructurings.[10] These special charges are a set of observable accounting accruals that directly communicate bad economic news to financial statement users. We examine the total (signed) amount of special items booked.[11]

Control variables

In the regressions examining contracting conservatism, we follow Beatty et al. (2008) and control for both contract-specific and firm-specific characteristics. For contract-specific controls, we include covenant intensity (number of financial covenants in the loan contract), loan maturity, loan size (scaled by lagged total assets), spread above LIBOR, and indicator variables for inclusion of performance pricing and for revolver loans. For firm-specific controls, we include firm size (log of market value of equity), firm performance (ROA and stock returns), credit rating, current accruals and growth rate in sales.

In reporting conservatism regressions, we follow Aier et al. (2014) and control for firm size (log of market value of equity), firm performance (ROA and stock returns), firm leverage to capture change in financial risk, and institutional ownership as a proxy for governance quality. In addition, we consider changes in the real investment and financing decisions made by DE-incorporated firms that Becker and Strömberg (2012) attribute to the *Credit Lyonnais* ruling. It is

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[10] Prior to 2001, Compustat does not have fields relating separately to asset impairments and restructuring charges. Also, we do not examine extraordinary items separately because they are extremely rare.

[11] Mechanically, there might be some overlap between special items and our discretionary accrual measures. For example, if the special items include inventory impairments or the establishment of short-term reserves for restructuring liabilities, those transactions could be reflected in accruals. The correlation between the special items and negative accruals is around 0.35, suggesting that they are related but not redundant.

possible that these corporate changes drive changes in reporting behavior, as opposed to these reporting changes being directly affected by the legal ruling, *per se*. Controlling for these forces provides a clearer, and thus more causal, interpretation about how the change in fiduciary duty affects financial reporting.

More specifically, the Delaware court ruling may affect corporate behavior (e.g., capital structure) and the risk profile of the company, which in turn may affect how the accounting system translates economic events to the financial statements. For example, Myers (1977) describes the debt overhang problem in which equity holders may be reluctant to raise new equity capital for positive NPV investments when most of the return on investment would go to creditors. Becker and Strömberg (2012) show that investment and equity issuance increase for firms affected by *Credit Lyonnais*, which they interpret as indicating a reduction in the debt overhang problem. To ensure any financial reporting effects we identify are not driven by the change in affected firms' financing and investing behaviors, we control for investment and equity issuance. We measure investment by capital expenditure divided by total assets.[12] Following Baker, Stein and Wurgler (2003), equity issuance is an indicator variable equal to one if stock issuance is positive, where stock issues are defined as the change in book equity minus changes in retained earnings adjusted for deferred taxes, normalized by lagged assets.

A second concern is the risk substitution problem, in which equity holders may have incentives to increase the riskiness of existing assets (Jensen and Meckling 1976). Becker and Strömberg (2012) find that performance volatilities decrease for firms affected by the court ruling, which they interpret as indicating a decline in the risk substitution problem. We therefore control

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[12] We obtain very similar results when we further control for R&D/Assets.

for the change in risk profile of the company using equity volatility, which is calculated as the annualized monthly standard deviation of the stock return.

Descriptive statistics

Table 1 presents descriptive statistics for our loan contracts sample. Since there are few studies of loan contracts from early time periods before the widespread availability of machine-readable filings (which started in 1994), we highlight comparisons with a more recent sample which is widely used in academic research (Nini, Smith, and Sufi 2009).

Table 1, panel A shows the distribution of contracts by year of signing and DE vs. non-DE. Of the 278 contracts collected, 158 are by DE firms and 120 by non-DE firms. After reading all the contracts, we ascertain that 204 (73%) of these contracts contain net worth covenants. Panel B compares loan characteristics between our hand-collected sample (loans from 1989 to 1994) and the sample from Nini et al. (2009) (loans from 1996 to 2005).

The comparison reveals several interesting patterns. First, our sample picks up smaller loans than Nini et al.'s (mean of $96 million compared to $450 million). Two factors contribute to this difference. (1) Nini et al.'s sample only includes contracts that contain a full table of contents according to their text search algorithm. Loans with a full table of contents tend to be larger (Nini et al. 2009). Our sample selection rules, however, do not contain such a bias. (2) The starting point for the Nini et al. sample is the *Dealscan* database from Loan Pricing Corporation, which has a focus on syndicated loans, whereas our procedure samples directly from companies' regulatory filings regardless of the type of loan. Syndicated loans are usually larger as they involve multiple lenders supplying the funds for the loans.

Loans from our sample period are much more likely to contain balance sheet covenants (net worth covenants are present in 73% of our sample) than those from later period covered by

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the Nini et al. sample (45%). This pattern is consistent with the declining use of balance sheet covenants documented in Demerjian (2011). Income escalators constitute 37% of the net worth covenants in our sample. The corresponding proportion for income escalators for loans from the period 1994–2004 as reported in Beatty et al. (2008) is two-thirds, highlighting that the use of income escalators has increased significantly in later years. The net worth covenants stipulate the exclusion of intangible assets in 70% of the sample loans. Finally, we find conservative contractual definitions involving differential treatment of specific items of gains and losses (such as extraordinary items, gains from asset sales, and gains from asset write-up) in 34% of the contracts.

Panel C of Table 1 provides descriptive statistics of the reporting conservatism variables. Panel D provides descriptive statistics of firm characteristics in the sample. All variables have values that are generally consistent with prior studies.

Results

Univariate analysis

Table 2, panel A presents univariate comparisons of contracting and reporting conservatism variables. We compare our dependent variables in the post-treatment period with those in the pre-treatment period for treatment group and control group separately. We then conduct a difference-in-difference test by comparing the difference in the treatment group with that in the control group. The difference-in-difference test shows a decrease in contracting conservatism following the ruling. All three proxies for contracting conservatism (*D(Escal)*, *D(Tang)* and *D(ConsDef)*) exhibit significant declines relative to the control group following the ruling, with negative difference-in-difference values. This suggests that contracting parties rely less on contracting conservatism following a shift in fiduciary duties to creditors.

On the other hand, we observe an increase in reporting conservatism following the ruling. *Crank* increases following the ruling, whereas discretionary accruals and special items decrease following the ruling (although the latter is not statistically significant). These results support Aier et al.'s (2014) conclusion that, with additional legal exposure created by the ruling, managers are more willing to supply conservative reporting to meet lenders' demand for conservatism (i.e., the "enforcement" problem diminishes).

Taken together, our findings on contracting and reporting conservatism suggest a substitution of contracting conservatism by relatively less costly reporting conservatism as creditors gain more power. However, these univariate findings need to be explored in a multivariate setting before we can draw robust inferences.

Panel B of Table 2 presents Pearson correlations amongst the contracting and accounting conservatism variables for the pooled sample of treated and untreated firms for all time periods. We note that the contracting conservatism measures have nearly zero correlation with one another.

Regression results for contracting conservatism

Table 3, panel A reports the results of our main analyses of contracting conservatism. We run logit regressions of the use of conservative loan modifications on an indicator variable for affected firms (DE-incorporated and in the zone of insolvency), an interaction term between *Afterlaw* and *Affected*, and control variables. In columns 1 to 3, the dependent variable is an indicator variable taking the value one as follows, and zero otherwise: *D(Escal)*: use of an income escalator in the contract; *D(Tang)*: use of a tangible net worth covenant; and *D(ConsDef)*: use of conservative definitions of cash flows, net income, or net worth in the loan contract. We find significantly lower use of income escalators, tangible net worth covenants, and conservative

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definitions for affected firms after the ruling. This result suggests that, when there is an exogenous increase in fiduciary duties which favors creditors, contracting parties rely less on protection in the form of covenant modifications of GAAP numbers which introduce additional conservatism.[13]

To assess the sensitivity of our results to different control groups, we carry out two sensitivity tests. First, we repeat our analysis on a subsample of firms that are close to insolvency and compare firms that are DE-incorporated with those that are not. The benefit of this analysis is that any effect of firms' distress risk can be more properly controlled. Panel B of Table 3 shows negative and significant coefficients in two out of three measures. Second, we re-run our analysis on a subsample of DE-incorporated firms and compare DE-incorporated firms that are close to insolvency with those that are not close to insolvency. This analysis is useful because it effectively controls for the potential confounding effect of states. Panel C shows that we continue to obtain negative and significant results for *D(Tang)* and negative, albeit marginally insignificant, results for the other two measures. One possible reason is that the smaller sample size reduces statistical power of the tests.

Regression results for reporting conservatism

We now turn our attention to reporting conservatism in financial statements issued by firms before and after the ruling. In Table 4, the dependent variable is *Crank* (as per Aier et al. 2014), *DA-DD* (signed discretionary accruals from the Dechow-Dichev model) or *SPI/AT* (signed special items deflated by assets).

Consistent with Aier et al.'s findings, we document in column (1) of panel A that *Crank* increases significantly after the ruling, indicating an overall increase in reporting conservatism. In

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[13] We obtain similar results if we use OLS regressions.

column (2), the results suggest that there are significant reductions in discretionary accruals among affected firms, relative to control groups. Column (3) shows that the magnitude of special items is more negative for the affected firms.

In panel B we analyze only close-to-default firms and our results continue to hold for *Crank* and *DA-DD*; the sign is negative for *SPI/AT* but the coefficient is not statistically different from zero. In panel C, we focus only on Delaware firms and find that all of our results hold when interacting the time and low distance-to-default indicator variable (*Lowdist*).

Taken together, our results suggest that, with improved alignment between the board and creditors resulting from the *Credit Lyonnais* case, Delaware firms nearing financial distress (but not in bankruptcy) utilize less debt contracting conservatism (Table 3) and increase their financial reporting conservatism (Table 4). In the analyses that follow, we examine whether there is a direct substitution effect between the two approaches to implement conservatism. We also conduct cross-sectional analyses to examine whether any substitution effect varies with *ex-ante* firm characteristics of the borrowers that reflect the relative strengths and weaknesses of the two conservatism mechanisms.

4. Additional analyses

Direct test of substitution

So far, we have examined the effect of the exogenous shock of the *Credit Lyonnais* ruling on each type of conservatism separately. To further understand the relation between the two types of conservatism, we now examine the interaction between these mechanisms. Since public filings are made every fiscal quarter whereas loan contracts (including new and amended ones) are written only when needed, we investigate whether contracting parties consider reporting conservatism

when deciding how contracting conservatism should change in response to the ruling. We expect the substitution to be more pronounced for firms that experience a greater increase in reporting conservatism after the ruling.

We create an indicator variable, *D(Inc AccgCon)*, equal to one if a firm's average reporting conservatism measure is below median in the pre-ruling period and above median in the post-ruling period, and zero otherwise. We then interact *D(Inc AccgCon)* with the difference-in-difference variable in model (1) and create a three-way interaction term *Afterlaw × Affected × D(Inc AccgCon)*. We augment the contracting conservatism tests from Table 3 by adding the *D(Inc AccgCon)* indicator variable, the three-way interaction term and the associated double interaction terms. Dependent variables are the three proxies for contracting conservatism. We expect the three-way interaction term to be significantly negative.

Table 5 reports the results. We focus our discussion on panel A where *Crank* is the measure of reporting conservatism. Consistent with our expectation, the three-way interaction terms are negative across all proxies and are statistically significant for income escalator and tangible net worth. The results indicate that more conservative reporting choices are associated with greater reduction in contracting conservatism after the ruling. The tenor of the results does not change in panels B and C. The findings in Table 5 support the conjecture that contracting parties consider reporting conservatism when deciding how contracting conservatism should change in response to the ruling.

Cross-sectional analyses

In Hypothesis 2A and Hypothesis 2B, we predict that *ex-ante* business uncertainty and board independence would affect the relation between reporting conservatism and contracting

conservatism cross-sectionally following the ruling. We use *equity volatility* (log of annualized monthly standard deviation of stock returns, taken from CRSP) as a proxy for *ex-ante* business uncertainty. To facilitate interpretation, we divide the sample based on pre-event average equity volatility, split at the median. We measure *Independent Board* as an indicator variable taking value of one if the average proportion of independent board members during the pre-event period is above 50%, and zero otherwise. The cross-sectional tests follow the same approach as Table 5. We examine the triple interaction terms in the contracting conservatism regressions for two subsamples split by either *ex-ante* business uncertainty or board independence. Table 6 reports the results of the cross-sectional analyses. Panel A shows the effect of *ex-ante* uncertainty. We find that the substitution effect is concentrated in the subsample of firms with high *ex-ante* business uncertainty. This finding is consistent with hypothesis Hypothesis 2A and suggests that the higher *ex-ante* costs of writing contracts in high uncertainty firms move the contracting parties further away from using contracting conservatism following the ruling, which is possible because reporting conservatism is now less costly to enforce. Panel B shows the results for board independence. The substitution effect is concentrated in firms with more independent boards.

To summarize, the results in Tables 5 and 6 provide support for our main interpretation that, following the *Credit Lyonnais* ruling, whereby the court directed a stronger alignment between directors and creditors for Delaware firms near financial distress, there is a substitution of contracting conservatism by financial reporting conservatism in affected firms.

5. Robustness analyses

Causality

Inferences from the difference-in-difference methodology rely on the assumption that, absent the treatment effect, both treated and control firms would have changed at the same rate

over time. To assess whether this is plausible, we test for a parallel trend *prior to* the treatment. We replace the *Afterlaw* indicator variable with four separate year-variables: *Afterlaw*(t = −1), *Afterlaw*(t = 0), *Afterlaw*(t = 1) and *Afterlaw*(t >= 2).[14] As shown in Table 7, none of the pre-event variables are significant. This suggests that the parallel trend assumption is not violated. Furthermore, most of the differences in both contracting and reporting conservatism start to be significantly different from 1992 or, for some proxies, from 1993 onwards.

The results also rule out reverse causality, in which the court ruling is an outcome of lobbying at the state of incorporation. If, for example, a broad coalition of DE-incorporated firms, all experiencing increases in reporting conservatism and decreases in contracting conservatism, successfully influenced the court decisions, then causality might be reversed. If this were the case, we should already see an effect of the court ruling prior to its passage (Bertrand and Mullainathan 2003; Armstrong, Balakrishnan and Cohen 2012). The absence of any significant pre-trend is evidence against reverse causality.

The remaining robustness tests described below are untabulated for the sake of brevity. All of the results discussed are available from the authors upon request.

Potential two-step bias when using residuals from a first-step regression as dependent variable in a second-step regression

In column (2) of all panels in Table 4, and also in column (2) of panel B in Table 7, because the dependent variable *DA-DD* is the residual from a separate first-step regression, we acknowledge the possibility of a two-step bias as explained in Chen et al. (2018). Due to the small

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[14] *Afterlaw*(t=-1) is a dummy equal to one if the court ruling will pass one year from now. *Afterlaw*(t=0) is a dummy equal to one if the court ruling passed this year, and *Afterlaw*(t=1) and *Afterlaw*(t>=2) are dummies equal to one if the court ruling passed one year ago and two or more years ago, respectively.

size of the samples involved in those analyses, we are unable to fully implement the Chen et al. (2018) adjustment because it requires the inclusion of first-step regressors as well as their interactions with the other variables in the model (see Chen et al. 2018, p. 774). Given our controls for industry and year fixed effects and samples with at most 221 observations, we do not have enough degrees of freedom to conduct the statistical tests. However, in order to alleviate concerns about the possible two-step bias, we also run the aforementioned regressions using the full Compustat sample (since those analyses do not require loan-related variables) and the full sample results, involving approximately 28,000 observations, are robust to the inclusion of first-step regressors.

Changes in firms' behavior post Credit Lyonnais

Becker and Strömberg (2012) document changes in firms' real decisions as a result of the *Credit Lyonnais* ruling, in particular, increase in investment and decrease in cash flow volatility for affected firms. In the main tests of reporting conservatism (Table 4), we control for capex (scaled by assets) and equity volatility to help alleviate concerns that changes in reporting conservatism may reflect these real decisions. To help address concerns that the effects on contracting conservatism may be driven by these real decisions, we also re-run the tests in Table 3 with the inclusion of these two additional variables and our conclusions remain unchanged.

Business cycle variation across geographic regions and industries

The *Credit Lyonnais* court ruling passed in 1991, just after an economic recession in early 1990. It is unlikely that our results are driven by an economic recession affecting DE firms disproportionately, given that the court ruling affected firms at the state of incorporation level

rather than the states in which they physically operate.[15] However, to mitigate this concern, we use: (1) state of headquarters location × year joint fixed effects to capture the impact of variations in state-level business conditions, and (2) industry × year joint fixed effects to capture the impact of variations across industry groups on contracting and reporting conservatism. Our results continue to hold when these additional variables are included and thus are unlikely to be driven by variations in business cycle across industries or geographic regions.

Alternative measures of insolvency

Although we believe that the Merton-based distance-to-default measure represents a reasonable measure of insolvency risks because it incorporates investors' expectations of future default risk (Vassalou and Xing 2004), we repeat our analyses using Altman's Z-Score (Altman 1968) to capture near insolvency. Firms with a Z-Score below the median 1990 value are classified as being close to default. Using this alternative measure of insolvency risk, we obtain similar results for both contracting and reporting conservatism.

Alternative clustering of standard errors

Following Becker and Strömberg (2012), we cluster the standard errors at the intersection of state of incorporation and year level. To assess the sensitivity of our results, we repeat our main tests using two-way clustering of standard errors at firm and year level. We continue to find similar results for both contracting and reporting conservatism.

Firm fixed effects

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[15] We are not aware of any research suggesting that DE firms as a group share any physical, geographic concentration within the U.S. that could have made them more or less susceptible to the recession than non-DE firms.

In our tests of Hypothesis 1, we have used logit specifications with industry fixed effects. We repeat the tests using firm fixed effects and OLS specifications for Hypothesis 1 and using firm fixed effects for Hypothesis 2A and Hypothesis 2B (in these specifications, the *Affected* main effects are dropped). The paper's conclusions continue to hold if we use the alternative specifications with firm fixed effects.

Regression by quartile of distance-to-default

In our main specification, we simply partition the sample based on whether distance-to-default is below or above the median. This binary cutoff is somewhat arbitrary and the likelihood that a firm considers itself close to distress may change in a more gradual fashion. We repeat the analysis by sorting firms into solvency quartiles. We run regressions which allow us to examine the gradual change in accounting or loan contracting choices as firms move towards insolvency, and we find that much of the effects documented in this paper are concentrated in the quartile of firms closest to financial distress, as we would expect.

6. Conclusion

We exploit an exogenous shift in the balance of power between creditors and owners to examine how contracting and reporting conservatism are affected by managerial fiduciary duties. A Delaware court ruling in the 1991 *Credit Lyonnais* case provides a natural experiment to examine this question. The ruling increased officers and directors' fiduciary duties to creditors of firms that are solvent but are in the vicinity of insolvency. Prior to the ruling, it was held that management owes fiduciary duties to creditors only after the firm becomes insolvent. Hence, the

ruling shifted the balance of power in favor of creditors thereby mitigating the creditor-owner conflict.

Adopting a difference-in-difference empirical design, we find that, following the ruling, debt contracts of affected firms rely less on the use of income escalators, tangible net worth covenants, and conservative definitions of net income, cash flows, or net worth (Table 3). We also find that affected firms have higher composite conservatism score, lower discretionary accruals, and report more negative special items following the ruling (Table 4). The results suggest a substitution of contracting conservatism by reporting conservatism following the ruling. When we further examine the interdependence between the two conservatism mechanisms, we find (Table 5) that the increased use of reporting conservatism is directly correlated with a lower usage of contracting conservatism. Moreover, the substitution effect is more pronounced in firms with greater business uncertainty and more independent boards of directors (Table 6).

We conduct a battery of sensitivity analyses for alternative explanations and our results are robust to these tests. Overall, our study demonstrates how a shift in the relative strengths of the duties owed by managers to creditors versus shareholders changes the nature of both financial reporting and debt contracting.

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Appendix 1. Variable definitions

Variables	Definition
D(Escal)	Indicator variable equal to one if net worth covenant includes income escalator, zero otherwise.
D(Tang)	Indicator variable equal to one if net worth covenant excludes intangible assets, zero otherwise.
D(ConsDef)	Indicator variable equal to one if definition of income, cash flow or net worth in debt contract includes losses but not gains for specific line items (e.g., extraordinary items), zero otherwise.
Crank	Composite measure of reporting conservatism from Aier et al. (2014). Higher values indicate more conservative financial reporting.
DA-DD	Discretionary accruals estimated using the Dechow and Dichev (2002) model for each year and each Fama-French 48 industry with at least 20 observations. Total accruals are regressed on current, lead and lagged operating cash flows, change in revenue and PPE. Discretionary accruals are residuals from the regressions.
SPI/AT	Special charges (Compustat data item 17) scaled by total assets (item 6).
Afterlaw	Indicator variable equal to one if year>1991, zero otherwise.
Affected	Indicator variable equal to one if firm is incorporated in Delaware and close to insolvency (distance-to-default is below median in 1990), zero otherwise.
Cov Intensity	Number of financial covenants.
Maturity	Maturity of loans in years.
Loan/Assets	Loan amount over total assets (item 6).
Ppricing	Indicator variable equal to one if performance pricing is included in loan contract, zero otherwise.
Revolver	Indicator variable equal to one if loan agreement contains a line of credit/revolver, zero otherwise.
Spread	The interest rate spread over LIBOR for loans without performance pricing.
Rating	For firms with available credit rating, the debt rating (coded numerically from 1 (AAA) to 24 (C)) is regressed on total assets, ROA, debt to assets, and indicator variables for presence of subordinated debt, divided, and loss The coefficient estimates are then used to generate credit ratings for all firm-years.
ROA	Log of one plus net income (item 18) over lagged total assets (item 6).
Stock Return	Log of one plus stock return over past 12 months.
Sales Growth	Change in sales over lagged sales (item 12).
Firm Size	Log of total assets (item 6).
Curr Accrual	Absolute value of current accruals, calculated as changes in account receivables (item 302), inventory (item 303), accounts payables and accrued liabilities (item 304), and accrued income taxes (item 305), all over total assets (item 6).
Firm Leverage	Short term debt (item 34) + long term debt (item 9) - cash (item 1), all over total assets (item 6).
Capx/Asset	Capital expenditure (item 128) over total assets (item 6).
D(Issue Equity)	Indicator variable equal to one for positive stock issuance (calculated as change in common equity plus change in deferred tax minus change in retained earnings, all scaled by total assets), zero otherwise (Baker, Stein and Wurgler 2003).
Equity Volatility	Log of annualized monthly standard deviation of stock returns, from CRSP.
IO	Percentage of outstanding equity owned by institutions.
Delaware	Indicator variable equal to one if firm is incorporated in Delaware, zero otherwise.
Lowdist	Indicator variable equal to one if firm is close to insolvency (distance-to-default below median in 1990), zero otherwise.
D(Inc AccgCon)	Indicator variable equal to one if firm's average reporting conservatism measure is below median before passage of court ruling and above median in post-ruling period, zero otherwise.
Afterlaw(t=−1)	Indicator variable equal to one if one year before passage of court ruling, zero otherwise.
Afterlaw(t=0)	Indicator variable equal to one for the year during which court ruling passes, zero otherwise.
Afterlaw(t=1)	Indicator variable equal to one if one year after passage of ruling, zero otherwise.
Afterlaw(t>=2)	Indicator variable equal to one if two or more years after passage of court ruling, zero otherwise.

Appendix 2. Procedures for collection of loan sample data

Since there are no readily available machine-readable data on loan contracts for the time period in our study, we conduct our loan choice analysis on a small, hand-collected sample.

We retrieve loan contracts filed by companies with the SEC. Under SEC Regulation S-K, item 601 (b), public firms are required to include all material contracts as exhibits in their filings. Most loan contracts can be found in Forms 10-K, 10-Q, 8-K and registration statements, and they are typically attached as exhibit item 4 and/or item 10. Because the time period we study is before the mandatory implementation of electronic filings on the SEC's *EDGAR* system, we manually search microfiche records of firms' filings from Q-File to locate loan contracts. After identifying the location of loan contracts from the microfiche records, the next step is to purchase the exhibits from commercial data providers because most microfiche records only contain the main filings and do not include exhibits. Due to the high cost of data collection, we adopt a random sampling strategy to construct the credit agreement sample. The procedure is as follows:

(1) We focus on firms in our base dataset which have debt to total assets ratio $((dlc + dltt) / at)$ of 10% or more. According to Item 601(b) (4) of Regulation S-K, firms are required to file long-term debt instruments only when the debt amount exceeds 10% of total assets. This step ensures that the firms we select have a good chance of having "material" debt contracts that need to be disclosed as exhibits.

(2) For each year, we categorize firms into two groups, Delaware and non-Delaware, and randomly select 150 firms from each year and each group.[16] We locate and read the microfiche records for these firms from Q-File. Q-File provides index books containing firm names and filing

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[16] The distribution of public firms on Compustat between DE and non-DE incorporation is approximately half-half, which is similar to the proportions reported in Daines (2001).

types. Firms are alphabetically ordered by name. We manually match our random sample with the index book by company name and verify our matches using the EIN numbers from both sources.[17] To make our hand collection work manageable, we only run this random selection procedure four times from years 1991 to 1994, and we read the exhibit list of the selected companies' 10-K reports.[18] In each 10-K exhibit list, we search for credit agreements that were initiated three years before (1989 – 1991) and three years after (1992 – 1994) the *Credit Lyonnais* ruling. Following Beatty, Cheng and Zach (2019), our search looks for key words related to "credit", "loan" or "financing" in the exhibit list.[19] If a loan was initiated within the year of the 10-K, the loan contract can usually be immediately located in the 10-K using the exhibit number. If a loan was initiated prior to the year of the 10-K, the 10-K exhibit list references the type, date and the exhibit number of the original filing where the actual copy of the loan was filed. The original filing could be of any form, such as 10-K, 10-Q, 8-K and registration statements.

(3) Since Q-File keeps only the main body of filings but not exhibits, we record the location (filing type, date and exhibit number) of the actual copies of the loan contracts and purchase those copies from commercial data providers.

(4) Finally, we read through each loan contract and manually code the variables of interest.

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[17] When matching by name, we consider both current and historical names of a company.

[18] This simplified approach focusing on 10-Ks is likely to have an innocuous impact on our sample selection for the following reasons: Compared to other filing types, a 10-K report has the most comprehensive exhibit list. All material loan contracts of a company as of the fiscal year-end will be disclosed in the 10-K exhibit list. The 10-K report constitutes a reasonable starting point for our search since it includes loans initiated within the year of the 10-K as well as references to loans initiated in the prior periods back to their original filings. Thus, from a company's 10-K exhibit list, we are able to track down the filing where loans were originally filed.

[19] For example, a credit agreement can be called "credit agreement", "loan agreement", "credit facility", "loan and security agreement", "loan & security agreement", "revolving credit", "financing and security agreement", "financing & security agreement", "credit and guarantee agreement", "credit & guarantee agreement", "credit and security agreement" or "credit & security agreement".

Through the above search procedure steps (1) to (3), we identify 333 exhibit items that are likely to be credit agreements. We are able to find 310 copies of exhibits from commercial data providers (23 cases cannot be found for various reasons, including cases where the filings refer to an exhibit that seemingly was never filed with the SEC). For the 310 exhibit copies, we further eliminate 28 copies (3 cases are promissory notes or loan commitment letters without covenant details; 2 cases are guaranty or security agreements only; 3 cases are minor amendments or incomplete contracts; 16 cases are bond contracts; and 4 cases are unreadable due to low quality of the microfiche copies). After further requiring availability of control variable values when conducting regressions, our final credit agreement sample consists of 278 loan contracts that were initiated in a six-year window around the *Credit Lyonnais* ruling.[20]

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[20] Only a few studies have examined private loan agreements before 1996 (when electronic filings became mandatory). Leftwich (1983) reviews 10 loan agreements from before 1977, Beneish and Press (1993) investigate 96 loan contracts or amendments from 1983 to 1987, and Beatty, Ramesh and Weber (2002) study 285 credit agreements that are searchable through Lexis-Nexis during 1994 to 1996 (when electronic filings were sparsely available).

TABLE 1
Descriptive statistics

Panel A: Distribution of the loan contracts sample

All credit agreements:

	1989	1990	1991	1992	1993	1994	Total
Delaware	15	28	32	25	35	23	158
Non-Delaware	21	18	24	20	22	15	120
Total	36	46	56	45	57	38	278

Credit agreements with net worth covenant:

	1989	1990	1991	1992	1993	1994	Total
Delaware	10	22	25	19	27	15	118
Non-Delaware	16	14	17	13	17	9	86
Total	26	36	42	32	44	24	204

Panel B: Loan characteristics

	Our sample				Nini et al. (2009) sample		
	Mean	Median	N		Mean	Median	N
Loan amount (in $ millions)	96.175	25.000	278		450.0	200.0	3,720
Loan size (amount/total assets)	0.342	0.171	278		0.338	0.245	3,720
Spread (%)	1.061	0.750	278		1.700	1.500	3,720
Secured	0.633	1.000	278		0.647	1.000	3,117
Has a line of credit/revolver	0.770	1.000	278		0.938	1.000	3,720
Maturity	3.290	3.000	278				
Has performance pricing	0.227	0.000	278				
Financial Covenants							
Number of financial covenants	3.320	3.000	278		2.336	2.000	3,603
Any net worth covenants	0.734	1.000	278		0.452	0.000	3,603
Contracting Conservatism Variables							
Income escalator	0.373	0.000	204				
Intangible asset exclusion	0.696	1.000	204				
Conservatively defined income, cash flow, or net worth	0.342	0.000	278				

Panel C: Reporting conservatism variables

	N	Mean	Median	SD	P25	P75
Crank	222	5.519	5.667	1.166	5.000	6.333
DA-DD	221	0.006	0.009	0.076	-0.043	0.045
SPI/AT	242	−0.008	0.000	0.030	−0.002	0.000

Panel D: Firm characteristics

	N	Mean	Median	SD	P25	P75
ROA	278	0.019	0.031	0.080	0.000	0.062
Stock Return	278	0.064	0.079	0.541	−0.267	0.384
Sales Growth	278	0.162	0.125	0.263	0.008	0.246
Firm Size	278	5.072	5.034	1.593	3.987	5.967
Curr Accrual	278	0.049	0.031	0.055	0.011	0.064

The sample period is from 1989 to 1994. We exclude financial and utility industries (sic 6000–6999 and sic 4000–4999). All variables are defined in Appendix 1.

TABLE 2
Univariate analysis

Panel A: Comparison of treatment and control groups

	N	Treatment group Before	After	p-value	N	Control group Before	After	p-value	Diff-in-diff Diff-in-diff	p-value
D(Escal)	49	0.219	0.118	0.395	155	0.340	0.600	0.002	−0.361	0.024
D(Tang)	49	0.750	0.235	0.000	155	0.780	0.655	0.091	−0.389	0.011
D(ConsDef)	59	0.568	0.227	0.010	219	0.305	0.333	0.667	−0.369	0.010
Crank	48	5.244	6.462	0.001	174	5.403	5.561	0.381	1.060	0.006
DA-DD	41	0.011	−0.057	0.018	180	0.006	0.009	0.781	−0.072	0.033
SPI/AT	53	−0.006	−0.013	0.496	189	−0.007	−0.008	0.661	−0.005	0.604

Panel B: Correlation table

		[1]	[2]	[3]	[4]	[5]	[6]
[1]	*D(Escal)*	1.00					
[2]	*D(Tang)*	0.02	1.00				
[3]	*D(ConsDef)*	0.05	−0.03	1.00			
[4]	*Crank*	−0.14*	−0.11	0.15**	1.00		
[5]	*DA-DD*	−0.03	0.20**	−0.05	−0.13*	1.00	
[6]	*SPI/AT*	0.17**	0.07	−0.10	−0.03	0.35***	1.00

The sample period is from 1989 to 1994. We exclude financial and utility industries (sic 6000–6999 and sic 4000–4999). The treatment group consists of firms that are incorporated in Delaware and are close to insolvency. *, ** and *** denote significance level at 10%, 5% and 1% respectively. All variables are defined in Appendix 1.

TABLE 3

Contracting conservatism

	Panel A: Full sample				Panel B: Only close-to-default firms				Panel C: Only Delaware firms		
	(1) D(Escal)	(2) D(Tang)	(3) D(ConsDef)		(1) D(Escal)	(2) D(Tang)	(3) D(ConsDef)		(1) D(Escal)	(2) D(Tang)	(3) D(ConsDef)
Afterlaw*Affected	**−1.428**	**−3.721**	**−2.086**	**Afterlaw*Delaware**	**−3.179**	**1.520**	**−5.019**	**Afterlaw*Lowdist**	**−1.649**	**−4.476**	**−1.487**
	(−2.55)**	**(−4.05)*****	**(−1.73)***		**(−1.96)****	**(0.53)**	**(−2.88)*****		**(−1.39)**	**(−3.56)*****	**(−1.61)**
Affected	−1.051	−0.028	0.807	Delaware	−2.017	−3.541	1.773	Lowdist	−1.349	−0.624	1.122
	(−3.60)***	(−0.05)	(3.00)***		(−1.84)*	(−1.50)	(1.74)*		(−1.74)*	(−0.55)	(1.73)*
Cov Intensity	0.449	0.328	0.482	Cov Intensity	0.641	1.404	0.784	Cov Intensity	0.538	0.849	0.548
	(3.26)***	(2.32)**	(4.83)***		(1.54)	(1.85)*	(3.29)***		(2.61)***	(2.15)**	(2.96)***
Maturity	0.156	−0.217	−0.005	Maturity	0.490	2.987	0.092	Maturity	0.085	−0.785	0.035
	(1.83)*	(−1.71)*	(−0.06)		(1.85)*	(1.20)	(0.54)		(0.45)	(−1.89)*	(0.26)
Loan/Assets	0.018	−0.714	0.364	Loan/Assets	3.074	−5.614	−0.839	Loan/Assets	0.563	−0.934	0.674
	(0.09)	(−4.73)***	(1.72)*		(2.48)**	(−1.15)	(−0.45)		(1.79)*	(−2.41)**	(2.73)***
Ppricing	0.178	0.176	0.467	Ppricing	−0.160	3.148	0.497	Ppricing	0.970	0.628	1.273
	(0.32)	(0.25)	(1.15)		(−0.14)	(0.74)	(0.52)		(1.09)	(0.68)	(1.87)*
Revolver	−0.793	−0.054	−0.306	Revolver	−2.250	10.896	0.369	Revolver	−0.731	−0.080	−1.424
	(−1.44)	(−0.10)	(−0.85)		(−1.90)*	(1.12)	(0.46)		(−0.84)	(−0.07)	(−1.90)*
Spread	0.072	0.153	0.109	Spread	0.101	−1.883	−0.842	Spread	0.579	0.135	−0.166
	(0.45)	(1.05)	(0.64)		(0.33)	(−1.23)	(−2.36)**		(1.33)	(0.69)	(−0.65)
Rating	0.145	−0.127	−0.040	Rating	0.625	−1.296	−0.027	Rating	0.335	0.080	0.004
	(1.51)	(−1.58)	(−0.47)		(1.60)	(−1.34)	(−0.16)		(2.23)**	(0.57)	(0.03)
ROA	4.524	2.453	4.750	ROA	−17.860	52.140	−1.254	ROA	28.938	7.448	2.665
	(1.08)	(0.89)	(1.46)		(−1.91)*	(3.38)***	(−0.40)		(3.36)***	(1.03)	(0.58)
Stock Return	0.894	0.545	0.122	Stock Return	4.046	−5.226	0.965	Stock Return	0.701	0.302	1.483
	(1.40)	(0.85)	(0.26)		(2.90)***	(−2.79)***	(1.17)		(0.89)	(0.31)	(2.14)**
Sales Growth	0.498	−1.555	−1.108	Sales Growth	0.828	0.352	−0.950	Sales Growth	−0.640	−1.471	−3.481
	(0.57)	(−1.86)*	(−1.46)		(0.42)	(0.05)	(−0.84)		(−0.51)	(−0.88)	(−2.77)***
Firm Size	0.319	−0.436	0.342	Firm Size	0.236	−7.935	0.228	Firm Size	0.826	−0.392	0.089
	(1.57)	(−2.64)***	(3.10)***		(0.56)	(−1.49)	(0.79)		(2.72)***	(−1.04)	(0.47)
Curr Accrual	−5.014	11.528	2.330	Curr Accrual	−9.759	44.367	2.552	Curr Accrual	−1.039	−6.092	2.270
	(−1.04)	(1.86)*	(0.70)		(−1.44)	(1.92)*	(0.35)		(−0.14)	(−0.35)	(0.39)
Observations	204	204	278	Observations	96	96	119	Observations	118	118	158
R-squared	0.257	0.323	0.234	R-squared	0.481	0.772	0.384	R-squared	0.435	0.548	0.358
Industry FE	Yes	Yes	Yes	Industry FE	Yes	Yes	Yes	Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Year FE	Yes	Yes	Yes	Year FE	Yes	Yes	Yes

The sample consists of 278 randomly selected loan contracts from 1989 to 1994. All variables are defined in Appendix 1. Intercept terms are included in the logit regressions but are not reported. *z*-statistics are presented beneath the coefficients within parentheses. *, ** and *** denote significance level at 10%, 5% and 1% respectively. Standard errors are corrected for heteroscedasticity and are clustered by the interaction of the state of incorporation and year level, except for panel C which is for only one state, and therefore robust standard errors are presented instead. In panel B, close-to-default firms are those with below median distance-to-default, measured in 1990. Bold text indicates variable of interest.

TABLE 4
Reporting conservatism

Panel A: Full sample

	(1) Crank	(2) DA-DD	(3) SPI/AT
Afterlaw*Affected	**0.884**	**−0.039**	**−0.015**
	(1.96)*	**(−2.09)****	**(−2.03)****
Affected	−0.005	0.015	0.007
	(−0.02)	(2.10)**	(1.52)
ROA	2.341	0.407	0.209
	(1.74)*	(5.76)***	(2.48)**
Stock Return	0.143	−0.000	0.002
	(0.91)	(−0.03)	(0.44)
Firm Size	0.065	−0.012	−0.001
	(1.06)	(−3.18)***	(−1.22)
Firm Leverage	−1.412	0.060	0.013
	(−3.85)***	(2.11)**	(1.44)
Capx/Asset	−0.136	−0.093	0.004
	(−0.15)	(−1.79)*	(0.37)
D(Issue Equity)	−0.140	−0.010	0.003
	(−0.82)	(−0.58)	(0.78)
Equity Volatility	0.049	−0.007	−0.002
	(0.25)	(−0.64)	(−0.35)
IO	0.339	0.046	−0.029
	(0.75)	(1.41)	(−2.84)***
Observations	222	221	242
R-squared	0.316	0.333	0.403
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Panel B: Only close-to-default firms

	(1) Crank	(2) DA-DD	(3) SPI/AT
Afterlaw*Delaware	**1.464**	**−0.080**	**−0.010**
	(2.57)**	**(−1.95)***	**(−0.78)**
Delaware	0.212	0.003	0.001
	(0.51)	(0.19)	(0.11)
ROA	−5.197	0.338	0.335
	(−2.60)**	(3.13)***	(3.16)***
Stock Return	0.921	0.008	−0.004
	(3.13)***	(0.59)	(−0.55)
Firm Size	−0.307	−0.012	−0.001
	(−2.73)***	(−1.53)	(−0.31)
Firm Leverage	0.699	0.001	0.047
	(0.62)	(0.02)	(2.86)***
Capx/Asset	3.770	0.137	−0.045
	(1.05)	(1.28)	(−0.66)
D(Issue Equity)	−0.409	0.002	0.004
	(−1.22)	(0.12)	(0.60)
Equity Volatility	−0.057	−0.014	−0.001
	(−0.13)	(−0.46)	(−0.20)
IO	2.038	0.063	−0.017
	(2.39)**	(1.45)	(−1.21)
Observations	100	94	105
R-squared	0.521	0.559	0.637
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Panel C: Only Delaware firms

	(1) Crank	(2) DA-DD	(3) SPI/AT
Afterlaw*Lowdist	**1.034**	**−0.054**	**−0.014**
	(2.40)**	**(−2.04)****	**(−2.50)****
Lowdist	−0.033	0.007	0.007
	(−0.09)	(0.32)	(1.65)
ROA	−0.016	0.340	0.391
	(−0.01)	(2.51)**	(6.08)***
Stock Return	0.412	0.004	−0.005
	(1.84)*	(0.21)	(−1.52)
Firm Size	−0.040	−0.014	0.000
	(−0.47)	(−2.80)***	(0.25)
Firm Leverage	−1.348	0.059	0.003
	(−2.41)**	(1.25)	(0.37)
Capx/Asset	0.201	−0.124	0.026
	(0.23)	(−1.32)	(1.65)
D(Issue Equity)	−0.370	−0.019	−0.005
	(−1.60)	(−1.05)	(−1.29)
Equity Volatility	−0.190	−0.015	0.007
	(−0.68)	(−0.92)	(1.87)*
IO	0.840	0.023	−0.025
	(1.40)	(0.54)	(−2.76)***
Observations	123	116	138
R-squared	0.356	0.388	0.636
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

The sample period is from 1989 to 1994. All variables are defined in Appendix 1. Intercept terms are included in the regressions but are not reported. *t*-statistics are presented beneath the coefficients within parentheses. *, ** and *** denote significance level at 10%, 5% and 1% respectively. Standard errors are corrected for heteroscedasticity and are clustered by the interaction of state of incorporation and year, except for panel C which is for only one state, and therefore robust standard errors are presented instead. In panel B, close-to-default firms are those with below median distance-to-default, measured in 1990. Bold text indicates variable of interest.

TABLE 5

Interaction between contracting conservatism and reporting conservatism

	Panel A: *AccgCon=Crank*			**Panel B:** *AccgCon=DA-DD*			**Panel C:** *AccgCon=SPI/AT*		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	D(Escal)	*D(Tang)*	*D(ConsDef)*	*D(Escal)*	*D(Tang)*	*D(ConsDef)*	*D(Escal)*	*D(Tang)*	*D(ConsDef)*
*Afterlaw*Affected*	0.074	0.147	−0.183	−0.004	0.127	−0.136	0.003	−0.651	−0.283
	(0.39)	(0.81)	(−1.04)	(−0.02)	(0.91)	(−0.92)	(0.02)	(−5.69)***	(−1.67)*
Afterlaw*Affected*D(Inc AccgCon)	**−0.661**	**−0.899**	**−0.212**	**0.096**	**−0.520**	**−0.476**	**−0.672**	**0.253**	**−0.561**
	(−1.85)*	**(−3.62)***	**(−0.91)**	**(0.27)**	**(−2.10)**	**(−1.90)***	**(−2.61)**	**(0.82)**	**(−2.17)**
Affected	−0.126	0.066	0.087	−0.140	0.027	0.144	−0.151	0.084	0.152
	(−1.73)*	(0.68)	(1.51)	(−2.14)**	(0.38)	(2.78)***	(−2.51)**	(0.93)	(1.98)*
D(Inc AccgCon)	−0.098	0.143	−0.167	−0.437	0.329	0.760	0.637	0.139	−0.014
	(−0.66)	(1.65)	(−2.00)**	(−3.74)***	(3.35)***	(10.82)***	(4.78)***	(1.31)	(−0.11)
*Affected*D(Inc AccgCon)*	−0.030	−0.303	0.267	0.188	−0.713	−0.423	−0.002	−0.448	−0.018
	(−0.15)	(−1.81)*	(1.96)*	(0.87)	(−3.06)***	(−1.84)*	(−0.01)	(−1.85)*	(−0.08)
*Afterlaw*D(Inc AccgCon)*	0.471	0.131	−0.020	−0.173	0.001	−0.015	−0.175	−0.146	0.065
	(2.08)**	(1.01)	(−0.12)	(−1.09)	(0.00)	(−0.15)	(−1.05)	(−0.93)	(0.38)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	204	204	278	204	204	278	204	204	278
R-squared	0.325	0.424	0.290	0.337	0.425	0.334	0.481	0.363	0.281
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The sample period is from 1989 to 1994. All variables are defined in Appendix 1. Intercept terms and control variables as in Table 3 are included in the regressions but are not reported. *t*-statistics are presented beneath the coefficients within parentheses. *, ** and *** denote significance level at 10%, 5% and 1% respectively. Standard errors are corrected for heteroscedasticity and are clustered by the interaction of state of incorporation and year. Bold text indicates variable of interest.

TABLE 6
Cross-sectional analyses

Panel A: Effect of *ex-ante* uncertainty

	High equity volatility			Low equity volatility		
	(1)	*(2)*	*(3)*	*(1)*	*(2)*	*(3)*
	D(Escal)	*D(Tang)*	*D(ConsDef)*	*D(Escal)*	*D(Tang)*	*D(ConsDef)*
*Afterlaw*Affected*	0.641	0.443	0.091	−0.672	−0.305	−0.359
	(2.16)**	(1.80)*	(0.25)	(−1.88)*	(−1.05)	(−1.14)
Afterlaw*Affected*D(Inc AccgCon)	**−1.090**	**−1.335**	**−0.083**	**−0.235**	**−0.290**	**−0.480**
	(−2.28)	**(−6.52)***	**(−0.20)**	**(−0.49)**	**(−0.67)**	**(−0.96)**
Affected	−0.131	−0.100	−0.027	−0.012	0.204	0.094
	(−1.11)	(−0.98)	(−0.18)	(−0.06)	(1.04)	(0.46)
D(Inc AccgCon)	−0.254	−0.096	−0.212	−0.146	0.095	−0.237
	(−1.33)	(−0.66)	(−1.82)*	(−0.85)	(0.76)	(−1.51)
*Affected*D(Inc AccgCon)*	−0.176	0.175	0.335	0.054	−0.779	0.307
	(−0.57)	(1.23)	(1.83)*	(0.16)	(−2.17)**	(0.92)
*Afterlaw*D(Inc AccgCon)*	0.642	0.317	−0.212	0.602	0.229	0.250
	(2.02)**	(1.47)	(−1.02)	(2.72)***	(1.18)	(0.85)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	96	96	134	108	108	144
R-squared	0.519	0.569	0.437	0.450	0.533	0.293
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Test of equality of coefficient on *Afterlaw*Affected*D(Inc AccgCon)*, high vs. low equity volatility:						
p-value	0.024	0.056	0.039			

TABLE 6
Cross-sectional analyses (continued)

Panel B: Effect of board independence

	Independent board			Not independent board		
	(1)	*(2)*	*(3)*	*(1)*	*(2)*	*(3)*
	D(Escal)	*D(Tang)*	*D(ConsDef)*	*D(Escal)*	*D(Tang)*	*D(ConsDef)*
Afterlaw*Affected	0.232	0.383	−0.136	−0.123	−0.821	−0.444
	(0.72)	(1.64)	(−0.50)	(−0.65)	(−0.95)	(−1.18)
Afterlaw*Affected*D(Inc AccgCon)	**−1.023**	**−0.612**	**−0.724**	**−0.077**	**−0.562**	**−0.039**
	(−2.11)**	**(−1.69)***	**(−1.71)***	**(−0.42)**	**(−0.73)**	**(−0.08)**
Affected	−0.207	−0.056	0.076	−0.312	−0.420	0.370
	(−2.46)**	(−0.46)	(0.66)	(−2.80)**	(−1.49)	(1.44)
D(Inc AccgCon)	−0.131	−0.042	−0.046	−0.322	0.002	0.006
	(−0.62)	(−0.26)	(−0.26)	(−2.96)**	(0.01)	(0.03)
Affected*D(Inc AccgCon)	−0.250	−0.561	0.055	−0.009	0.866	0.239
	(−0.99)	(−1.79)*	(0.16)	(−0.05)	(1.66)	(0.55)
Afterlaw*D(Inc AccgCon)	1.073	0.179	0.421	−0.258	0.020	0.127
	(3.55)***	(0.71)	(1.45)	(−1.59)	(0.05)	(0.36)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	110	110	145	39	39	59
R-squared	0.476	0.586	0.348	0.994	0.912	0.755
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Test of equality of coefficient on *Afterlaw*Affected*D(Inc AccgCon)*, independent vs. not independent board:						
p-value	0.038	0.920	0.193			

The sample period is from 1989 to 1994. All variables are defined in Appendix 1. In panel A, the sample is divided based on pre-event average equity volatility, split at the median. In panel B, the sample is divided based on whether the firm had a majority independent board in the pre-event period. Intercept terms and control variables as in Table 3 are included in the regressions but are not reported. *t*-statistics are presented beneath the coefficients within parentheses. *, ** and *** denote significance level at 10%, 5% and 1% respectively. Standard errors are corrected for heteroscedasticity and are clustered by the interaction of state of incorporation and year. Bold text indicates variable of interest.

TABLE 7
Parallel trend

	Panel A: Contracting conservatism			Panel B: Reporting conservatism		
	(1)	(2)	(3)	(1)	(2)	(3)
	D(Escal)	D(Tang)	D(ConsDef)	Crank	DA-DD	SPI/AT
Afterlaw(t = –1)*Affected	−0.129	0.145	0.088	0.569	0.009	−0.005
	(−1.07)	(1.06)	(1.01)	(1.39)	(0.62)	(−0.74)
Afterlaw(t = 0)*Affected	**0.010**	**−0.041**	**−0.010**	**−1.003**	**0.015**	**0.007**
	(0.06)	**(−0.30)**	**(−0.09)**	**(−3.68)*****	**(0.64)**	**(0.78)**
Afterlaw(t = 1)*Affected	**−0.240**	**−0.510**	**−0.566**	**0.142**	**−0.030**	**−0.025**
	(−1.75)*	**(−3.07)*****	**(−5.03)*****	**(0.50)**	**(−1.10)**	**(−2.81)*****
Afterlaw(t >= 2)*Affected	**−0.274**	**−0.545**	**−0.127**	**1.406**	**−0.033**	**−0.007**
	(−2.21)**	**(−3.87)*****	**(−1.00)**	**(6.27)*****	**(−1.72)***	**(−0.78)**
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	204	204	278	222	221	242
R-squared	0.299	0.374	0.274	0.352	0.334	0.411
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

The sample period is from 1989 to 1994. All variables are defined in Appendix 1. Intercept terms are included in the regressions but are not reported. Panel A includes all control variables as in Table 3 and panel B includes all control variables as in Table 4. *t*-statistics are presented beneath the coefficients within parentheses. *, ** and *** denote significance level at 10%, 5% and 1% respectively. Standard errors are corrected for heteroscedasticity and are clustered by the interaction of state of incorporation and year. Bold text indicates variables of interest.