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Real effects of auditor conservatism

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Abstract

We examine the effect of auditor conservatism on corporate innovation. We hypothesize that, because conservative auditors constrain income-increasing accounting discretion, managers may sacrifice long-term investments in innovation to boost current earnings and meet short-term performance targets. Exploiting state-level auditor legal liability shocks as a means of identification, we find evidence consistent with this hypothesis. Cross-sectional analyses reveal that the negative effect of increased auditor conservatism on corporate innovation is more pronounced when the client firms are under greater equity- and debt-market pressures, when the client firms are exposed to greater litigation risk, and when the client firms are audited by large auditors. Our study highlights how auditors, as external monitors, can affect not only the financial reporting quality of their clients but may also induce alterations in their real operations.

Keywords: Innovation; Patents; Citations; R&D; Short-termism; Auditor conservatism; Real effects; State legal liability laws; Financial reporting discretion; Going-concern opinions

1. Introduction

Innovation is a key driver of economic growth (He and Tian 2018). However, motivating innovation in public firms is difficult and may require shielding managers from capital-market pressures as well as patience with short-term failures (Manso 2011; Ferreira, Manso, and Silva 2014; Tian and Wang 2014). Recent studies explore how various governance mechanisms affect firms' incentives to innovate (Aghion, Van Reenen, and Zingales 2013; He and Tian 2013; Faleye, Hoitash, and Hoitash 2011; Atanassov 2013; He and Tian 2018). In this study, we examine whether auditors, as stakeholders in public firms' governance, affect corporate innovation.

Specifically, we study a potential link between auditor conservatism and innovation output. Studies suggest that auditors' litigation and reputation losses derive mainly from clients' income overstatement rather than understatement (DeFond, Lim, and Zang 2016; Lys and Watts 1994; St. Pierre and Anderson 1984; DeFond and Subramanyam 1998). As such, auditors may have incentives to be conservative in their assessment of clients' financial statements (Francis and Krishnan 1999; Kim, Chung, and Firth 2003; Cahan and Zhang 2006). Regulators and researchers often associate auditor conservatism with greater earnings quality (DeFond and Francis 2005, p. 7), which could spur investment in innovation by mitigating adverse selection and moral hazard frictions (Park 2018; Lobo, Xie, and Zhang 2017; Li et al. 2016).

Alternatively, conservative auditors constrain income-increasing discretion in earnings numbers, making managers more likely to fall short of short-term performance targets (e.g., Baber, Fairfield, and Haggard 1991; Burnett et al. 2012). As such, conservative auditors may induce managers to myopically try to boost current earnings, even at the expense of long-term investments in innovation. For instance, Graham, Harvey, and Rajgopal (2005) report that nearly 80% of managers surveyed would take short-term oriented actions, such as cutting investments in innovation, to avoid missing earnings targets. Several other studies document that managers often cut investments in

innovation when their ability to meet short-term performance targets by exercising accounting discretion is constrained (Wang and D'Souza 2006; Cohen, Dey, and Lys 2008; Cohen and Zarowin 2010; Zang 2012). Theoretical models of managerial myopia suggest that, in the presence of information asymmetry, outsiders rely on accounting earnings to infer firm value and managerial ability (Gigler et al. 2014; Ewert and Wagenhofer 2005). This may incentivize managers to focus on improving short-term earnings (Narayanan 1985; Stein 1988; Stein 1989; Stein 2003). As a result, we hypothesize that, faced with conservative auditors who jeopardize meeting earnings targets by limiting income-increasing accounting discretion, managers may sacrifice long-term investments in innovation to improve current earnings.

An empirically challenging issue in investigating the effect of auditor conservatism on corporate innovation is that it is difficult to measure auditor conservatism. In addition, the relation between auditor conservatism and corporate innovation is likely to be endogenous. To overcome these challenges, we exploit plausibly exogenous state-level shocks to auditor legal liability as a means of identification. Under common law, auditor liability to third parties is determined by one of three principles: privity, the restatement of tort, or foreseeability (Anantharaman, Pittman, and Wans 2016). Privity limits auditor liability to the party in contract, whereas under foreseeability auditor liability to third parties is very expansive; restatement is in between. Between 1970 and 1998, 22 states expanded and two states reduced auditor legal liability to third parties. We use these state-level changes in auditor legal liability as shocks to auditor conservatism. Our approach is motivated by a large body of theoretical and empirical literature showing that an increase in the legal liability for audit failures encourages auditors to become more conservative in their assessment of clients' financial statements (see DeFond and Zhang 2014 for a review).

We employ a difference-in-differences regression (with firm fixed effects) in which firms headquartered or incorporated in states that change auditor legal liability form the treatment group

and firms in states that do not form the control group. We first *validate* that these liability shocks indeed lead to increased auditor conservatism. We show that treatment firms are more likely to receive modified going-concern opinions, less likely to restate financial statements, and less prone to meet or beat the zero earnings thresholds than control firms following increases in auditor legal liability. Auditors of treatment firms are also more likely to commit type-I errors and more willing to constrain income-increasing (as opposed to income-decreasing) accruals, consistent with the notion that conservative auditors err on the side of caution when evaluating firms' financial statements. These validation analyses suggest that state-level variations in auditor legal-liability predictably capture salient aspects of the auditing process that studies associate with the notion of auditor conservatism (e.g., Lu and Sapra 2009; Francis and Krishnan 1999; Lennox and Kausar 2017).

Having validated our measure of state-level changes in auditor conservatism, we turn our attention to the main research question, that is, how auditor conservatism affects corporate innovation. As a first step, we find that treatment firms that experience an increase in auditor conservatism reduce investments in R&D by nearly 6%, relative to control firms. This result provides preliminary evidence that increased auditor conservatism may reduce corporate innovation because R&D investments are innovation inputs.

Following prior studies, we use patent-based metrics as our primary measures of innovation (e.g., Tian and Wang 2014; He and Tian 2013; Atanassov 2013). Specifically, we construct two measures of corporate innovation: the number of patent applications a firm files for in a given year that are eventually granted and the number of (nonself) citations that a firm's patents receive in subsequent years. We find that an increase in auditor conservatism results in a significant decline in patents and citations. The effects are statistically significant and economically meaningful. Treatment firms receive approximately 7% fewer patents and 10% fewer citations, relative to control firms.

We predict that the negative effect of auditor conservatism on client innovation is likely to be

more pronounced when clients face greater equity and debt market pressure. We measure equity-market pressure by analyst coverage and short-term institutional ownership. Managers who face greater analyst and short-term investor pressures are likely to be more concerned about current-period earnings (Bhojraj et al. 2009; He and Tian 2013; Bushee 2001). We use outstanding bank loans to measure debt-market pressure. Technical violations of covenants embedded in loan contracts may lead to the transfer of firm control from the management to creditors (Tan 2013; Chava and Roberts 2008). Consequently, managers may have incentives to avoid covenant violations by sacrificing investments in innovation when accrual management is constrained. Consistent with these predictions, we find that the negative effect of auditor conservatism on innovation is stronger if managers have greater equity- and debt-market incentives to focus on short-term earnings.

Next, we predict that the negative effect of auditor conservatism induced by increased auditor legal liability on client innovation is likely to be more pronounced for client firms with greater litigation risk. Clients' litigation exposure increases auditors' potential loss from audit failures, thus making auditors more conservative (Anantharaman et al. 2016). Finally, the literature argues that large audit firms are more conservative than small ones (DeFond and Zhang 2014). Large firms are often viewed as having deep pockets, which increases their litigation exposure (Anantharaman et al. 2016). Therefore clients of large audit firms may experience greater declines in innovation when auditor conservatism increases. We find robust evidence that the negative effect of auditor conservatism on corporate innovation is significantly more pronounced when firms are exposed to greater litigation risk and for clients of large audit firms.

We conduct several additional analyses to strengthen the key inferences. First, we show that the inferences are unaffected when we limit the sample only to the treatment firms. Thus the economic and political conditions in the control states do not drive our results. Second, we do not find statistically significant differences in innovation between treatment and control firms prior to the

shocks to auditor conservatism, which reinforces the inference that pre-existing differences between treatment and control firms are unlikely to explain the results. Third, we can exploit both positive and negative shocks to legal liability affecting auditor conservatism; we find that, while an increase in auditor legal liability decreases innovation, a decrease in it increases innovation. Fourth, for tighter research design, we employ a neighboring-state matched-sample design in which we compare innovation of firms in treatment states with those in their neighboring states. Because economic conditions in neighboring states are likely to be similar, this design mitigates concerns about omitted factors, such as local and industry differences between treatment and control firms. We continue to observe that an increase in auditor conservatism leads to a decline in innovation. Finally, our inferences are robust to alternative measures of innovation, changes in the sample period, clustering choices, and other sensitivity tests.

We contribute to the literature on how financial reporting affects corporate investment. Specifically, our study adds to the expanding literature on how auditing affects clients' investment decisions.[1] We contribute by illuminating how auditor conservatism affects managers' incentive to invest in innovation.

Our findings also add to the innovation literature (see He and Tian (2018) for a review). Several studies provide *suggestive* evidence that greater audit quality may spurs innovation by mitigating adverse selection and moral hazard (Park 2018; Lobo, Xie, and Zhang 2017; Li et al. 2016). Our evidence contrasts with this intuition by showing that auditor conservatism that is often associated with greater audit quality (DeFond and Francis 2005, p. 7) may exacerbate managerial

[1] Kausar, Shroff, and White (2016) find that clients' voluntary choice to engage an auditor acts as a signaling device that alleviates adverse selection problem. Specifically, firms obtaining voluntary audit increase their debt, investment, and operating performance and become more responsive to their investment opportunities. Bae et al. (2017) find that auditor knowledge and resources aid clients in enhancing investment efficiency. Cai et al. (2016) show that common auditors between merging firms facilitate the flow of information throughout the acquisition process, thereby enhancing the efficiency of merger and acquisition (M&A) outcomes. Their findings are also corroborated by Dhaliwal et al. (2016), who find that shared auditors lead to a greater likelihood of M&A transactions and lower deal premiums.

myopia and impede innovation.

Our findings also have policy implications. Regulators have grappled with the question of auditor legal liability for decades. The U.S. Department of the Treasury formed an advisory committee in 2007 to address the issue of liability caps for the audit profession. The committee consulted with auditors, investors, academic and legal experts regarding whether to limit auditor legal liability but "… was unable to reach a consensus as to whether limits on auditor liability would be beneficial or harmful to the capital markets and to investors or, for that matter, whether such limits are necessary to sustain the auditing profession" (Levitt and Nicolaisen 2008, p. VII:23). We contribute to this debate by highlighting one specific negative externality of increased auditor legal liability on innovation.

2. Hypothesis development

2.1. Auditor conservatism and auditor cost function

Auditors suffer larger losses from incorrectly issuing clean opinions than from incorrectly issuing going-concern opinions (Kaplan and Williams 2013). Similarly, their litigation and reputation losses derive mostly from income-increasing (rather than income-decreasing) earnings manipulations (DeFond, Lim, and Zang 2016). As such, auditors may have incentives to be conservative in their assessment of client financial statements (DeFond and Subramanyam 1998; Kim, Chung, and Firth 2003; Cahan and Zhang 2006). Following the literature, we refer to auditors' preference for conservatively reported financial statements as "auditor conservatism" (e.g., Lu and Sapra 2009; Francis and Krishnan 1999; Lennox and Kausar 2017).

While litigation and reputational concerns provide an auditor with incentives to be conservative, the client firm could opt to switch to a less conservative auditor if it perceives its auditor to be too conservative (DeFond and Zhang 2014). That is, an auditor may incur economic losses by

being too conservative. Further, auditors receive remuneration from the clients they audit whereas audit quality is difficult to observe for outsiders. These features may motivate auditors to cater to their clients. Thus a rational auditor's equilibrium level of conservatism will trade off the expected benefit of increased conservatism (i.e., reduced litigation and reputational costs) with the expected cost associated with the risk of losing a client. For example, when litigation risk increases for a client due to state-level changes in legal liability, ceteris paribus, an auditor's equilibrium level of conservatism may increase for that given client.

2.2. Auditor conservatism and client innovation

Investors and regulators often associate auditor conservatism with greater audit quality (DeFond and Francis 2005). For instance, auditor conservatism can enhance the perception of auditor independence, which may mitigate adverse selection and moral hazard (e.g., DeFond and Zhang 2014). As a result, greater auditor conservatism may lead to greater investment in innovation (Park 2018; Lobo, Xie, and Zhang 2017; Li et al. 2016). However, alternative arguments exist that suggest that conservative auditors may, in fact, exacerbate managerial myopia and impede innovation.

A large literature documents that managers manipulate accounting earnings to avoid covenant violations, decreases in earnings, or to meet or beat earnings thresholds (DeFond and Jiambalvo 1994; Burgstahler and Dichev 1997; Degeorge, Patel, and Zeckhauser 1999). However, when such manipulation is constrained, managers, being subject to capital market pressures, tend to reduce investments in innovation (Graham et al. 2005; Cohen and Zarowin 2010; Zang 2012; Burnett et al. 2012; Chang et al. 2015). Reduction in R&D expenditures immediately improves earnings, whereas the growth prospects realized from such investments are uncertain and may take several years to materialize (Wang and D'Souza 2006). Thus we argue that, if managers expect that conservative auditors will limit their discretion in manipulating accrual-based earnings, they may invest less in

innovation to meet or beat earnings thresholds without having to manipulate earnings by accrual management.

Further, note that, while the success of R&D projects is likely to increase a firm's competitive advantage and boost its growth prospects, the manager making these investments often does not stay long enough to enjoy the fruits of risk-taking if shareholders do not tolerate short-term failures (e.g., Baber, Fairfield, and Haggard 1991). To the extent that managerial compensation is tied to accounting performance directly or indirectly, greater auditor conservatism is likely to result in intolerance for failures. The literature suggests that tolerance for failure and short-term underperformance is important to incentivize managers to invest in innovation (Manso 2011; Tian and Wang 2014). Based on the above arguments, we formulate our hypothesis (stated in the null form) as follows.

H1: Changes in auditor conservatism do not affect R&D investment and innovation.

The above discussion suggests that increased auditor conservatism could harm innovation. However, when faced with a conservative auditor, the client firm may want to switch to a less conservative auditor (DeFond and Zhang 2014). Switching is not costless. Investors often react negatively to auditor switches, especially if they perceive that the client firm is seeking a more lenient auditor (DeFond and Zhang 2014). Besides, while auditor conservatism may damp innovation, it may also result in other benefits. For example, studies find that conservative reporting can enhance debt-contracting efficiency and reduce agency costs (e.g., Chy, De Franco, and Su 2020). From a rational client firm's perspective, an endogenously chosen level of auditor conservatism will reflect the optimal trade-off between the cost and benefit of auditor conservatism.[2]

[2] In our setting, the changes in auditor conservatism are induced by regulatory changes, rather than chosen by the client firm. For example, switching to another less conservative auditor, an endogenous response by the client to auditor conservatism, may not be easy in our setting, because the new auditor may still be subject to increased legal liability for ordinary negligence.

3. Identification and research design

3.1 Identification

3.1.1 Institutional setting

A key challenge in studying the effects of auditor conservatism on innovation is that auditor conservatism is difficult to measure. Proxies, such as discretionary accruals or going-concern opinions, while informative, capture auditor conservatism with significant measurement errors. Besides, omitted correlated variables can bias the regression estimates, impeding interpretation of the regression results. To circumvent these issues, we exploit state-level variations in auditor legal liability as a means of identification. This choice is motivated by a large stream of theoretical and empirical literature showing that litigation risk for audit failures encourages auditors to conservatively assess clients' financial statements (e.g., Lu and Sapra 2009; Anantharaman et al. 2016; DeFond and Zhang 2014).

Auditors are exposed to litigation risks at both the federal and state levels (Anantharaman et al. 2016). Because the federal law applies to all firms, identification by means of variations in litigation exposure is difficult, due to lack of a control group. Instead, we exploit variations in auditors' litigation exposure to third parties at the state level over time. Under common law, third-party auditor legal liability at the state level is determined by legal precedents and is based on one of the three theories: (1) privity (or near privity) approach, (2) restatement of torts, and (3) reasonable foreseeability (Gaver, Paterson, and Pacini 2012; Vick 1993; Anantharaman et al. 2016). The privity approach generally allows only clients to recover losses due to auditor negligence (Anantharaman et al. 2016). Restatement of torts allows the intended beneficiaries to recover damages from auditors (Vick 1993). The reasonable foreseeability standard allows any reasonably foreseeable party who relies on the audit to recover in case of damages (Scherl 1994). In short, privity and reasonable foreseeability stand at the two opposite extremes of auditor liability to third parties, with the

restatement of torts being in between.

We gather precedent-setting court decisions on changes in state-level auditor legal liability between 1970 and 1995 and use these state-level variations in legal liability to examine our research question. We rely on studies in law journals to gather all precedent-setting cases relating to auditor legal liability. Scherl (1994) lists all state-level precedent-setting cases governing auditor legal liability. We cross-check these cases with other studies (Wiener 1983; Dulle 1987; Vick 1993; Gaver, Paterson, and Pacini 2012). We then collect the transcripts of all the cases and read the transcripts to verify the precedents. Precedent-setting common law verdicts specifically and clearly mention that the case in question is the first impression under the jurisdiction. These transcripts also mention the precedents in other jurisdictions, which allows us to further cross-check the listed cases in law journals. See Appendix A for further discussion of the institutional background. Appendix B details all shocks to auditor legal liability.

Several features of our setting that suggest that our identification strategy is plausibly exogenous. First, our quasi-natural experiment is not based on state regulations, whose passage could sometimes be subject to lobbying efforts. Instead, the experiment involves common-law judicial verdicts decided by court judges, who are independent of the state government and are deemed immune to state economic and political pressures (Klasa et al. 2018). Second, in deciding on the appropriate level of auditor liability to third parties, court judges intend to strike a balance between auditor's exposure to indefinite *extra-contractual* liability and the damages that noncontractual parties may still suffer by relying on auditor assurance (MacKey 1993). As such, courts' decisions regarding auditor legal liability are not intended to hurt innovation outputs of the clients. Nonetheless, changes in common law precedents are not random events, because court rulings that alter precedents result from past litigation aimed at changing prevailing precedents. Besides, factors that lead to lawsuits and changes in court precedents are likely to be similar for each of the court rulings used in

the sample, making the state-level auditor conservatism measure correlated across states. These limitations of the setting pose challenges in drawing strong inferences.

3.1.2 State jurisdiction for third-party auditor legal liability

An important aspect of our research setting is to identify the state jurisdictions under which auditors could be held liable by third parties for ordinary negligence. As per Section 145 of the Restatement (Second) Conflict of Laws, in deciding the state of jurisdiction for ordinary negligence claims under tort theories, courts adopt the "most significant relationship" approach (Anantharaman et al. 2016).[3] Under this approach, the contacts that Section 145 considers in deciding the states of jurisdiction are (1) the place of incorporation and the place of business of the parties, (2) the place where the injury or conduct occurred, and (3) the place where the relationship between the parties is centered. These contacts merit consideration according to their relative importance with respect to the particular issue.

Anantharaman et al. (2016) map the states of the most significant relationship as expounded in tort theories above into (1) client states of headquarters and incorporation and (2) states of auditor engagement office or auditor location. Note that auditors could be subject to legal liability for ordinary negligence, even if auditor engagement office is located in a different state than client business location or incorporation state, provided the most significant relationship between the auditor and the client firm occurs in the client business location state.

3.2 Sample selection

We use patent and citation data from Kogan et al. (2017), who provide firm-year CRSP

[3] "The rights and liabilities of the parties with respect to an issue in tort are determined by the local law of the state which, with respect to that issue, has the most significant relationship to the occurrence and the parties."- Section 145 of the Restatement (Second) Conflict of Laws.

permno-matched patent and citations data from 1926 to 2010.[4] We collect data on financial statement items from Compustat and stock-price data from CRSP. Our sample period begins in 1970 (when data on R&D become available in Compustat) and ends in 1998, three years after the last shock to auditor legal liability. In additional analyses, we also consider different beginning and ending (more recent) sample periods. We drop firms in financial and utility industries, firms with missing SIC industry code, and firms having zero, negative, or missing assets and sales. The measurement of auditor legal liability hinges on firms' incorporation and headquarter states; as a result, we drop firms incorporated or headquartered outside the United States. In our research design, firms that are located or incorporated in states that undergo auditor legal liability changes are treatment firms and firms that are located or incorporated in states that do not change auditor legal liability during the sample period are control firms. Because treatment firms may become control firms (and vice versa) by changing the state of location during the sample period, we drop all firm-years two years leading up to as well as after a change in a firm's headquarters state.[5] This mitigates concerns that our inferences are confounded by any potential changes in the state of location in response to auditor legal liability shocks.[6] We also restrict the treatment sample to firms that have nonmissing data both in the year prior to and in the year following state-level changes in auditor legal liability. Application of these filters results in 63,976 firm-year observations.

3.3 Variable construction and measurement

3.3.1 Auditor conservatism measure: state-level variations in auditor legal liability

To measure auditor conservatism, we construct an indicator variable *More_Aud_Liab* based

[4] Studies mostly use NBER patent and citation data (Hall and Jaffe 2001). Kogan et al. (2017) correct and improve upon the existing NBER dataset.
[5] In our sample period, 356 firms change their state of headquarters, but no firm changes the state of incorporation.
[6] Our inferences are unaffected if we do not apply this sample filer.

on state-level variations in auditor legal liability. Anantharaman et al. (2016) suggest that courts generally adopt the "most significant relationship" approach in determining which state court will hear cases against auditors. They argue that the client state of incorporation, location, state of audit engagement office, or state of audit firm head office are most likely the jurisdictions in which cases against auditors will be heard. As such, we set *More_Aud_Liab* to one if either client state of headquarters or incorporation or both change from low liability to high liability regime and zero otherwise.[7,8] *More_Aud_Liab* reverts from one to zero if a state where a firm is located or incorporated decreases auditor legal liability. *More_Aud_Liab* remains zero throughout the sample period for firms in control states. Note that our identification strategy does not rely on the assumption that a client chooses auditor from its headquarters or incorporation state but that the most significant relationship between the client firm and auditor in terms of the audit occurs in the client headquarters or incorporation state. Further, when one of the client's headquarters or incorporation states has high liability but the other does not, we rely on the assumption, following Anantharaman et al. (2016), that the auditor's ex-ante decision process to be conservative in evaluating client financial statements will likely consider the law under which litigation threats to the auditor are stronger.

3.3.2 Measures of innovation

We use two patent-based metrics to measure innovation (e.g., Tian and Wang 2014; He and

[7] States of headquarters reported in Compustat may be misstated because Standard and Poor's backfills firms' previous headquarters locations with the most recent business addresses. To mitigate this problem, we use the states of business location listed in firms' 10-K filings with the SEC's EDGAR. Because the SEC did not require electronic filings of 10-K until May 1996, we backfill firm-headquarters states from the first instance of business-location appearance in 10-K filings.

[8] Auditor engagement office/location data are not available before 1999. Anantharaman et al. (2016) suggest that, besides client state of headquarters/incorporation, auditors could be subject to legal liability in the state of the auditor engagement office when the auditor engagement office is located in a different state than client headquarters state. For the sample of firms that have client headquarters in a different state than auditor engagement office state, if the client engagement office has higher liability than client state of location, then our coding may potentially *underestimate* the treatment effect as some treatment firms will be coded as control firms.

Tian 2013; Atanassov 2013). The first measure is the total number of patent applications a firm files for in a given year that are eventually granted. However, this measure fails to fully take into account the quality of innovation, because both incremental technological discoveries and truly groundbreaking ones receive the same weight. Consequently, our second measure is based on citation counts, counting all future (nonself) citations that a firm's patent portfolio in a given year receives in total. We set the patent counts and citation counts to zero for firm-years without available patent and citation information. In addition, the distributions for patent counts and citation counts are right-skewed; hence we take the natural logarithm of one plus the patent counts (*logPatent*) and citation counts (*logCite*) as the two measures of innovation. In robustness tests, we also consider alternative measures of innovation.[9]

3.3.3 Control variables

We control for time-varying firm characteristics that the literature has shown to be associated with firm-innovation output, auditor conservatism, or both (e.g., Tian and Wang 2014; Anantharaman et al. 2016; Atanassov 2013). These include *Size* (the natural logarithm of book value of assets), *Age* (the natural logarithm of one plus the number of years a firm appears in the Compustat database), *ROA* (income before extraordinary items scaled by the beginning book value of assets), *Leverage* (total short- and long-term debt scaled by assets), *CAPEX/Assets* (capital expenditures scaled by assets), *PPE/Assets* (property, plant, and equipment scaled by assets), and *MKBK* (market-to-book ratio). In addition, we include industry concentration as measured by the Herfindahl-Hirschman index at the two-digit SIC industry-code level (*HHI*), squared HHI (*HHISquared*), and stock liquidity

[9] Note that consistent with the innovation literature (He and Tian 2013; Atanassov 2013), we count patents and citations in the year of application, not in the year when such application is granted. To illustrate, suppose a firm files for a patent in 1990 but the application is granted in 1993. In our coding of patent, we code the innovation to have occured in 1990, not in 1993. All subsequent citations accruing to this patent are likewise counted in 1990.

(*Liquidity*).[10] All continuous variables, except for *Age*, are winsorized at the 1% and 99% levels to mitigate the effect of outliers. All variables are defined in Appendix C.

3.4 Empirical methodology

Our research design employs a generalized difference-in-differences approach (e.g., Bertrand and Mullainathan 2003; Klasa et al. 2018; Acharya, Baghai, and Subramaniam 2013) in which we compare the average within-firm differences for treatment firms after and before the treatment (i.e., shocks to auditor legal liability) with the same (after and before) differences in control firms. The treatment effect thus captures the effect of increases in auditor conservatism (induced by state-level regime change of auditor legal liability) on innovation. We use ordinary least squares (OLS) regressions and estimate variants of the following specification.[11]

$$Innovation_{i,t+1} = \beta_2 More\_Aud\_Liab_{i,t} + X_{i,t}\beta_3 + \gamma_i + \mu_t + \epsilon_{i,t+1}. \qquad (1)$$

β_2 captures the causal effect of an increase in auditor conservatism among treatment firms, relative to control firms, on next year's innovation. X_{it} is the matrix of all time-varying control variables measured contemporaneously, and β_3 is a column vector of coefficients associated with these control variables. Importantly, we include firm fixed effects (γ_i) to account for any time-invariant firm-specific heterogeneity affecting the estimates and year fixed effects (μ_t) to mitigate the effect of secular time trend and macroeconomic conditions that may affect the universe of firms in the sample. Finally, $\epsilon_{i,t+1} \sim N(0, \sum_g)$ is the error term such that arbitrary correlation within-group *(g)* is allowed. We cluster standard errors at the firm level to mitigate the overstatement of statistical

[10] We include both *HHI* and *HHIsquared* as control variables because Aghion et al. (2005) show that industry competition has a nonlinear effect on firm innovation.

[11] One could argue that Poisson or Negative Binomial estimation methods should be used, instead of OLS, given the count nature of patent/citations. Similarly, for binary outcome variables (such as issuance of a going-concern opinion), Logit/Probit regression could also be employed. However, inclusion of a large number of fixed effects in Logit/Probit/Poisson/Negative Binomial models may result in inconsistent estimates due to the incidental-parameter problem (Lancaster 2000). Inferences are unaffected if these methods are used.

significance owing to serial correlation in the error term. Because *More_Aud_Liab* varies at the state of location and incorporation level, clustering at the state of location and incorporation is also another option. However, recent evidence in Mackinnon and Webb (2017) suggests that, when cluster sizes are unbalanced, we may need substantially a greater number of clusters to obtain consistent standard errors than the number of U.S. states.[12]

4. Empirical analyses

4.1 Descriptive statistics

Panel A of Table 1 reports the means, medians, and standard deviations of the variables used. The mean value of *More_Aud_Liab* is 0.34, suggesting that 34% of firm-year observations fall in high auditor legal liability regime. An average firm has 5.7 (79.2) annual patent (citation) counts. However, the median values for both of them are zero, implying that both patent and citation counts are right-skewed. Accordingly, we make the logarithmic transformation of (one plus) patent and citation counts - *logPatent* and *logCite*. Firms on average allocate approximately 3.3% of assets to R&D expenditures, have an average market-to-book ratio of 1.47, and ROA of 0.006. *ModGC, SmallProfit*, *Restate*, and *TAC* have means of 0.266, 0.15, 0.11, and -0.032, respectively, which are comparable to prior studies (e.g., Anantharaman et al. 2016; Aobdia 2019). In Panel B, we tabulate the number of unique firms for each treatment state in the sample. The sample selection filter requires a firm to have nonmissing data both the year before and the year after shocks to state-level auditor legal liability. California has the largest number of unique firms (511) in the sample, with Texas (263), Florida (140), Ohio (121), and Pennsylvania (107) making the top five states.

[12] Inferences remain unaffected if we apply different clustering choices. See Section 5.5 for sensitivity analyses on alternative clustering choices.

4.2. Validation of the state-level auditor-conservatism measure

Prior to our main analyses, we *validate* the state-level auditor conservatism measure *More_Aud_Liab*. We do so by examining whether *More_Aud_Liab* predictably affects various aspects of auditor conservatism and audit process quality used in prior studies. Because different measures of auditor conservatism and audit quality capture different aspects of the auditing process (Che, Hope, and Langli 2020), we use several measures to increase the generalizability of the evidence. Following prior studies (e.g., Anantharaman et al. 2016; Aobdia 2019), we use the contemporaneous values of the dependent variables in these validation tests.

Modified GC Opinion. Our first measure of auditor conservatism is the issuance of modified going-concern opinion (*ModGC*). A large number of studies have used modified going-concern opinions as a measure of audit process quality (e.g., Che et al. 2020; Aobdia 2019, Anantharaman et al. 2016). These opinions are auditor's most direct communication with outsiders about the audit process and its outcome (DeFond and Zhang 2014). Accordingly, we construct *ModGC* as an indicator variable that takes one if the auditor issues a modified going-concern opinion in the current fiscal year and zero otherwise.[13] Table 2, Column 1, reports the results. We find that treatment firms are more likely to receive modified going-concern opinions than control firms following state-level increases in auditor legal liability. The coefficient estimate is statistically significant at the 1% level.

Type-I Errors and Type-II Errors. To explore further the issuance of a modified going-concern opinion following increased state-level auditor legal liability, we define *Type-I Error* (*Type-II Error*), which equals one if the auditor issues (does not issue) a going-concern opinion and the firm does not go bankrupt (goes bankrupt) within the next year and zero otherwise.[14] Type-I errors capture

[13] Compustat provides data on auditor opinions only from 1988, so our tests relating to *ModGC* use the sample period from 1988 to 1998. Furthermore, Compustat codes auditor opinions in a range between 1 and 5, where 1 is unqualified opinion. Anantharaman et al. (2016) use Compustat variable *auop=4* as indicating going-concern opinions. We follow their procedure.

[14] We collect all Chapter 7 and Chapter 11 filings from Chava and Jarrow (2004) and Chava, Stefanescu, and Turnbull

auditor conservatism and Type-II errors capture auditor aggressiveness. We see in Columns 2 and 3 that *More_Aud_Liab* loads positively for Type-I Error but does not affect Type-II Error. In other words, auditors are willing to make Type-I errors to err on the side of caution but not Type-II errors following increases in legal liability.

Restatements. Aobdia (2019) finds that restatements are predictably associated with (i) PCAOB inspection deficiencies and (ii) audit firms' internal inspection deficiencies. We explore how increased auditor legal liability affects the likelihood of restatements. During the main sample period (1970–1998), we do not have restatement data available in Audit Analytics. We tackle this data limitation by examining the effect during the 1999–2017 sample period when Audit Analytics data are available. A caveat with this later sample period is that we do not have any time-series variations in our measure of auditor litigation risk, *More_Aud_Liab*. Thus our test is based on the cross-sectional variations in *More_Aud_Liab* and hence less powerful.[15] We tabulate the result in Table 2, Column 4. We find a strong negative effect of *More_Aud_Liab* on *Restate*, which takes the value of one if a firm has an accounting restatement in a fiscal year and zero otherwise.

Meet or Beat Earnings Thresholds. Following Aobdia (2019), we use the propensity to meet or beat the zero-earnings threshold as another measure of audit quality. *SmallProfit* is an indicator variable that takes one if current year's ROA is between 0 and 0.03 dollar and zero otherwise. The results in Column 5 suggest that greater auditor legal liability leads to a reduced likelihood of meeting or beating the zero-earnings threshold.

Accruals-based Measures. In Columns 6–8, we employ accruals-based proxies as measures of audit quality. In Column 6, we use total accruals scaled by lagged book assets (TAC) as a measure of managerial discretion in financial reporting. We do not find strong evidence of a decrease in TAC.

(2011).

[15] Because we do not have within-firm variations in auditor-liability shocks in this period, we replace firm fixed effects with industry (SIC three-digit) fixed effects.

In Columns 7–8, we construct unsigned measures of both income-increasing and income-decreasing TAC. |TACpos| is the absolute value of all TAC greater than zero, and |TACneg| is the absolute value of all TAC less than zero. We find evidence of a decrease in income-increasing absolute accruals in Column 7 but no change in income-decreasing absolute accruals. This evidence is consistent with increased legal liability making auditors more conservative about income-increasing managerial discretion than about income-decreasing discretion.

Other Tests. We also examine the effect of increased auditor legal liability on audit fees. Auditors can respond to increases in litigation risk by exerting greater effort, charging their clients higher risk premium, or both, all of which could increase in audit fees (DeFond and Zhang 2014). Because audit fee data are not available in Audit Analytics during our main sample period, we conduct the analysis for the 1999–2017 sample period instead. Untabulated analysis suggests a positive effect of *More_Aud_Liab* on audit fee, but the coefficient estimate is not statistically significant.[16]

Summary. The evidence in these validation tests suggests that increases in state-level auditor legal liability make auditors more likely to issue modified going-concern opinions and more willing to commit Type-I errors. Increased auditor legal liability also leads to a decrease in accounting restatements, makes auditors more likely to constrain the propensity to meet or beat the zero-earnings threshold, and reduces manager's income-increasing accrual discretion. Overall, these results are consistent with the literature that greater auditor litigation risk leads to greater auditor conservatism (DeFond and Zhang 2014, p. 278).

[16] For the audit-fee test, we only have cross-sectional variations in *More_Aud_Liab* in the 1999–2017 sample period, making the test less powerful. Further, an increase in audit fees involves bargaining between the auditor and the client firm in which the client firm must agree to increased fees (DeFond and Zhang 2014). When faced with a conservative auditor who is more likely to issue modified going-concern opinions or constrain accrual discretion, the client may be reluctant to pay higher audit fees.

4.3. Main results

Having validated the measure of auditor conservatism, *More_Aud_Liab*, we now turn to our main research question and investigate how increased auditor conservatism affects corporate innovation. As a first step, we examine how auditor conservatism affects the current year's corporate investments in R&D, scaled by the book value of assets. Column 1 of Table 3 suggests that firms decrease their investments in R&D substantially when auditor conservatism (*More_Aud_Liab*) increases. An average treatment firm decreases investment in R&D by 6% (=0.002/0.033), relative to control firms, following an increase in auditor conservatism. The estimate is statistically significant at the 1% level and economically meaningful. Because some firms may strategically choose not to disclose R&D expenditures, our estimates of R&D investments could be measured with error. Koh and Reeb (2015) suggest that replacing missing R&D with zeros (as in Table 3 Column 2) or industry averages (as in Table 3 Column 3) and including an indicator variable for missing R&D mitigate measurement error in R&D. We follow these procedures in Columns 2 and 3 and observe that our inferences remain unchanged.

Our evidence on R&D investments suggests that increased auditor conservatism exacerbates managerial myopia. However, R&D is an input-based measure and contains significant measurement error (Koh and Reeb 2015). Thus, following prior studies (He and Tian 2013; Faleye, Hoitash, and Hoitash 2011; Brav et al. 2018), we use patent-based proxies as our main measure of innovation. Column (1) of Table 4 tabulates the effect of an increase in auditor conservatism on firms' patenting activities next year. The effect is negative and statistically significant at the 1% level. Treatment firms experience approximately a 6.9% decline in patents granted relative to control firms. Column (2) reports the estimates concerning the second measure of innovation, *logCite.* An increase in auditor conservatism decreases the number of annual citation counts by approximately 10.2% for the treatment firms, relative to the control firms. The coefficient estimate is both statistically (at the 1%

level) and economically significant. Further, the economic magnitudes are comparable to prior studies. For example, He and Tian (2013) find that loss of one analyst following a firm leads to 18.2% increase in patents (29.4% increase in citations) over a three-year window. Bradley, Kim, and Tian (2017) find that passing a union election results in an 8.7% (12.5%) decline in patents (citations). Other studies also show comparable magnitudes (see He and Tian (2018) for a review of this literature.)

Some studies use two- or three-year ahead patent/citation measures as proxies for innovation (He and Tian 2013; Atanassov 2013). Following these studies, we also examine the robustness of the inferences using two-year-ahead patent and citation measures in Columns 3 and 4. We also use three-year-ahead measures in untabulated analyses. Across all these specifications, we find statistically and economically significant adverse effects of auditor conservatism on innovation.

Regarding the time-varying firm characteristics used as controls, the estimates suggest that *Size*, *Age*, *MKBK*, and *PPE/Assets* are all positively associated with innovation. Thus larger, older, more valuable firms, and those with more fixed assets innovate more. *Leverage* has a negative sign, as firms borrowing more may be reluctant to invest further in innovation due to risk considerations. Industry concentration has a nonlinear effect on innovation, but the effect is not statistically significant. Stock-market illiquidity has a negative effect on innovation, suggesting that greater costs of adverse selection could reduce innovation outputs. The negative sign on *ROA* could be explained by the requirement that R&D expenditures be expensed, implying a mechanical negative association between innovation input (R&D) and ROA.

In untabulated analyses, we drop all time-varying controls to obtain baseline estimates for the effect of increased auditor conservatism on corporate innovation. The coefficient estimates are not materially different in terms of economic magnitudes from those reported in Table 4 Columns (1) and (2). The statistical significance is also comparable. Thus our inferences are not sensitive to the

inclusion or exclusion of time-varying controls.

4.4. Limiting the sample to the treatment states only

In our main analyses, we use firms located or incorporated in states that undergo changes in auditor legal liability as the treatment group. Firms located and incorporated in states that do not undergo changes in auditor legal liability during the sample period comprise the control group. A question in this setting is whether the treatment effect actually derives from firms in the treatment states or it is dependent on the choice of a control group. To tackle this issue, we retain firms in the treatment group only and drop all those from the control group. In this framework, the treatment firms themselves form the control group until they receive treatment. We re-estimate the treatment effect in Table 5. Columns 1–7 provide results for the validation of state-level auditor conservatism measure.[17] Column 8 tabulates results for R&D expenditure and Columns 9 and 10 show the results for the innovation proxies. Our inferences in the "treatment states only" sample are consistent with the full sample in Tables 2–4. The coefficient magnitudes and statistical significances are not materially different from the full sample. Thus, because our results obtain in the "treatment states only" sample too, the key inferences are not driven by economic, political, or industry conditions in the control states.

4.5. Evolution of treatment effects

We examine the evolution of the treatment effects in Table 6. We construct *More_Aud_Liab* (t=-2) and *More_Aud_Liab* (t=-1), which equal one if the firm is located or incorporated in a state

[17] We cannot estimate the effect of *More_Aud_Liab* on restatements in the treatment state only sample in Table 5 (and the evolution of treatment effect in Table 6), because the restatement test is based on a recent sample period with no time-series variations in *More_Aud_liab.*

that will increase auditor legal liability in two years and in one year respectively and zero otherwise.[18] These two variables test the parallel-trends assumption underlying the difference-in-differences research design. Columns 1–7 report results for the validation tests of state-level auditor conservatism measure *More_Aud_Liab*. Column 8 shows the results for the R&D expenditure test and Columns 9 and 10 report results for corporation innovation tests. Across Columns 1–10, we find that *More_Aud_Liab* (t=-2) and *More_Aud_Liab* (t=-1) are not statistically significant. This implies that, prior to the shocks to auditor legal liability, treatment and control groups do not differ significantly from each other in terms of audit quality, R&D expenditure, and innovation. Thus this evidence suggests that the parallel-trends assumption is likely to be satisfied. Next, we construct *More_Aud_Liab* (t=1), *More_Aud_Liab* (t=2), *More_Aud_Liab* (t=3), and *More_Aud_Liab* (t>=4) are indicator variables that equal one if a firm's state increased auditor legal liability in the current year, one year ago, two years ago, and three or more years ago, respectively, and zero otherwise. These variables capture the gradual evolution of the treatment effects. The effects are generally persistent, except for accruals and *SmallProfit*. Second, whereas we do not see an effect for *TAC* in Table 2, we find a decrease in *TAC* in the first two years of increased legal liability in Table 6. Third, for patent and citations, the magnitude of the treatment effects becomes larger the further forward we go in event-time from the initial shocks, consistent with the path-dependent nature of innovation.

4.6. Cross-sectional analyses

In this section, we conduct cross-sectional analyses to deepen the understanding of the mechanism through which auditor conservatism affects firm innovation.[19] We focus on the two main measures of innovation, patents and citations, in these analyses. Our arguments imply that firms that

[18] For a clean test, we drop firms in California (New Jersey) after 1992 (1995), when California (New Jersey) decreased auditor legal liability.
[19] The number of observations in these cross-sectional analyses varies due to data limitation.

face greater equity and debt-market pressures are more likely to experience reductions in innovation when audited by conservative auditors. We measure equity-market pressures with two proxies: analyst coverage and short-term oriented institutional owners. A firm's incentive to cut investments in innovation to meet earnings thresholds is likely to be higher if analyst pressure to meet earnings thresholds is greater (Bhojraj et al. 2009; He and Tian 2013). Similarly, managers are more likely to focus on short-term earnings when the ownership structure is populated by short-term oriented institutional investors (Bushee 2001). These arguments imply that auditor conservatism may exacerbate managerial myopia for firms with greater analyst coverage and short-term institutional ownership. We gather analyst-coverage data from I/B/E/S, institutional-ownership data from Thomson Reuters (Form 13F), and firms' transient institutional ownership data from Bushee (1998). We define analyst pressure to be high (*High_Analyst_Pressure*) if analyst coverage for a firm is greater than the industry-year (three-digit SIC) median. We construct *High_STInstOwn* that takes one if a firm's transient institutional ownership is greater than the industry-year (three-digit SIC) median and zero otherwise. In Columns 1 and 2 (3 and 4) of Table 7, we observe that the adverse effects of auditor conservatism are significantly more pronounced for firms that have greater analyst pressure (higher short-term investor ownership).

In Columns 5 and 6, we examine how auditor conservatism affects innovation when a firm faces debt-market pressures to meet performance targets. Faced with conservative auditors, managers may cut investments in innovation to avoid earnings-based covenant violations. We measure debt-market pressure to meet earnings targets by *High_Debt_Pressure*, which takes the value of one if the total amount of outstanding bank loans (scaled by book assets) in a given fiscal year is greater than the (SIC three-digit) industry-year median and zero otherwise.[20] We observe that the effect of

[20] We use the DealScan database to construct *High_Debt_Pressure*. We use bank loans to measure debt market pressure, because debt covenants in bank loans are more intensely monitored than corporate bonds. Further, almost all bank loans contain covenants whereas bond issues may come without covenants (Christensen and Nikolaev 2012; Nikolaev 2010).

increased auditor conservatism on innovation outputs is more pronounced when the debt market pressure is higher.

If auditor conservatism induced by increased legal liability impedes corporate innovation, the effect is likely to be more pronounced for client firms that are more likely to be sued. We construct an indicator variable *KS_High* that takes one if Kim and Skinner (2012)'s litigation-risk measure is greater than the industry-year median and zero otherwise. Columns 1 and 2 of Panel B, Table 7, report the results. The treatment effect is significantly more pronounced for firms with higher litigation exposure. Finally, prior evidence suggests that large audit firms are more exposed to litigation risk, because they are perceived as deep pockets (e.g., Anantharaman et al. 2016). As such, firms with big auditors are more constrained in their accruals manipulation. This suggests that any change in auditor conservatism may affect the innovation outputs of clients of large audit-firms more.[21] Table 7, Panel B, Columns 3 and 4, suggest that the effect is indeed stronger among clients audited by large audit firms.

These cross-sectional analyses further corroborate our inferences that firms reduce investments in innovation in response to conservative auditors precisely when cutting investments in innovation most likely helps managers avoid costly career consequences. These results also show that the adverse effects of auditor conservatism follow a predictable pattern, with the treatment effect aligning with the incentives of managers to reduce investment in innovation.

5. Robustness tests

5.1. Decrease in auditor legal liability and corporate innovation

[21] *BigN* takes 1 if a firm is audited by one of the large auditors and zero otherwise. Arthur Andersen, Arthur Young and Co., Coopers and Lybrand, Ernst and Whinney, Deloitte Haskins and Sells, Peat Marwick Mitchell/KPMG, Price Waterhouse, Touche Ross (and later the resulting mergers) constitute the *Big-N* firms in the sample. All other audit firms are *Non-BigN*.

The analyses in Section 4.3 suggest that auditor conservatism induced by legal liability has a negative externality in the form of a reduction in innovation measured by patent-based metrics. In this section, we take advantage of a particular feature of the research design to bolster our inferences. As mentioned earlier, state courts expanded auditor legal liability, starting from the early 1970s, and continued the trend until the 1980s. In the 1990s, however, two states, California (1992) and New Jersey (1995), *decreased* auditor legal liability. If an increase in auditor litigation risk damps innovation through the channel of auditor conservatism, then it follows that a decrease in liability is likely to boost innovation. To investigate this further, we use California and New Jersey as treatment states and all other states as control states. For a clean test, we drop firms in states that increase auditor legal liability. We use the sample period between 1989 and 1998 (three years before the California shock and three years after the New Jersey shock) to conduct these analyses. We define *Less_Aud_Liab* as an indicator variable that equals one if a firm's state of location or incorporation decreases auditor legal liability and zero otherwise. Table 8, Panel A, presents the results. Column 1 shows that, relative to control firms, treatment firms experience an estimated increase of approximately 13% in the number of patents applied for and eventually granted. Column 2 shows that a decrease in auditor legal liability increases the number of annual citations by approximately 22% for the treatment firms, relative to the control firms. The estimates are statistically significant at the 1% level.[22]

[22] In untabulated analyses, we also confirm that auditors become less conservative following a decrease in auditor legal liability. Further, we conduct the parallel-trend test with *Less_Aud_Liab (t=-2)* and *Less_Aud_Liab (t=-1)*, which equal one if the firm is located or incorporated in a state that will decrease auditor legal liability in two years or one year, respectively, and zero otherwise. The estimated coefficients for these variables are statistically indistinguishable from zero, suggesting that the parallel-trends assumption is satisfied. Finally, the evolution of treatment effect for the negative-shock sample are generally consistent with the results in Table 6.

5.2. Evidence from neighboring-state matched-sample design

As an additional robustness test, we adopt neighboring-state matching (e.g., Huang 2008; Dube, Lester, and Reich 2010; Dou, Khan, and Zou 2016). Specifically, we use each treatment state's neighboring state as its control sample.[23] To illustrate, consider a firm in North Carolina and that North Carolina increases auditor legal liability. We subtract the patent (citation) count of the firm in North Carolina before the shock from the patent count for the same firm after the shock (i.e., first difference). However, other factors, such as economic and political conditions, may have affected this within-firm difference. Consequently, we take another firm from South Carolina as a control for the firm in North Carolina. Economic and political conditions are arguably similar in both states and thus South Carolina firms are likely to be good controls for North Carolina firms. We take the same difference between post and pre-shock for the South Carolina firm (i.e., second difference). We then subtract the second difference in South Carolina firm from the first difference in the Norht Carolina firm to arrive at the difference-in-differences estimate. To further mitigate concerns about omitted factors, we tighten the event window by considering six years pre- and post-shocks.[24] Table 8 *Panel B* reports the results. The control variables have the same sign and similar magnitude to the full sample but are omitted for brevity. More importantly, we find that inferences continue to hold.

5.3. Auditor turnover and auditor switches

Our evidence suggests that auditor conservatism leads to a decline in client innovation output. To the extent that this adverse real effect is costly, a client firm could endogenously switch to a less conservative auditor. Switching auditor is not costless, however. Investors often respond negatively to auditor switches, especially if they perceive that the client firm seeks greater leniency (DeFond and

[23] We drop Hawaii from the sample because it does not have any contiguous neighbors.
[24] In untabulated analyses, we also consider other windows such as four or eight years. Inferences remain unaffected.

Zhang 2014).

We empirically test whether client firms change their auditors following increased auditor conservatism. Table 8, Panel C, reports the results. *Switch* takes the value of one if a client switches to a different auditor in the current fiscal year and zero otherwise. Column 1 shows no statistically significant effect of *More_Aud_Liab* on auditor switches. Another possibility is that firms could switch to non-BigN auditors from BigN auditors, because the latter are perceived to be more conservative (Anantharaman et al. 2016). However, in Column 2, we do not see any significant effect on switch to Non-BigN auditor either.[25] Finally, we control for auditor switches in our main tests. Columns 3 and 4 show that the effect of *More_Aud_Liab* on firm innovation is statistically and economically significant after controlling for auditor switches.

5.4. Choice of state jurisdiction and delaware-incorporated firms

We use both states of headquarters and incorporation in our definition of *More_Aud_Liab*, following Anantharaman et al. (2016), who use the exposition in tort theories in mapping the states where auditors may be subject to legal liability for ordinary negligence. This coding scheme assumes that the most significant relationship between the auditor and the client occurs either in the client's state of headquarters or incorporation. To test the sensitivity of this assumption, we define *More_Aud_Liab* at the state of client headquarters only. *More_Aud_Liab* equals one if the client's state of headquarters increases auditor legal liability and zero otherwise. The assumption would be that the most significant relationship between the auditor and client occurs only in the clients' state of headquarters. While this assumption is more restrictive than the assumption in our main tests (and

[25] However, it would be too strong to interpret the absence of significant results in these tests as evidence that changes in auditor conservatism or auditor legal liability do *not* affect client's decision to switch to a less conservative auditor. The reason is that our empirical setting is not particularly well suited for examining the effect of auditor liability changes on auditor turnover, because the new auditor could still be subject to the same legal liability, even if the new auditor operates from an engagement office in a different state.

different from that employed by Anantharaman et al. 2016), the robustness test nonetheless allows us to gauge the sensitivity of the results to different assumptions underlying our identification strategy. In Panel D of Table 8, Columns 1 and 2 show that our inferences continue to hold.

In Columns 3 and 4, we conduct another sensitivity test. Approximately 55% of firms in our sample are incorporated in the state of Delaware, which does not receive treatment in our setting. We drop Delaware-incorporated firms and show in Columns 3 and 4 that our inferences are unaffected.[26]

5.5. Clustering choices

In our primary analyses, we cluster standard errors by client firm based on Petersen (2009). This accounts for the fact that innovation often hinges upon firm-specific human capital as well as prior success in innovative efforts. This likely makes the regression error term autocorrelated within a firm over time. In Panel E of Table 8, we examine the sensitivity to alternative clustering choices. Columns 1 and 2 use industry clustering at the SIC three-digit level. We also conduct two-way clustering at the firm and industry-year level in Columns 3 and 4. Inferences hold. In untabulated analyses, we alternatively use firm and year clustering and firm and state-year clustering. These two-way clustering choices account for the fact that residuals could be correlated within a firm as well as across firms. Our inferences continue to hold.

5.6. Alternative measures of innovation

We check the robustness of the inferences to *scaled* measures of innovation used in prior literature. Acharya, Baghai, and Subramaniam (2013) adopt the number of employees to scale the

[26] In this sensitivity analysis, the coefficient estimates are smaller than the main results. This could be due to two reasons. First, we lose nearly 55% of the observations, leading to loss of power. Second, although Delaware does not receive treatment, firms incorporated in Delaware are often located in other states. As such, when we drop Delaware firms, many treated firms also drop out if they are located in a treatment state. Despite the loss of observations and power, it is reassuring to see that our inferences are not sensitive to dropping Delaware-incorporated firms.

patent and citation counts. We construct similar measures *log(Patent/Emp)* and *log(Cite/Emp)*, where we scale the total counts of patents and citations by the number of employees in millions and take the logarithm of (one plus) the scaled patents and citations as measures of innovation respectively. Columns 1 and 2 of *Panel F* of Table 6 re-run the main tests using these two measures and find that the inferences remain unaffected. Both patents and citations per million of employees decrease significantly when auditor litigation risk is higher. In untabulated analyses, we also use citations per patent as well as size-scaled measures (He and Tian 2013). Inferences continue to hold.

5.7. Recent sample period

In Panel G of Table 8, we test whether the inferences hold in a more recent sample period. Columns 1 and 2 conduct the main analyses for the sample period 1990–2005.[27] Inferences continue to hold. In columns 3 and 4, we use the sample period 1999–2005. Because we have data on auditor-engagement office and headquarters office from Audit Analytics for this period, we also use states of engagement office and headquarters (in addition to client incorporation and headquarters states) in our definition of *More_Aud_Liab*, which is consistent with the work of Anantharaman et al. (2016). As we observe, inferences are unaffected.

5.8. Difference in auditor engagement and client headquarters states

Our identification strategy in the main analyses assumes that auditors could be subject to legal liability for ordinary negligence in the state of client location and the state of client incorporation. We explore whether the effect of auditor conservatism on innovation differs for firms with auditors from the same state versus a different one. If auditor perception of litigation exposure differs for clients of

[27] We limit the sample to 2005 because the patent and citations data in Kogan et al. (2017) end in 2010. We require five years of data after 2005 because it may take several years for patents to accumulate citations.

the same state versus a different state, then we expect the effect of changes in auditor legal liability on firm innovation to be conditional on whether auditor engagement office is in the same state as client location (or incorporation).[28] Our sample period is 1999–2005, because we have auditor engagement office data in Audit Analytics starting from 1999. We construct an indicator variable *SameState* that equals one if the auditor engagement office is in the same state as the client headquarters or incorporation state and zero otherwise. We interact *More_Aud_Liab* with *SameState*. In Columns 5 and 6 of Table 8, Panel G, we do not see any differential effect of auditor legal liability shocks on innovation for firms with auditors from the same state.

5.9. Potential truncation bias

There may be a lag between a patent's application year and grant year (He and Tian 2013). This implies that patents applied for in the later years of the sample may still not have been granted. To tackle this concern, Figure 1a (1b) depicts the time-series variations of annual total patents (citations) for all firms in the CRSP-Compustat matched database during the 1950–2010 period. We observe a gradual decline in patent grants in the last few years of the database (see Figure 1a). However, the sample in our analysis ends in 1998, and it is highly likely that the outcomes of patent applications made in 1998 are public by 2010. As a result, truncation bias is unlikely to be a concern for the first measure of innovation, *logPatent*. The second truncation bias relates to the other measure of innovation, *logCite*. Patents tend to receive citations over many years after the grant year and in our sample, citations are observed only until 2010 at most. We check the gradual evolution of citations and patents over the years. When we focus on Figure 1b, it appears that the citation count experiences a sharp decline after the fiscal year 1998. However, the decline becomes significant only after 2000.

[28] The auditor engagement office is usually located in the client business location state. For example, the Audit Analytics database shows that 79% of firms incorporated and located in the United States have their auditor engagement offices in the same state as their headquarters during the period 1999–2017.

Thus truncation bias is unlikely to affect the estimates of the effect of auditor legal liability on innovation.

6. Conclusion

Audit services facilitate the efficient allocation of scarce resources in the economy by mitigating information asymmetry between the firm and investors. Conservative auditors are generally viewed as providing better audits and having greater independence (DeFond and Francis 2005). While both audit quality and independence matter for the efficient functioning of capital markets, we show that greater auditor conservatism can induce managerial myopia in the form of a reduction in corporate innovation. Our findings may inform regulators, as they contemplate the costs and benefits of increased auditor conservatism in framing policies that affect auditor incentives. Our study also adds to the real-effects literature by showing that auditors, besides directly affecting firms' reporting practices, may also harm the real operations of the clients they audit.

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Appendix A: Institutional Background on State-Level Auditor Liability

In the early 20th century, development in legal thoughts and a new understanding of social justice contributed to an expansion of legal liability of entities, such as railroads and factories, for injuries born out of negligence. For example, courts increasingly began to accept arguments that recognized damages for economic loss rather than mere physical loss (Baker and Prentice 2008).

Further, the rapid transformation of the United States from an agricultural to an industrial economy in the early and mid-20th century required entrepreneurs to raise external financing from stock exchanges. Because external financing is fraught with inherent perceptions of risk and asymmetric distribution of information, professional accounting began to evolve to provide "certifications" as to the reliability of the information in financial statements generated by businesses that wanted to raise capital from external investors (Baker and Prentice 2008). A problem with this arrangement was that, while investors and creditors increasingly relied on auditors for the soundness of company-furnished financial information, they lacked privity with the auditor. As such, their only legal resort against auditors' negligence of duty was litigation for fraud under federal securities law, which required the proof of intent to deceive. Subsequently, Judge Cardozo in *Ultramares Corp. v. Touché* ruled that auditors should be held liable for negligence to their clients *and any third party specifically identified as a user of the report*. Judge Cardozo's decision came at a time of fast development of legal thought on tort theories that entertained prevailing ideas of social justice, rather than strict adherence to the textual understanding of precedents (Baker and Prentice 2008). The precedent set by Judge Cardozo in *Ultramares Corp. v. Touché* remained the norm for most courts until the 1960s (MacKey 1993). However, Judge Cardozo's decision to enhance auditor legal liability beyond strict privity to a limited number of users of the report set the stage for subsequent expansion of auditor liability.

In a landmark verdict in 1958, (*Biakanja v. Irving*, 49 Cal. 2d 647, 320 P.2d 16, 320 P. 16 (1958)), the California Supreme Court expanded the liability of professional notary publics to include parties not just in privity but those affected by the notary public's certification (The Harvard Law Review Association 1958). While *Biakanja v. Irving* specifically applied to the notaries public, the verdict's principle was more generally applicable for determining whether the defendant owed a duty to the plaintiff (Wiener 1983). As a result, *Biakanja v. Irving* was a defining moment in the expansion of legal liability for professionals in California and signaled a new regime of legal liability for auditors. Importantly, the debate on auditors' duty of care increasingly recognized the public role that accountants' certification of businesses' financial information plays (Baker and Prentice 2008). This recognition partly resulted from the expanding size of the professional accounting firms, the growth of large investment funds, and the development of legal thoughts on tort liability.

Owing to these developments, many state courts moved to an expansive regime of auditor legal liability beginning in the early 1970s.[29] In 1983, the New Jersey Supreme Court rejected both the privity and the restatement approaches, ruling that auditors could be held liable for ordinary negligence to any "reasonably foreseen" party as recipients of the statements for routine business purposes. In the same year (1983), the Wisconsin Supreme Court also favored the foreseeability approach. Continuing this trend, the U.S. Supreme court expanded auditor legal liability by supporting a *foreseeability* approach.[30] In 1992, in a dramatic shift of auditors' litigation-risk exposure, the California Supreme Court reversed a decision by the Court of Appeals and limited auditors' liability by favoring the restatement approach, thereby reducing the auditors' expected litigation costs and litigation risks significantly. In 1995, the New Jersey legislature passed the

[29] Rhode Island was the first state to expand auditor's legal liability formally in 1968 (*Rusch Factors, Inc. v. Levin*, 284 F. Supp. 85, D.R.I. 1968). We do not use this precedent, because Compustat data on R&D expenditures are not available for the sample of firms used in the paper before 1970.
[30] *United States v. Arthur Young and Co.*, 465 U.S. 805, 104 S. Ct. 1495, 79 L. Ed. 2d 826 (1984).

Accountant Liability Act, which limited the auditors' liability as per the restatement approach. Judicial decisions on auditor liability to third parties under common law often attempt to balance auditors' public role as certifiers of businesses' financial information and the indefinite liability that auditors could be subject to under an expansive liability regime.

To understand our identification strategy, two points are worth mentioning. First, auditors face legal liability under both federal securities law and common law. However, federal law covers only fraud or gross negligence, whereas ordinary negligence falls under common law liability governed by state courts. Second, the identification in our study depends on the concept of *stare decisis* or legal precedent. It refers to the policy of courts in common law to adhere to principles established by decisions in earlier cases. Under this principle, a precedent set by other courts can be either binding or persuasive. In general, decisions of a court will be a mandatory authority for any court lower in the hierarchy. Thus California Supreme Court decisions are binding for all California Court of Appeals (intermediate court) and Trial Courts (lower court), when a matter pertains to a case previously decided by the California Supreme Court. Similarly, intermediate court decisions are binding upon all trial courts.

Appendix B: Auditor Legal Liability Shocks (* Indicates Negative Shock)

State	Change Year	Court Rulings
TX	1971	*Shatterproof Glass Corporation v. James, 466 S.W.2d 873 (Tex. Civ. App. 1971).*
ND	1974	*Bunge Corporation v. Eide*, 372 F. Supp. 1058 (D.N.D. 1974).
MN	1976	*Bonhiver v. Graff*, 248 N.W.2d 291, 311 Minn. 111 (1976).
PA	1978	*Sharp v. Coopers and Lybrand*, 457 F. Supp. 879 (E.D. Pa. 1978).
NE	1979	*Seedkem, Inc. v. Safranek*, 466 F. Supp. 340 (D. Neb. 1979).
KY	1981	*Ingram Industries, Inc. v. Nowicki*, 527 F. Supp. 683 (E.D. Ky. 1981).
NH	1982	*Spherex, Inc. v. Alexander Grant and Co.*, 122 N.H. 898, 451 A.2d 1308 (1982).
OH	1982	*Haddon View Inv. Co. v. C. and L.*, 70 Ohio St. 2d 154, 436 N.E.2d 212, 24 O.O.3d 268 (1982).
HI	1983	*Matter of Hawaii Corp.*, 567 F. Supp. 609 (D. Haw. 1983).
NJ	1983	*Rosenblum v. Adler*, 461 A.2d 138, 93 N.J. 324, 93 N.H. 324 (1983).
WI	1983	*Citizens State Bank v. Timm, Schmidt and Co.*, 335 N.W.2d 361, 113 Wis. 2d 376, 113 Wis. 361 (1983).
MS	1987	*Touche Ross and Co. v. Commercial Union Ins. Co.*, 514 So. 2d 315 (Miss. 1987).
GA	1987	*Badische Corp. v. Caylor*, 356 S.E.2d 198, 257 Ga. 131 (1987).
NC	1988	*Raritan River Steel v. Cherry, Bekaert and Holland*, 367 S.E.2d 609, 322 N.C. 200 (1988).
MI	1989	*Law Office of Stockler v. Rose*, 436 N.W.2d 70, 174 Mich. App. 14 (Ct. App. 1989).
WV	1989	*First Nat. Bank of Bluefield v. Crawford*, 386 S.E.2d 310 (W. Va. 1989).
LA	1990	*First Nat. Bank of Commerce v. Monco Agency Inc.*, 911 F.2d 1053 (5th Cir. 1990).
FL	1990	*First Fla. Bank, NA v. Max Mitchell and Co.*, 558 So. 2d 9 (Fla. 1990).
MT	1990	*Thayer v. Hicks*, 793 P.2d 784, 243 Mont. 138 (1990).
TN	1991	*Bethlehem Steel Corp. v. Ernst and Whinney*, 822 S.W.2d 592 (Tenn. 1991).
CA*	1992	*Bily v. Arthur Young and Co.*, 834 P.2d 745, 3 Cal. 4th 370, 11 Cal. Rptr. 2d 51 (1992).
MO	1993	*MidAmerican Bank and Trust Co. v. Harrison*, 851 S.W.2d 563 (Mo. Ct. App. 1993).
AL	1994	*Boykin v. Arthur Andersen and Co.*, 639 So. 2d 504 (Ala. 1994).
NJ*	1994	*Accountant Liability Act (Effective from March, 1995)*

Appendix C: Variable Definitions

Variable	Definition
Age	The logarithm of the number of years a firm in the Compustat database
BigN	Indicator variable that takes one for firms audited by big audit firms, zero otherwise
CAPEX/Assets	Capital expenditures scaled by the book value of assets
HHI	Herfindahl Index, measured at the SIC three-digit for sales
HHISquared	HHI squared
High_Analyst_Pressure	Indicator variable that takes one if analyst coverage for a firm is greater than the industry-year median, zero otherwise
High_Debt_Pressure	Indicator variable that takes one if the total amount of outstanding bank loans (scaled by book assets) in a given fiscal year is greater than the (SIC three-digit) industry-year median, zero otherwise
High_STInstOwn	Indicator variable that takes one for firm-years that have transient institutional ownership greater than the industry-year median, zero otherwise
Illiquidity	The annual average of monthly bid-ask spreads scaled by lagged stock price
KS_High	Indicator variable that takes one if Kim and Skinner (2012)'s litigation risk measure is greater than the industry-year median, zero otherwise
Less_Aud_Liab	Indicator variable that equals one after states decrease auditor legal liability, zero otherwise.
Leverage	Total short- and long-term debt scaled by the book value of assets
logCite	Log(1+CitationCount), where *CitationCount* equals the number of future (nonself) citations attributed to patents filed for in a fiscal year
logPatent	Log(1+PatentCount), where *PatentCount* equals the number of patents filed for (and are eventually granted) in a fiscal year
MKBK	The market value of equity and debt scaled by the book value of assets
ModGC	Indicator variable equaling one if the auditor issues modified going-concern opinion, zero otherwise
More_Aud_Liab	Indicator variable that goes from zero to one when (and after) a firm's location or incorporation state increases auditor legal liability and goes from one to zero when (and after) a firm's location or incorporation state decreases auditor legal liability. It remains zero for the states that do not change auditor legal liability during the sample period.
PPE/Assets	Property, plant, and equipment, scaled by the book value of assets
R&D/Assets	R&D expenditures scaled by the book value of assets, set to zero if missing
Restate	An indicator variable that equals one if the fiscal year-end financial statements are restated, zero otherwise

ROA	Income before extraordinary items, scaled by the beginning book value of assets
Size	The logarithm of the book value of assets
SmallProfit	Indicator variable that takes one if a firm's ROA is between 0 and 0.03 dollar, zero otherwise
Type-I Error	Indicator variable equaling one if the auditor issues a modified going-concern opinion and the firm does not go bankrupt within the next year, zero otherwise
Type-II Error	Indicator variable equaling one if the auditor does not issue a modified going-concern opinion and the firm goes bankrupt within the next year, zero otherwise

Figure 1a: Total Annual Patent Counts for All Compustat-CRSP Matched Firms

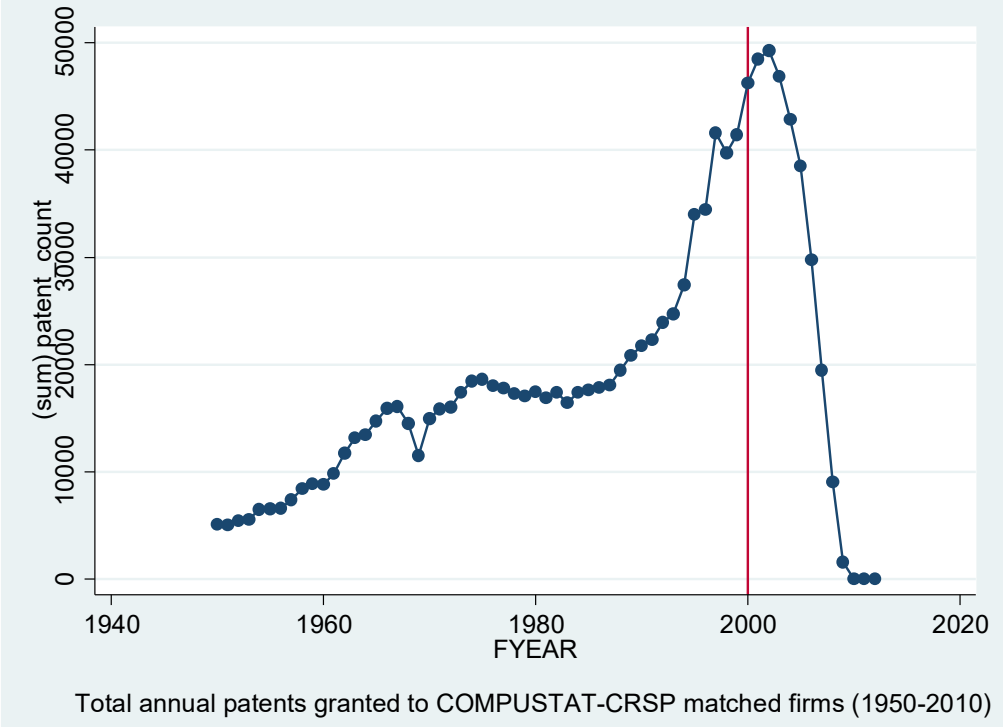

Figure 1b: Total Annual Citation Counts for All Compustat-CRSP Matched Firms

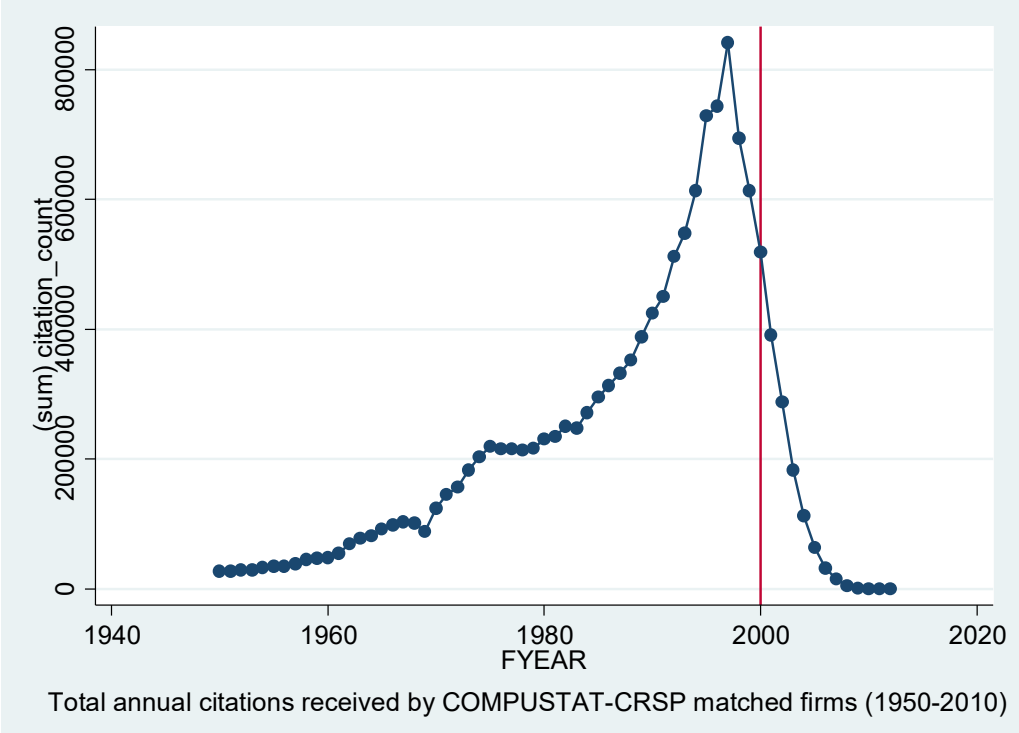

Table 1: Descriptive Statistics

The table reports descriptive statistics. *Panel A* reports summary statistics for the main variables used in the study. The sample period is 1988–1998 for *ModGC*, 1999–2017 for *Restate*, and 1970–1998 for all other variables. *Panel B* reports the number of unique firms in each treatment state. See Appendix C for variable definitions.

Panel A: Summary Statistics for Key Variables

Variables	N	Mean	Std. Dev.	Median
More_Aud_Liab	63,976	0.340	0.474	0.000
R&D/Assets	63,976	0.033	0.067	0.000
PatentCount	63,976	5.679	20.971	0.000
CitationCount	63,976	79.227	278.003	0.000
ModGC	31,442	0.266	0.442	0.000
SmallProfit	63,976	0.15	0.35	0
Restate	53,529	0.11	0.32	0
TAC	63,976	-0.032	0.168	-0.036
Size	63,976	4.509	1.950	4.346
Age	63,976	2.489	0.804	2.565
MKBK	63,976	1.471	1.425	0.993
Leverage	63,976	0.234	0.188	0.213
CAPEX/Assets	63,976	0.075	0.069	0.056
PPE/Assets	63,976	0.315	0.206	0.274
HHI	63,976	0.209	0.146	0.169
ROA	63,976	0.006	0.187	0.047
Illiquidity	63,976	0.172	0.078	0.157

Panel B: Unique Firms for Treatment States

(1) Treatment State	(2) Unique Firms	(3) Treatment State	(4) Unique Firms
TX	263	MS	3
ND	0	NC	51
MN	48	MI	79
PA	107	WV	4
NE	5	FL	140
KY	11	MT	1
OH	121	LA	18
NH	8	TN	33
HI	6	CA	511
NJ	96	MO	55
WI	33	AL	21
GA	73		

Table 2: Validation of State-Level Auditor Conservatism Measure

The table reports estimates for the effect of an increase in auditor legal liability on auditor conservatism proxies. The sample period is 1988–1998 in Columns 1–3, 1999–2017 in Column 4, and 1970–1998 in Column 5–8. *ModGC* equals one if a firm receives a modified opinion in a given year, zero otherwise. Type-I (Type-II) Error equals one if the auditor issues (does not issue) a modified going-concern opinion and the firm does not go bankrupt (goes bankrupt) within the next year, zero otherwise. *Restate* equals one if the fiscal year-end financial statements are restated, zero otherwise. *SmallProfit* takes one if a firm's ROA is between 0 and 0.03 dollar, zero otherwise. *TAC* is total accruals scaled by book assets. |TACpos| (|TACneg|) is the absolute value of positive TAC (negative TAC). All specifications use the OLS regression. Test statistics (two-sided) based on robust standard errors clustered at the firm level are reported in parentheses. Asterisks ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level respectively. Appendix B details all shocks to auditor legal liability and Appendix C provides variable definitions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	ModGC	Type-I Error	Type-II Error	Restate	SmallProfit	TAC	|TACpos|	|TACneg|
More_Aud_Liab	***0.056******	***0.056******	***-0.000***	***-0.018******	***-0.009****	***-0.001***	***-0.003****	***0.002***
	(3.39)	***(3.45)***	***(-0.03)***	***(-4.07)***	***(-1.93)***	***(-0.56)***	***(-1.77)***	***(0.75)***
Size	-0.004	-0.004	-0.001	-0.000***	0.029***	0.006***	-0.001	-0.004**
	(-0.38)	(-0.42)	(-1.24)	(-6.42)	(10.45)	(4.08)	(-0.81)	(-2.03)
Age	-0.036	-0.038*	0.000	-0.011***	-0.021***	-0.058***	-0.078***	-0.003
	(-1.64)	(-1.72)	(0.12)	(-3.48)	(-3.58)	(-16.34)	(-23.58)	(-1.17)
MKBK	-0.005	-0.005	0.000	-0.001	-0.020***	0.003***	0.007***	0.006***
	(-1.48)	(-1.51)	(1.31)	(-1.14)	(-12.50)	(4.69)	(7.14)	(4.26)
Leverage	0.210***	0.205***	0.012***	0.023**	0.209***	0.027*	0.104***	0.014
	(5.73)	(5.68)	(2.66)	(2.32)	(17.13)	(1.82)	(8.37)	(1.50)
CAPEX/Assets	-0.091	-0.087	0.013*	0.047	-0.269***	0.004	0.173***	0.096***
	(-1.12)	(-1.06)	(1.84)	(1.04)	(-8.80)	(0.18)	(4.74)	(7.72)
PPE/Assets	0.052	0.057	-0.003	-0.011	0.087***	-0.164***	-0.242***	-0.022***
	(0.91)	(1.00)	(-0.71)	(-0.68)	(4.81)	(-11.00)	(-11.00)	(-2.78)
HHI	-0.152	-0.161	-0.007	-0.031	0.065	0.076***	0.046	-0.060**
	(-0.98)	(-1.03)	(-0.65)	(-0.70)	(1.24)	(3.04)	(1.65)	(-2.15)
HHI2	0.259	0.267	0.016	0.043	-0.076	-0.099***	-0.061	0.085**
	(1.24)	(1.28)	(0.74)	(0.91)	(-1.13)	(-2.79)	(-1.47)	(2.21)
ROA	-0.189***	-0.183***	-0.002**	-0.005	-0.120***	0.284***	0.163***	-0.159***
	(-7.22)	(-7.02)	(-2.02)	(-0.50)	(-10.69)	(38.15)	(22.83)	(-16.84)
Illiquidity	0.229***	0.206***	0.002	0.001	-0.243***	-0.137***	0.052***	0.142***
	(3.31)	(3.00)	(0.32)	(0.81)	(-8.59)	(-11.34)	(3.56)	(12.84)
Observations	31,442	31,442	31,442	53,529	63,976	63,976	21,237	42,739
Adjusted R^2	0.295	0.293	0.041	0.037	0.121	0.350	0.420	0.442
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Ind, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year

Table 3: The Effect of Auditor Conservatism on R&D Expenditures

The table reports the regression results for the effect of an increase in auditor conservatism on client investments in R&D. *More_Aud_Liab* captures an increase in auditor conservatism (see Table 2). The sample period is 1970–1998. R&D/Assets is the client's annual R&D expenditures scaled by the book value of assets. All specifications use the OLS regression. Test statistics (two-sided) based on robust standard errors clustered at the firm level are in parentheses. Asterisks ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level respectively. Appendix B details all shocks to auditor legal liability and Appendix C provides variable definitions.

Variables	(1) R&D/Assets	(2) R&D/Assets	(3) R&D/Assets
More_Aud_Liab	***-0.002******	***-0.002******	***-0.002******
	(-3.57)	***(-3.53)***	***(-3.19)***
R&D Missing Dummy		-0.027***	0.015***
		(-21.29)	(11.07)
Size	-0.006***	-0.006***	-0.007***
	(-9.90)	(-11.30)	(-11.32)
Age	0.009***	0.009***	0.009***
	(8.65)	(8.96)	(8.70)
MKBK	0.003***	0.003***	0.003***
	(6.24)	(6.25)	(6.00)
Leverage	-0.008***	-0.007***	-0.007***
	(-4.32)	(-4.14)	(-3.89)
CAPEX/Assets	0.018***	0.018***	0.017***
	(5.48)	(5.49)	(5.01)
PPE/Assets	0.023***	0.023***	0.024***
	(8.30)	(8.54)	(8.54)
HHI	-0.005	-0.001	0.003
	(-0.64)	(-0.14)	(0.46)
HHI2	0.003	-0.003	-0.009
	(0.35)	(-0.34)	(-1.02)
ROA	-0.044***	-0.042***	-0.043***
	(-15.19)	(-14.88)	(-14.72)
Illiquidity	0.008*	0.007*	0.008*
	(1.92)	(1.83)	(1.92)
Observations	63,976	63,976	63,976
Adjusted R^2	0.829	0.836	0.823
Fixed Effects	Firm, Year	Firm, Year	Firm, Year
Missing R&D Replaced With	Zero	Zero	Industry Mean

Table 4: Main Results – The Effects of Auditor Conservatism on Corporate Innovation

The table reports the main results for the effect of an increase in auditor conservatism on client innovation. *More_Aud_Liab* captures an increase in auditor conservatism (see Table 2). The sample period is 1970–1998. *logPatent* equals one plus the number of patents a firm receives in a fiscal year. *logCite* equals one plus the number of all future (nonself) citations attributed to patents a firm receives in a fiscal year. All specifications use the OLS regression. Test statistics (two-sided) based on robust standard errors clustered at the firm level are in parentheses. Asterisks ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level respectively. Appendix B details all shocks to auditor legal liability and Appendix C provides variable definitions.

Variables	(1) logPatent_{t+1}	(2) logCite_{t+1}	(3) logPatent_{t+2}	(4) logCite_{t+2}
More_Aud_Liab	***-0.069******	***-0.102******	***-0.064******	***-0.093******
	(-4.37)	***(-3.58)***	***(-3.99)***	***(-3.24)***
Size	0.166***	0.283***	0.164***	0.271***
	(13.15)	(12.97)	(12.10)	(11.51)
Age	0.063***	0.155***	0.061***	0.145***
	(3.36)	(4.46)	(3.02)	(3.89)
MKBK	0.020***	0.033***	0.020***	0.036***
	(4.71)	(3.85)	(4.60)	(3.89)
Leverage	-0.169***	-0.326***	-0.187***	-0.353***
	(-5.07)	(-4.91)	(-5.16)	(-4.89)
CAPEX/Assets	-0.035	0.017	0.023	0.127
	(-0.76)	(0.17)	(0.46)	(1.16)
PPE/Assets	0.097**	0.144	0.070	0.086
	(2.14)	(1.60)	(1.44)	(0.89)
HHI	0.061	0.272	0.052	0.219
	(0.36)	(0.83)	(0.28)	(0.62)
HHI2	-0.066	-0.368	-0.048	-0.360
	(-0.30)	(-0.85)	(-0.20)	(-0.77)
ROA	-0.060***	-0.108**	-0.025	-0.070
	(-2.77)	(-2.20)	(-1.01)	(-1.26)
Illiquidity	-0.109**	-0.215**	-0.050	-0.119
	(-2.23)	(-2.02)	(-0.93)	(-1.00)
Observations	63,976	63,976	57,039	57,039
Adjusted R^2	0.854	0.782	0.859	0.788
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year

Table 5: Limiting the Sample to the Treatment States Only

The table reports results by limiting the sample to firms in the treatment states only. *More_Aud_Liab* equals one for firms located or incorporated in states under higher auditor legal liability regime and zero for low auditor legal liability regime. *ModGC* equals one if a firm receives a modified opinion in a given year, zero otherwise. Type-I (Type-II) Error equals one if the auditor issues (does not issue) a modified going-concern opinion and the firm does not go bankrupt (goes bankrupt) within the next year, zero otherwise. *SmallProfit* takes one if a firm's ROA is between 0 and 0.03 dollar, zero otherwise. *TAC* is total accruals scaled by book assets. |TACpos| (|TACneg|) is the absolute value of positive TAC (negative TAC). R&D/Assets is the client's annual R&D expenditures scaled by the book value of assets. *logPatent* equals one plus the number of patents a firm receives in a fiscal year. *logCite equals* one plus the number of future (nonself) citations attributed to patents a firm receives in a fiscal year. The sample period is 1988–1998 in Columns 1–3 and 1970–1998 in Columns 4–10. All specifications use the OLS regression. Test statistics (two-sided) based on robust standard errors clustered at the firm level are in parentheses. Asterisks ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level respectively. Appendix B details all shocks to auditor legal liability and Appendix C provides variable definitions.

	Validation Test: State-level Auditor Conservatism Measure							R&D and Innovation		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables	ModGC	Type-I Error	Type-II Error	SmallProfit	TAC	|TACpos|	|TACneg|	R&D/Assets	logPatent_{t+1}	logCite_{t+1}
More_Aud_Liab	***0.050******	***0.050******	***0.001***	***-0.010****	***-0.001***	***-0.004****	***0.002***	***-0.002******	***-0.074******	***-0.109******
	(3.45)	***(3.26)***	***(1.27)***	***(-2.10)***	***(-0.74)***	***(-1.91)***	***(0.83)***	***(-3.21)***	***(-4.60)***	***(-3.69)***
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,089	16,089	16,089	35,836	35,836	11,755	24,081	35,836	35,836	35,836
Adjusted R-sq.	0.294	0.290	0.053	0.128	0.261	0.354	0.288	0.828	0.859	0.789
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year

Table 6: Evolution of Treatment Effects

The table examines the evolution of the treatment effect. The sample period is 1988–1998 in Columns 1–3 and 1970–1998 in Columns 4–10, excluding CA (NJ) firms after 1992 (1995). *More_Aud_Liab (t=-2) and More_Aud_Liab (t=-1)* are indicator variables that equal one if a firm's state will increase auditor legal liability in two years or one year, respectively, zero otherwise. *More_Aud_Liab (t=1)*, *More_Aud_Liab (t=2)*, *More_Aud_Liab (t=3)*, and *More_Aud_Liab (t>=4)* are indicator variables that equal one if a firm's state increased auditor legal liability in the current year, one year ago, two years ago, and three or more years ago, respectively, zero otherwise. *ModGC* equals one if a firm receives a modified opinion in a given year, zero otherwise. Type-I (Type-II) Error equals one if the auditor issues (does not issue) a modified going-concern opinion and the firm does not go bankrupt (goes bankrupt) within the next year, zero otherwise. *SmallProfit* takes one if a firm's ROA is between 0 and 0.03 dollar, zero otherwise. *TAC* is total accruals scaled by book assets. |TACpos| (|TACneg|) is the absolute value of positive TAC (negative TAC). R&D/Assets is research and development expenditures scaled by book assets. *logPatent* equals one plus the number of patents a firm receives in a fiscal year. *logCite equals* one plus the number of future (nonself) citations attributed to patents a firm receives in a fiscal year. Test statistics (two-sided) based on robust standard errors clustered at the firm level are reported in parentheses. Asterisks ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level respectively.

	Validation Test: State-level Auditor Conservatism Measure							R&D and Innovation		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables	ModGC	Type-I Error	Type-II Error	SmallProfit	TAC	|TACpos|	|TACneg|	R&D/Assets	$logPatent_{t+1}$	$logCite_{t+1}$
More_Aud_Liab (t=-2)	*-0.008*	*-0.006*	*0.001*	*-0.003*	*-0.000*	-0.004	-0.005	*-0.001*	*0.006*	*0.035*
	(-0.42)	*(-0.31)*	*(0.59)*	*(-0.32)*	*(-0.14)*	(-1.18)	(-1.39)	*(-0.88)*	*(0.38)*	*(0.95)*
More_Aud_Liab (t=-1)	*0.019*	*0.019*	*0.001*	*-0.008*	*-0.002*	-0.000	-0.001	*0.001*	*0.004*	*-0.008*
	(0.89)	*(0.88)*	*(1.23)*	*(-0.80)*	*(-0.62)*	(-0.03)	(-0.37)	*(0.81)*	*(0.27)*	*(-0.22)*
More_Aud_Liab (t=1)	*0.048***	*0.050***	*0.000*	*-0.019**	*-0.010****	-0.008**	-0.006	*-0.002****	*-0.019*	*-0.017*
	(2.16)	*(2.27)*	*(0.02)*	*(-1.91)*	*(-2.64)*	(-2.10)	(-1.49)	*(-3.69)*	*(-1.07)*	*(-0.42)*
More_Aud_Liab (t=2)	*0.060****	*0.060****	*-0.000*	*-0.020***	*-0.009***	-0.007*	0.003	*-0.002**	*-0.051****	*-0.059*
	(2.68)	*(2.70)*	*(-0.28)*	*(-1.99)*	*(-2.26)*	(-1.66)	(0.63)	*(-2.00)*	*(-2.61)*	*(-1.46)*
More_Aud_Liab (t=3)	*0.055***	*0.060***	*-0.001**	*-0.013*	*-0.004*	-0.006	-0.003	*-0.003****	*-0.059****	*-0.084**
	(2.38)	*(2.57)*	*(-1.75)*	*(-1.24)*	*(-0.97)*	(-1.47)	(-0.76)	*(-3.86)*	*(-2.89)*	*(-1.94)*
More_Aud_Liab (t>=4)	*0.045***	*0.046***	*0.001*	*-0.008*	*-0.002*	-0.002	-0.001	*-0.003****	*-0.080****	*-0.119****
	(2.16)	*(2.21)*	*(1.12)*	*(-1.57)*	*(-0.87)*	(-1.02)	(-0.28)	*(-5.07)*	*(-3.99)*	*(-3.30)*
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	26,338	26,338	26,338	58,864	58,864	19602	39262	58,864	58,864	58,864
Adjusted R^2	0.295	0.293	0.041	0.121	0.353	0.182	0.442	0.829	0.854	0.782

Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year

Table 7: Cross-Sectional Analyses

The table reports the cross-sectional variations in the effect of auditor conservatism on corporate innovation. *More_Aud_Liab* captures an increase in auditor conservatism (see Table 2) and equals one for firms located or incorporated in states under higher auditor legal liability regime, zero for low auditor legal liability regime. *logPatent* equals one plus the number of patents a firm receives in a fiscal year. *logCite* equals one plus the number of all future (nonself) citations attributed to patents a firm receives in a fiscal year. In Panel A, the sample period is 1976–1998 in Columns 1 and 2, 1980–1998 in Columns 3 and 4, and 1982–1998 in Columns 5 and 6. *High_Analyst_Pressure* is an indicator variable that takes one if analyst coverage for a firm is greater than the industry-year median, zero otherwise. *High_STInstOwn* takes one for firm-years that have transient institutional ownership greater than the industry-year median, zero otherwise. *High_Debt_Pressure* takes on one if the total amount of outstanding bank loans in a given fiscal year is greater than the (SIC three-digit) industry-year median, zero otherwise. In Panel B, the sample period is 1970–1998 in Columns 1 and 2 and 1974–1998 in Columns 3 and 4. *KS_High* takes one if Kim and Skinner (2012)'s litigation risk measure is greater than the industry-year median, zero otherwise. *BigN* takes one for firms audited by big audit firms, zero otherwise. All specifications are estimated using OLS regression. Test statistics (two-sided) based on robust standard errors clustered at the firm level are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level respectively. Appendix B details all shocks to auditor legal liability and Appendix C provides variable definitions.

Panel A: Cross-sectional Analyses Based on Capital Market Pressure

Variables	(1) logPatent_{t+1}	(2) logCite_{t+1}	(3) logPatent_{t+1}	(4) logCite_{t+1}	(5) logPatent_{t+1}	(6) logCite_{t+1}
More_Aud_Liab ×	*-0.0610***	*-0.103***				
High_Analyst_Pressure	*(-2.160)*	*(-1.964)*				
More_Aud_Liab ×			*-0.0522***	*-0.0990***		
High_STInstOwn			*(-2.362)*	*(-2.393)*		
More_Aud_Liab ×					*-0.0817***	*-0.0999****
High_Debt_Pressure					*(-2.369)*	*(-2.760)*
More_Aud_Liab	-0.0586***	-0.0937***	-0.0616***	-0.0896***	-0.0761***	-0.131***
	(-3.833)	(-3.122)	(-3.878)	(-2.710)	(-3.774)	(-3.524)
High_Analyst_Pressure	0.117***	0.190***				
	(5.927)	(4.941)				
High_STInstOwn			0.0138	0.0640**		
			(1.097)	(2.331)		
High_Debt_Pressure					0.0998***	0.141***
					(3.245)	(3.555)
Observations	56,247	56,247	49,582	49,582	45,792	45,792

Adjusted R^2	0.854	0.778	0.855	0.778	0.866	0.793
Controls, Firm & Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Cross-sectional Analyses Based on Litigation Exposure and BigN Clients

Variables	(1) $logPatent_{t+1}$	(2) $logCite_{t+1}$	(3) $logPatent_{t+1}$	(4) $logCite_{t+1}$
More_Aud_Liab × KS_High	*-0.0197***	*-0.0358**		
	(-2.356)	*(-1.703)*		
More_Aud_Liab × BigN			*-0.0586****	*-0.0974***
			(-2.687)	*(-2.026)*
More_Aud_Liab	-0.0620**	-0.100***	-0.0354*	-0.0476
	(-2.630)	(-2.820)	(-1.672)	(-1.020)
KS_High	0.0201***	0.0256**		
	(6.667)	(2.413)		
BigN			-0.0159	-0.0512
			(-0.724)	(-1.042)
Observations	63,976	63,976	59,226	59,226
Adjusted R^2	0.856	0.782	0.852	0.780
Controls, Firm & Year FE	Yes	Yes	Yes	Yes

Table 8: Additional Analyses and Robustness Tests

The table reports the results from robustness tests. *More_Aud_Liab* captures an increase in auditor conservatism (see Table 2) and equals one for firms located or incorporated in states under higher auditor legal liability regime, zero for low auditor legal liability regime. *logPatent* equals one plus the number of patents a firm receives in a fiscal year. *logCite* equals one plus the number of all future (nonself) citations attributed to patents a firm receives in a fiscal year. *Panel A* reports estimates for the effect of a decrease in auditor legal liability *(Less_Aud_Liab)* on innovation. The sample period is 1989–1998. Firms in states that increase auditor legal liability are dropped. In Panel B, the sample is restricted to a neighboring-state matched sample. Each treatment state is matched with a neighboring control state and six years pre- and post-shock firm-year observations are used in the analyses. The sample period is 1970–1998. Panel C examines the effect of an increase in auditor conservatism on auditor turnover in Columns 1 and 2 and tests the robustness of the main results after controlling for auditor turnover in Columns 3 and 4. *Switch (To_NonBigN)* equals one if a firm changes its auditor of the previous year to a different (from BigN to NonBigN) auditor. The sample period is 1974–1998. Panel D, Columns 1 and 2 of define *More_Aud_Liab* at the state of headquarters only under the assumption that most of the auditing process occurs in the client state of headquarters. Panel D, Columns 3 and 4 drop firms incorporated in Delaware. Panel E tests the sensitivity of the main results to alternative clustering choices. Panel F scales innovation measures by the number of employees (in millions). Panel G examines the treatment effect in the more recent sample period. The sample period is 1990–2005 in Columns 1 and 2 and 1999–2005 in Columns 3 and 4. All specifications are estimated using the OLS regression. Test statistics (two-sided) based on robust standard errors clustered at the firm level are in parentheses. Asterisks ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level respectively. Appendix B details all shocks to auditor legal liability and Appendix C provides variable definitions.

Panel A: The Effect of a Decrease in Auditor Legal Liability on Innovation

Variables	(1) $logPatent_{t+1}$	(2) $logCite_{t+1}$
Less_Aud_Liab	*0.131***	*0.220***
	(4.19)	*(3.43)*
Observations	19,703	19,703
Adjusted R^2	0.863	0.790
Controls	Yes	Yes
Fixed Effects	Firm, Year	Firm, Year

Panel B: Neighboring-State Matched Sample

Variables	(1) $logPatent_{t+1}$	(2) $logCite_{t+1}$
More_Aud_Liab	***-0.0852*****	***-0.129*****
	(-5.263)	***(-4.183)***
Other Controls	Yes	Yes
Observations	32,446	32,446
Adjusted R-squared	0.877	0.806
Fixed Effects	Firm, Year	Firm, Year

Panel C: Accounting for Auditor Turnover

Variables	(1) Switch	(2) To_NonBigN	(3) $logPatent_{t+1}$	(4) $logCite_{t+1}$
More_Aud_Liab	*0.000*	*-0.001*	*-0.0718****	*-0.1166****
	(0.12)	*(-0.47)*	*(-4.636)*	*(-3.999)*
Switch			-0.0066	-0.0141
			(-1.088)	(-0.905)
Other Controls	Yes	Yes	Yes	Yes
Observations	59,226	59,226	59,226	59,226
Adjusted R-squared	0.053	0.030	0.853	0.778
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year

Panel D: Sensitivity to Auditor Legal Liability Jurisdiction and Delaware-incorporated Firms

Variables	(1) $logPatent_{t+1}$	(2) $logCite_{t+1}$	(1) $logPatent_{t+1}$	(2) $logCite_{t+1}$
More_Aud_Liab	*-0.100****	*-0.141****	*-0.050***	*-0.055***
	(-5.46)	*(-4.36)*	*(-2.32)*	*(-2.47)*
Observations	63,976	63,976	28,470	28,470
Adjusted R^2	0.854	0.782	0.851	0.773
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year

Panel E: Sensitivity to Clustering Choices

	Industry (SIC3) Clustering		Firm and Industry-Year Clustering	
Variables	(1) $logPatent_{t+1}$	(2) $logCite_{t+1}$	(3) $logPatent_{t+1}$	(4) $logCite_{t+1}$
More_Aud_Liab	*-0.069****	*-0.102***	*-0.069****	*-0.102****
	(-2.63)	*(-2.57)*	*(-4.33)*	*(-3.62)*
Observations	63,976	63,976	63,976	63,976
Adjusted R^2	0.854	0.782	0.851	0.773
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year

Panel F: Alternative Measures of Innovation

Variables	(1) $\log(\frac{\text{Patent}}{\text{Emp}})_{t+1}$	(2) $\log(\frac{\text{Cite}}{\text{Emp}})_{t+1}$
More_Aud_Liab	*-0.051***	*-0.102***
	(-4.02)	*(-6.99)*
Observations	63,976	63,976
Adjusted R^2	0.651	0.648
Controls	Yes	Yes
Fixed Effects	Firm, Year	Firm, Year

Panel G: Recent Sample Period

	Sample Period: 1990-2005		Sample Period: 1999-2005			
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	logPatent_{t+1}	logCite_{t+1}	logPatent_{t+1}	logCite_{t+1}	logPatent_{t+1}	logCite_{t+1}
More_Aud_Liab	*-0.122***	*-0.137***	*-0.182***	*-0.226***	*-0.172***	*-0.215***
	(-4.40)	*(-2.60)*	*(-3.79)*	*(-3.32)*	*(-2.42)*	*(-2.07)*
Same_State					0.037	0.081
					(0.78)	(1.19)
More_Aud_Liab × *Same_State*					-0.002	0.011
					(-0.02)	(0.09)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	40,298	40,298	12,578	12,578	12,578	12,578
Adjusted R^2	0.863	0.787	0.480	0.440	0.480	0.441
Fixed Effects	Firm, Year	Firm, Year	Ind, Year	Ind, Year	Ind, Year	Ind, Year