

Does Litigation Encourage or Deter Real Earnings Management?*

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Abstract

We examine whether litigation risk encourages or deters real earnings management (REM). On the one hand, the literature argues that litigation risk restricts opportunism in voluntary disclosure and financial reporting choices. This can encourage REM, as managers seek to substitute strategic disclosure and reporting with more opaque and difficult-to-detect earnings management via real actions. On the other hand, REM's more negative implications for future stock price and operating performance documented in the literature suggest that shareholders should be motivated to identify and litigate instances of REM upon observing negative outcomes. This ex post settling-up opportunity for shareholders can ex ante deter managers from engaging in REM. We conduct difference-in-difference tests centered on an unanticipated court ruling that reduced shareholders' ability to initiate class action lawsuits against firms headquartered in the Ninth Circuit. We observe significant increases in REM following the ruling for Ninth Circuit firms relative to other firms. Further, these increases are more pronounced when managers are more entrenched and when firms have lower institutional ownership. We conclude that the threat of litigation deters REM, especially when other mechanisms such as corporate governance and monitoring are weaker.

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1. Introduction

Our goal is to examine whether litigation risk encourages or deters real earnings management. Security class action litigation enables shareholders to sue a firm for issuing misleading financial reports and/or disclosures (Skinner 1994, Francis Philbrick and Schipper 1994). The threat of litigation can, in turn, discipline managers' voluntary disclosure practices as well as financial reporting choices.¹ Importantly, managers can attempt to mislead investors not just via guidance and reporting choices but also by altering underlying real transactions (Bruns and Merchant 1990; Graham, Harvey and Rajgopal 2005). In fact, executives often prefer this method of managing earnings because it is more opaque and thus more difficult for investors to detect and litigate (Kothari, Mizik and Roychowdhury 2016). A concern in this context is that higher litigation risk can motivate managers to simply substitute earnings management via real activities for that via accruals (see Cohen Dey and Lys 2008).²

However, real earnings management (REM) hardly exists in a vacuum. Managers have to potentially justify any short-term actions undertaken to boost earnings but detrimental for long-term value to the investing community and to financial analysts. This may induce them to misrepresent their actions and issue misleading statements that in turn become subject to lawsuits. Further, REM is even more negatively associated with future firm performance than accruals management (Cohen and Zarowin 2010; Kothari et al. 2016). Shareholders seeking recompense for negative outcomes consequently have incentives to identify real actions that misled them and destroyed value. Thus, high threat of litigation can conceivably deter managers from engaging in real earnings management.

¹ See, for example, Skinner (1994), Field Lowry and Shu (2005), Houston, Lin, Liu, and Wei (2015), Hopkins (2014), Cazier, Christensen, Merkley and Treu (2017).

² Cohen et al. (2008) document firms substituted accruals management with more difficult-to-detect real earnings management after the Sarbanes and Oxley Act in 2002, which strengthened regulatory oversight over financial reporting.

REM encompasses a wide variety of activities with adverse consequences that can be subject to litigation. For example, overproduction to spread out fixed costs and understate cost of goods sold typically inflates current-period earnings but also results in excess inventory. Larger inventories often lead to companies booking losses in the future because of increased obsolescence risk. Upon observing such losses, shareholders can accuse managers of overstating earnings at the expense of long-term value. Furthermore, as in the case of the 2012 class-action lawsuit against General Motors (hereafter GM), shareholders may also allege that managers engaged in overproduction to mislead investors into forming inflated expectations of future demand and revenue.³

Another example involves “channel stuffing”, or aggressively shipping inventories to intermediate dealers without supporting retail demand in order to book sales. Indeed, channel stuffing was included among the security class action allegations against GM. Channel stuffing can lead investors to form inflated expectations of future demand and can involve offering price discounts to dealers that they come to expect even in future periods, reducing long-term margins (Roychowdhury 2006). Similarly, an aggressive (and opportunistic) reduction of discretionary expenses can be misinterpreted as having a persistent positive influence on earnings, when in fact these reductions are detrimental for a company’s long-term competitiveness (Cohen and Zarowin 2010, Kothari et al 2016, Vorst 2016). For example, the securities class action lawsuit against Hospira Inc. in 2011 accused the company of compromising crucial quality control procedures with the objective of reporting lower remediation costs and higher margins. The lawsuit alleged that by the time the FDA detected the quality control deficiencies, product quality had suffered, sales had declined, and the delayed remediation had become significantly more costly.

³ Please see Scott et al. v. Whitacre et al., case number 14-3770 in the U.S. Court of Appeals for the Second Circuit.

REM can also trigger litigation indirectly. Managers routinely have to explain their real decisions and actions to their investors and analysts, for example, during conference calls. If those real choices are motivated by the intent to mislead and boost short-term earnings at the expense of long-term value, managers have incentives to misrepresent their actions or the objectives underlying those actions. The resulting misleading statements can in turn lead to class-action lawsuits. For example, in 2009-2010, analysts and auditors suspected Green Mountain Coffee of overproducing inventory to under-report their cost of goods sold, due to a sustained increase in inventory levels over successive periods. Eventually, in an effort to convince investors that their production was driven by high expected demand, company officials allegedly started destroying and hiding excess inventory and claiming inflated sales. They ultimately faced a class-action lawsuit in September 2010 accusing them of falsely covering up via misleading disclosures their attempts to boost earnings via overproduction. The lawsuit relied heavily on evidence of overproduction to support the allegations. The case serves to illustrate that once shareholders litigate upon bearing losses, they scrutinize all past actions and not just misleading disclosures. A stricter litigation regime can therefore deter managers from engaging in REM, because (a) managers' real actions are potentially subject to scrutiny upon litigation and (b) attempts to "cover up" REM can themselves trigger litigation.

In summary, there are several counts on which shareholders can point to REM's role in overstating the company's operating performance and financial health, and damaging the value of their claims in the process. While there is no systematic evidence linking REM to litigation against managers and directors, Kim and Park (2014) find that REM at client firms is positively associated with auditors' legal exposure.

Studying the effect of litigation on earnings management is generally challenging because of intrinsic endogeneity. If firms more likely to engage in REM are more susceptible

to litigation, this would manifest as a positive association between REM and litigation likelihood. However this positive relation does not necessarily imply that litigation risk encourages managers to engage in REM (Skinner 1997; Field et al. 2005; Houston et al. 2015; Hopkins 2014).

To address endogeneity, we study changes in real earnings management following an exogenous shock to the stringency of securities class action litigation standards for firms located in the U.S. Ninth Circuit Court District. The Ninth Circuit court ruling is advantageous because it directly affected shareholders' ability to litigate via class action. In addition, we also investigate changes in REM following the staggered adoption across states of Universal Demand laws (UD laws). UD laws significantly weakened shareholders' ability to launch derivative lawsuits against directors and managers, and are not concentrated at a single point in time.

On July 2, 1999, the U.S. Ninth Circuit Court of Appeals issued an unexpected ruling (re: Silicon Graphics Inc. Securities Litigation) requiring plaintiffs to prove that defendants acted with "deliberate recklessness". The requirement significantly increased the hurdle for successful litigation against corporations headquartered in this circuit (Pritchard and Sale 2005).⁴ Further, the ruling was highly unexpected (Gibney 2001; Johnson, Nelson and Pritchard 1999), as this quote from Gibney (2001) indicates: "After advocating one of the least restrictive approaches to pleading scienter pre-PSLRA, the Ninth Circuit changed course dramatically with *Inre Silicon Graphics Inc. Securities Litigation*"⁵

⁴ The majority of lawsuits are litigated where the firms' headquarters are located (Cox, Thomas and Bai, 2009). The Ninth Circuit includes the following states: Alaska, Arizona, California, Hawaii, Idaho, Montana, Nevada, Oregon, and Washington. According to Johnson et al (1999), commenting on the 1999 ruling, "...the Ninth Circuit's interpretation in *Silicon Graphics* is the most stringent, requiring plaintiffs to allege facts that would show the defendants were "deliberately reckless" in making the misrepresentation that gave rise to the fraud claim..."

⁵ Relatedly, the ruling was unlikely to reflect the prevailing or evolving economic conditions, given the surprise it generated. Rather, it was much more likely to be driven by the ideologies of the ruling judges. The Ninth Circuit randomly selects three judges to adjudicate such cases (Hopkins 2014). In the case of *Silicon Graphics*, two conservative judges were selected, and they voted against the plaintiffs. The liberal-leaning third judge dissented, voting in favor of the plaintiffs.

Crane and Koch (2016) and Houston et al. (2015) demonstrate that after the ruling the number of class action lawsuits filed in the Ninth Circuit decreased significantly relative to other jurisdictions. In further support of a reduction in litigation risk, we find that a measure of litigation risk introduced by Kim and Skinner (2012) decreased significantly after the ruling for firms located in the Ninth Circuit relative to those located in other circuits. Since the shock affected only firms located in the states that belong to the Ninth Circuit, we are able to compare their post-ruling changes in REM to those of firms located in states belonging to other circuits in differences-in-differences tests.

Using a sample of firm-years which spans four years before and four years after the 1999 ruling, we find significant post-ruling increases in REM for the firms headquartered in the Ninth Circuit relative to the firms located in other circuits.⁶ Our results indicate that limiting shareholders' ability to file class action lawsuits increased managers' propensity to engage in REM in firms located in the Ninth Circuit, consistent with litigation deterring, rather than encouraging, REM. The results are not driven by unobserved firm characteristics since our regressions include firm fixed effects. We also rule out the concern that our results are driven by systematic differences in firm characteristics or economic conditions in the Ninth Circuit and other court circuits. In particular, we repeat our tests restricting our sample to propensity-score-matched control and treatment firms. In another test, we limit our sample to firms in adjacent counties on each side of the state border between a Ninth Circuit and a non-Ninth-Circuit state. We obtain similar results with both analyses. Our inferences are further robust to the inclusion of industry-year fixed effects which control for differential industry trends which may arise if the industry composition of the Ninth Circuit firms and firms located in other circuits differs systematically.

⁶ We use 1995-1998 as the period before the court ruling and 1999-2002 as the period after the ruling. In untabulated robustness tests, we confirm that our results are robust to using a three-year window before and after the ruling, where the period before the ruling encompasses years 1996-1998, and the period after the ruling encompasses 1999-2001.

To rule out the possibility that our results are driven by the collapse of the technology bubble in years 2000-2002, we exclude technology firms and firms in industries that had negative cumulative abnormal returns over the three-year period.⁷ We continue to obtain the same results. Additionally, we confirm that there are no trending differences in REM between the Ninth Circuit firms and firms located in other circuits before the court ruling. We thus find robust evidence that class action litigation deters real earnings management.

We expect that if litigation indeed deters REM then this deterrence effect should be more crucial in firms with poorer governance. Accordingly, we next examine whether the post-ruling increases in REM are more pronounced for firms with poor corporate governance. In our tests, we focus on institutional ownership and managerial entrenchment as measures of governance. Prior literature points to institutional investors' monitoring role in general and with respect to REM in particular (Bushee 1998; Roychowdhury 2006). Such investors are sophisticated and thus well equipped to detect REM. They are also known to influence managerial actions via either voice or the threat of exit and do not rely solely on litigation (Parrino, Sias and Starks 2003; Edmans 2009; Edmans and Manso 2011). Given the negative consequences of REM for future growth and firm value, institutional investors are likely to discourage these actions. Consequently, we predict that the post-ruling increases in REM are greater for Ninth Circuit firms with lower institutional ownership. Our second governance measure, managerial entrenchment, reflects circumstances in which monitoring by the board of directors, shareholders and other parties is generally less effective, enabling managers to pursue actions that best serve their own interests (Bebchuk, Cohen, and Ferrell 2009). We thus expect the post-ruling increases in REM for Ninth Circuit firms to be particularly pronounced

⁷ Following Gu and Li (2007), technology industries are defined as those with the following two-digit SIC industry codes: 28 (chemicals and allied products), 35 (industrial and commercial machinery and computer equipment), 36 (electronic and other electrical equipment and components, except computer equipment), 37 (transportation equipment), 38 (measuring, analyzing, and controlling instruments; photographic, medical and optical goods; watches and clocks), and 73 (business services).

when managers are more entrenched. We find evidence that supports both of our cross-sectional predictions.

To ensure that we are capturing managers' attempts to overstate earnings through real actions we next link the measures of REM directly to earnings performance. We repeat our analyses focusing on instances in which firms used real transactions to report higher-than-expected return on assets, or ROA (Kothari, Mizik and Roychowdhury 2016). We find that after the ruling, the frequency of reporting abnormally high ROA through REM increases more for the Ninth Circuit firms than for firms from other circuits.

Finally, we find that our inferences extend to exogenous shocks to investors' ability to launch derivative lawsuits, a different type of litigation. These lawsuits do not require shareholders to form a class. They can be filed by any shareholder on behalf of the corporation when managers or directors are in breach of their fiduciary duty. These breaches include instances of managers or directors mishandling corporate information or taking actions that hurt firm value. Staggered adoption of universal demand (UD) laws in 23 states between 1989 and 2005 made the filing of derivative lawsuits significantly more difficult by requiring that shareholders seek the board's approval before initiating litigation (Appel 2016). Consistent with our main results that are based on securities class action litigation, we observe significant increases in firm-level REM after a state adopts UD laws relative to those at firms incorporated in other states that did not adopt UD laws at the time. This analysis affirms the generalizability of our conclusions as well as alleviates concerns about confounding effects resulting from the Ninth Circuit Court ruling occurring at one specific point in time.

While managers are expected to exercise their discretion in real activities to generate earnings for their firms, an enduring concern has been that they sometimes do so at the expense of long-term value by engaging in opportunistic real earnings management. Not

surprisingly, the literature has been interested in factors that constrain REM. Research to date has focused on various aspects of the firm's governance: the appropriate design of the executive compensation package (Chen et al. 2015), the checks and balances within a firm, that is, internal corporate governance (Cheng et al. 2016), and the monitoring role of long-term institutional investors (Bushee 1998; Roychowdhury 2006). Our results suggest that litigation is an important force because it provides recourse to shareholders when governance, incentive mechanisms and other sources of monitoring fail. The *ex post* settling-up opportunity that litigation provides also makes it an effective *ex ante* deterrent to REM.

In a related study, using the Ninth Circuit setting, Hopkins (2014) finds evidence of increased restatement frequency among Ninth Circuit firms following the ruling, and concludes that litigation deters misreporting. The literature has arrived at similar conclusions regarding the influence of litigation risk on voluntary disclosures. Studies such as Skinner (1994), Field, Lowry and Shu (2005), and Houston et al. (2015) provide evidence that the threat of litigation disciplines managers' incentives to delay the voluntary disclosures of impending bad earnings news. Importantly, forces that discipline reporting and disclosure incentives, such as regulatory scrutiny and litigation risk, can perversely motivate managers to gravitate towards opaque and costly tactics to manipulate earnings, for example, by altering the underlying real transactions (Cohen et al. 2008). In this context, our evidence is particularly valuable because it indicates that the threat of litigation makes it difficult for managers to substitute opportunistic real actions for misleading disclosure and accrual choices.

2. Setting and hypotheses

Securities Class Action Litigation offers shareholders the opportunity to form a class and hold managers and directors responsible in court for violating SEC rules via actions including accounting fraud, insider trading and providing misleading information on firm

performance. Until 1995, class action lawsuits relied on the “fraud on the market” theory. This theory, adopted by many courts in the mid-seventies, did not require plaintiffs to prove that managers issued misleading information or were responsible for material omissions, because all public information was presumably reflected in the stock price. A large stock price drop was thus synonymous with corrective revelations and sufficient to trigger litigation (Skinner 1994; Francis, et al. 1994). These low-pleading standards resulted in many frivolous lawsuits and in high dismissal rates (Johnson, Kasznik and Nelson 2001). In 1995, in response to the corporate lobbying, Congress passed the Private Securities Litigation Reform Act (PSLRA), which raised the standards for shareholders to form a class (Levine and Pritchard 1998). Among other provisions, PSLRA required plaintiffs to provide a proof of scienter (i.e., intent or knowledge of wrong-doing) but the exact interpretation of the act’s pleading standards was left to the individual U.S. circuit courts.

The Ninth Circuit Court was at the time considered one of the most plaintiff-friendly circuit courts with a high volume of securities class action litigation (Gibney 2001; Pritchard and Sale 2005). It thus came as a surprise that on July 2, 1999 the Ninth Circuit Court of Appeals issued a ruling (Re: Silicon Graphics Inc. Securities Litigation, 183 F.3d 970) that resulted in a much stricter interpretation of pleading standards than in any other circuit courts (Johnson, Nelson and Pritchard 1999; Grundfest and Pritchard 2002). According to the ruling, to form a class, the plaintiffs in the Ninth Circuit are required to show that the defendants acted with “deliberate recklessness”, rather than mere “recklessness” which is sufficient in other circuits.⁸ The Ninth Circuit ruling introduced a particularly high burden of proof given that evidence of intent is usually obtained only in discovery, after a class has been formed. Empirical evidence indicates that the ruling significantly reduced the incidence

⁸ Re: Silicon Graphics Inc. Securities Litigation, 183 F.3d 970 (9th Cir. 1999) involved an allegation that managers engaged in insider trading after issuing misleading statements to hype the stock price. The case was dismissed on the premise that stock sales coupled with negative internal performance news are insufficient to prove that managers’ actions were deliberate. The ruling was unexpected and likely reflected the beliefs of the judges randomly selected from the pool and appointed to this particular case (Hopkins 2014).

of class action lawsuits. For example, Crane and Koch (2016) document that relative to the first half of 1999, lawsuit filings in the second half decreased in the Ninth Circuit by 43% compared to an increase of 14% in other circuits.⁹ Pritchard and Sale (2005) point out that the exceptionally strict pleading standards in the Ninth Circuit lead to a higher rate of dismissals by courts in that circuit.

The Ninth Circuit Court ruling allows us to use a difference-in-difference framework in which we compare post-ruling changes in REM in firms headquartered in the Ninth Circuit to similar changes in firms headquartered in other circuits. We use the firm's headquarters to determine the most likely location of litigation because most lawsuits are ultimately litigated where the firm is headquartered.¹⁰ Filing lawsuits in districts other than the firm's headquarters is costly and time-consuming for the plaintiffs. Moreover, even if shareholders were to file lawsuits against the firm in various locations, legal panels consolidate these lawsuits and relocate them to the district with the easiest access to documents and witnesses, i.e., a firm's headquarters (Cox, Thomas and Bai 2009; Hopkins 2014; Houston et al. 2015). Overall, the Ninth Circuit court ruling *Re: Silicon Graphics Inc.* unexpectedly and exogenously reduced litigation risk for firms headquartered in that circuit, providing an ideal setting for our tests of litigation's disciplining effect on real earnings management.

In a related study, Hopkins (2014) finds that Ninth Circuit firms exhibit increased restatement frequency as well as higher revenue accruals following the ruling, consistent with litigation risk deterring managers from misstating financial statements. Similarly, Cazier, Christensen, Merkley and Treu (2017) document an increase in non-GAAP reporting following the Ninth Circuit ruling which they interpret as evidence that litigation discourages voluntary non-GAAP reporting. Houston et al. (2015) use the Ninth Circuit ruling to provide

⁹ Crane and Koch (2016) find a similar magnitude of post-ruling changes in lawsuits filings between the Ninth Circuit and other circuits over longer horizons as well (from the beginning of 1998 to the end of 2000, and over their sample period: 1996-2014).

¹⁰ Using 2,194 lawsuits obtained from the Institutional Shareholder Services, Hopkins (2014) finds that at least 84% of lawsuits are ultimately litigated in the district of the firm's headquarter.

evidence on the disciplining effect of litigation on management forecasts. The implications of litigation risk for REM are not as straight-forward, and depend on the net effect of two countervailing effects.

On the one hand, there are reasons why litigation may not only fail to deter REM, but even increase it. First, given the intrinsic information asymmetry about real operations between a firm's managers and its external stakeholders, REM is more opaque and thus harder to detect than financial reporting violations and accrual manipulations (Cohen and Zarowin 2010; Kothari et al. 2016). Second, REM involves business decisions which managers could justify as sound given their interpretation of the economic circumstances, invoking the so called "business judgment" principle. Proving intentional wrong-doing under securities law may thus be significantly more difficult in the case of REM, negatively impacting shareholders' ability to form a class and reducing any potential deterrence effect of litigation. Third, a more litigious environment that discourages opportunistic reporting choices can encourage REM. Cohen et al. (2008) document firms substituted accruals management with more difficult-to-detect real earnings management after the Sarbanes and Oxley Act in 2002, which strengthened regulatory oversight over financial reporting. Since existing evidence suggests that litigation risk deters opportunistic accrual and disclosure decisions (Skinner 1994; Field Lowry and Shu 2005; Hopkins 2014; Cazier, et al. 2017), it is possible that it is associated with a shift towards greater REM. This would imply that litigation encourages REM, and thus, REM in firms headquartered in the Ninth Circuit should decrease after the ruling reduced shareholders' ability to litigate.

On the other hand, REM is more costly than accrual manipulations and results in negative future returns that investors are likely to scrutinize and link to prior suboptimal actions. For example, Cohen and Zarowin (2010) and Kothari et al. (2016) find that REM is more negatively associated with future operating performance than accruals. Furthermore,

REM involves decisions on pricing, production and discretionary expenditures such as R&D that have more long-term negative implications for value across multiple periods (Graham Harvey Rajgopal 2005, Kothari et al 2016). Since the very objective of a securities class-action lawsuit is to identify deceptive actions by managers that resulted in shareholders experiencing value losses, litigating shareholders and their trial lawyers are probably even more motivated to detect attempts to overstate earnings via REM than via accruals. In addition, managers are often questioned about the validity of their operational and strategic choices, for example in investor meetings and conference calls. In justifying actions that boost short-term earnings but are detrimental for long-term value, managers may be forced to issue misleading statements that they are later held accountable for in class action lawsuits when the underlying truth is revealed. Anecdotal evidence cited in the Introduction provides support for shareholder holding managers responsible both for REM as well as for misleading statements that managers issued to obscure instances of REM. The expected costs of litigation resulting from REM are significant and include not only monetary expenditures but also loss of reputation and capital market penalties, even in instances when the lawsuits are ultimately not successful.¹¹ Thus, litigation potentially serves as an effective deterrent to REM, in which case, REM in firms headquartered in the Ninth Circuit should increase after the ruling reduced shareholders' ability to litigate.

In summary, we test the following null hypothesis:

Hypothesis 1 (null): Real earnings management (REM) in firms headquartered in the Ninth Circuit did not change after the 1999 ruling.

¹¹ For example, Hospira Inc. decided to settle the lawsuit against it alleging myopic cost-cutting for \$60 million. Even in cases when lawsuits are eventually dismissed, they can impose significant costs on the firm and give rise to a deterrence effect. The case against General Motors alleging channel-stuffing and overproduction dragged on for three years, absorbing managers' attention, their time and expensive legal resources. In addition the announcement of the lawsuit was associated with a 30% drop in stock price.

3. Sample

We begin our sample selection process by obtaining data on all publicly listed firms from the Compustat/CRSP database with non-missing information on historical headquarters over the period 1995-2002.¹² Our tests compare a pre-ruling period: four years preceding the Ninth Circuit court ruling (1995-1998), and a post-ruling period: four years beginning in the year of the ruling (1999-2002). We restrict our analyses to four years before and four years after the ruling to limit concerns about the potential effect of confounding events over longer horizons.¹³

The sample excludes firms in financial industries (sic 6000-6999) and utilities (sic 4000-4999), as well as penny stocks (firms with stock price smaller than \$1). We next require the availability of COMPUSTAT and CRSP data necessary to construct our control variables: ROA, total assets, firm age, dividends, capital expenditures, leverage, cash, debt issues, and repurchases. We also control for institutional ownership and assume zero institutional ownership when the firm is not included in Thomson Financial 13F files. We winsorize all continuous variables at the 1st and 99th percentile every year. All variables are defined in Appendix A.

In our tests, we use the maximum number of observations we can obtain after requiring the dependent variable as well as the partitioning variable for that test. For example, our main analysis of the ruling's effect on abnormal discretionary expenses of the Ninth Circuit firms relative to non-Ninth Circuit firms includes 5,493 firms and 20,706 firm-year observations between 1995 and 2002. We classify a firm as treated (or a "Ninth Circuit" firm)

¹² To determine the relevant court circuit, we use the firm's historical headquarters at the time of the ruling obtained from 10-K filings from Edgar online. The historical headquarters are not available from COMPUSTAT, which backfills the data and instead reports the current headquarters for all firm-years in the database.

¹³ Using an alternative window of three years before and after 1999 that is 1996-2001, does not materially influence our results. All our ensuing discussion is thus based on the window of four years before and after 1999.

if its headquarters are located in one of the states subject to the Ninth Circuit Court.¹⁴ The remaining firms are classified as “controls”.

4. Empirical Analysis

4.1. Research Design and Variable Definition

Our multivariate tests employ a differences-in-differences research design in which we compare the changes in REM following the 1999 ruling for treated firms (firms headquartered in states located in the Ninth Circuit or “Ninth Circuit” firms) to the corresponding changes for control firms (firms headquartered outside of the Ninth Circuit).

We estimate the following difference-in-difference regression specification:

$$REM_{it} = \beta_0 + \beta_i Post_t \times Ninth\ Circuit_i + \gamma' X_{it} + FirmFE + YearFE + \varepsilon_{it} \quad (1)$$

i indexes firms and t time. The *Post* indicator takes the value of one from 1999 to 2002, and zero from 1995 to 1999 while the *Ninth Circuit* indicator takes the value of one in all sample years if the firm’s headquarters are located in one of the Ninth Circuit states. We expect a significant and positive β , the coefficient on the interaction term $Post_t \times Ninth\ Circuit$. Because we include firm (*FirmFE*) and year (*YearFE*) fixed effects, we do not include *Ninth Circuit* and *Post* indicators separately. These indicators are absorbed in the firm fixed effects and year fixed effects respectively. We cluster standard errors by the state of headquarters.

Our tests include three measures of REM: (1) abnormal discretionary expenses (*Abndisx*), (2) abnormal production costs (*Abnprod*), and (3) an aggregate measure which combines *Abndisx* and *Abnprod*, denoted *RM* (see, for example, Cohen and Zarowin 2010

¹⁴ Firms change their headquarters infrequently. During our sample period, 189 firms (3.4% of the sample) changed headquarter location, with 33 firms switching from non-Ninth Circuit states to Ninth Circuit states and 29 firms switching the other way. The largest shift occurred within non-Ninth Circuit firms, with 122 firms changing states, but not moving out of or into the Ninth Circuit. Our results are robust to excluding firms that changed states of headquarters.

and Zang 2012).¹⁵ Following Kothari et al. (2016), we first calculate residuals from the following models for normal levels:

$$DiscExp_{i,t} = \alpha_0 + \alpha_1 DiscExp_{i,t-1} + \alpha_2 I / Assets_{i,t-1} + \alpha_3 Sales_{i,t} + \varepsilon_{i,t} \quad (2)$$

$$Prod_{i,t} = \alpha_0 + \alpha_1 Prod_{i,t-1} + \alpha_2 I / Assets_{i,t-1} + \alpha_3 Sales_{i,t} + \alpha_4 \Delta Sales_{i,t} + \alpha_5 \Delta Sales_{i,t-1} + \varepsilon_{i,t} \quad (3)$$

DiscExp is discretionary expenses (the sum of advertising expenses, R&D expenses and SG&A expenses) and *Prod* is production costs (the sum of the cost of goods sold and the change in inventory). In order for all measures to be consistent and increase in the level of earnings management, *Abndisx* is given by the residual from equation (2) multiplied by negative one. *Abnprod* is the residual from equation (3). We add *Abndisx* and *Abnprod* to construct the aggregate measure, *RM*.

X in equation (1) represents a vector of control variables. We control for firm characteristics such as size (*Firm Size*), performance (*ROA*), capital structure and various investment and financing decisions (*Leverage*, *Cash*, *Dividends*, *Repurchases*, *Debt Issue*, *CAPEX*). Given prior evidence on the link between institutional ownership and REM, we include the percentage of shares held by institutional investors (*IO*) in our models. To ensure that variation in economic and demographic factors at the state level does not influence our results we control for a number of socio-economic variables at the state level (*GDP Growth*, *Unemployment*, *Income*, *Population*, *Gender*, *Ethnicity*, *Education*, *Vote Democrat*).

4.2. Descriptive Statistics

Table 1 reports the descriptive statistics for real earnings management measures as well as the control variables for our sample. The number of observations is slightly lower for *Abnprod* than it is for *Abndisx* as a consequence of data availability (19,194 versus 20,706). The intersection of observations with sufficient data to compute both measures and to

¹⁵ We do not use abnormal cash flow from operations (CFO) because different methods of real earnings management can have opposite effects on CFO (Roychowdhury, 2006).

construct our aggregate measure *RM* includes 19,177 firm-years. The exclusion of penny stocks and the data requirements for computing earnings management proxies leads to slightly larger and more profitable firms in our sample relative to the COMPUSTAT population. The median firm in our sample has an ROA of 13.0%, market capitalization of \$154 million and institutional ownership of 29.2%.

Table 2 provides some preliminary insights into the differences-in-differences in REM in a univariate setting. The table reports pre- and post-ruling levels of our three REM measures as well as changes in those measures separately for firms incorporated in the Ninth Circuit (Panel A) and firms incorporated in other circuits (Panel B). Table 2 also reports p-values corresponding to the t-tests examining whether changes in REM from the pre- to the post-ruling period are significantly different from zero. As Panel A demonstrates, Ninth Circuit firms exhibit increases in all three metrics of REM (*Abndisx*, *Abnprod* and *RM*) following the ruling, and these increases are statistically significant at the 5% level. Firms outside the Ninth Circuit exhibit a statistically significant decline in *Abndisx* across 1991, implying a decrease in earnings overstatement via abnormal discretionary expenses. At the same time, *Abnprod* increases for firms outside the Ninth Circuit and the composite measure *RM* declines. Panel C reports our main tests that compare the changes in REM for the Ninth-Circuit firms relative to those for the non-Ninth-Circuit firms. This test allows us to draw conclusions about changes in Ninth Circuit firms after controlling for any general trends in REM using changes in firms headquartered outside of the Ninth Circuit as benchmarks. Panel C reports that all three measures of REM increase significantly after the ruling for the Ninth-Circuit firms relative to the firms located in other states. The increases in *Abndisx* and *RM* are significant at the 1% level, while that in *Abnprod* is significant at the 10% level. Our preliminary conclusions from the univariate tests are thus consistent with our main

hypothesis: a decrease in litigation risk resulted in a significant increase in real earnings management.

4.3. Results on changes in real earnings management

In this section we examine in multivariate tests whether the Ninth Circuit court ruling and the resulting decrease in litigation risk led to any significant changes in the firms' propensity to engage in REM. In particular, we estimate equation (1) using our three measures of real earnings management as dependent variables. The difference-in-difference research design allows us to compare the post-ruling changes in REM at the Ninth Circuit firms relative to post-ruling changes at other firms. Table 3 presents the results. The coefficients on the interaction term *Post*Ninth Circuit* are positive and significant, with p-values less than 0.05 for *Abndisx* and the aggregate measure *RM*, and with p-value of 0.051 for *Abnprod*. The coefficient point to statistically significant increases in REM following the ruling for Ninth Circuit firms relative to other firms. Moreover, these increases are economically significant. The increase in *Abndisx* represents 13.3% of one standard deviation in *Abndisx* and that in *Abnprod* represents 7.3% of one standard deviation of *Abnprod*. The increase in the aggregate measure is even more pronounced, at 16.1% of one standard deviation. Thus, the results indicate that a decline in litigation risk with the Ninth Circuit ruling led to significant increases in REM at Ninth Circuit firms relative to other firms, consistent with litigation's deterrence effect on REM.

We next perform a number of robustness tests on these multivariate results. We begin by conducting a parallel-trends analysis. This analysis helps us establish if the relative changes in REM we document are present only after the ruling or, to the contrary, they precede the ruling. If changes in REM precede the ruling, they are attributable to trending differences between the Ninth Circuit firms and the firms headquartered in other circuits and do not result from the ruling itself. Table 4 reports the results of this parallel-trends analysis.

We find no trending differences between the Ninth Circuit and other circuit firms before the ruling. The coefficients on $D(t=-1)*Ninth\ Circuit$, and $D(t=-2)*Ninth\ Circuit$, indicators which take the value of one for the Ninth Circuit firms in year 1998 and 1997 respectively, are not significantly different from zero in any of the columns. The differences begin to appear in the year of the ruling ($D(t=0)*Ninth\ Circuit$ is significant and positive in two of the three columns), and are significantly positive in subsequent years, as indicated by the coefficients on $D(t=1)*Ninth\ Circuit$ and $D(t \geq 2)*Ninth\ Circuit$. The results of this analysis provide assurance that there are no pre-existing differential trends in real earnings management between the Ninth Circuit firms and firms headquartered in other circuits.

In our next set of robustness analyses we aim to further alleviate any concerns that systematic differences between the Ninth Circuit firms and non-Ninth Circuit firms contribute to our findings by inducing dissimilar trends in real earnings management across the subsamples. To address general concerns about the industry composition differing between the Ninth Circuit firms and the non-Ninth-Circuit firms, we re-estimate our regressions after augmenting them with industry-year fixed effects. Our results are fully robust to this alternative specification (not tabulated).

Next we consider the specific concern that Ninth Circuit firms are disproportionately represented by high technology industries and these firms experienced more pronounced and widespread declines in market values in the post-ruling period with the crash of the internet bubble in 2000. Because firms in high-technology industries faced more negative economic circumstances, their REM measures could be systematically biased relative to those of other firms. To directly address this concern, we conduct two separate tests in which we exclude from our sample (1) all firms in high technology industries, and (2) all firms in industries with negative cumulative abnormal returns over the years 2000-2002. Table 5, Panel A reports the results with high technology industries excluded from the sample, while Table 5,

Panel B tabulates the results based on the subsample of firms with no negative cumulative abnormal returns over the years 2000-2002 at the industry level. While these robustness tests result in significant decreases in the number of observations, we continue to find significant and positive coefficients on *Post*Ninth Circuit* in all columns.

To ensure that our results are not driven by dissimilar political and economic trends across the Ninth and other circuits we perform an adjacent-county analysis, in which we only include firms with headquarters located close to the state borders, where one of the borders passes the Ninth Circuit. Specifically, we limit our sample to firms with headquarters within 60 miles on either side of the state border that passes the Ninth Circuit.¹⁶ Because both treatment and control firms in this sample are located in close geographic proximity of each other, this test alleviates some of the potential concerns regarding geographic variation in economic, political and social trends. We continue to include controls for state level macro-economic, political and economic conditions in these tests. The results of this test are reported in Table 5, Panel C, and yield inferences consistent with the main tests: we find positive coefficients on *Post*Ninth Circuit* in all three columns with p-values less than 0.05.

To further assure that the composition of firms is similar in the subsamples of the Ninth-Circuit and non-Ninth-Circuit firms on dimensions other than just industry membership, we construct a propensity score matched sample of treatment and control firms. Specifically, we limit our sample to the Ninth-Circuit and non-Ninth-Circuit firm-years that are propensity score matched using the following characteristics: industry membership, ROA, size, dividends, capital expenditures, leverage, cash holdings, net debt issuance, stock repurchases and institutional ownership. Table 6, Panel A shows the mean values of firm characteristics for the Ninth-Circuit and non-Ninth-Circuit firms in this propensity score

¹⁶ We also perform a robustness test using 40 miles as an alternative distance from the Ninth Circuit state border. Even though the sample size is reduced significantly, the coefficients on *Post*Ninth Circuit* are significant and positive in all three specifications with different measures of real earnings management as dependent variables.

matched sample, as well as p -values from t -tests of differences. The matching is successful, yielding no significant differences in the overall propensity matching score or any of the firm characteristics. Table 6, Panel B, columns (1)-(3) report the results from re-estimating equation (1) on this propensity score matched sample. Despite a substantially lower number of observations, we find significant post-ruling increases in REM at the Ninth Circuit firms relative to those at similar firms headquartered in other circuits. The coefficient on *Post*Ninth Circuit* is statistically significant at the 5% level for *Abndisx* and *RM* and at the 10% level for *Abnprod*. In summary, our results are robust to various and alternative specifications and point to significant increases in REM after the decline in litigation risk for Ninth Circuit firms relative to other firms with the 1999 ruling.

4.4 Results on the role of managerial entrenchment and institutional ownership

The evidence till this point robustly indicates that REM is lower in the presence of higher litigation risk, implying that litigation deters REM. In this section, we investigate cross-sectional variation in litigation's deterrence effect on REM. We predict that litigation's deterrence role is particularly crucial in firms with poor corporate governance. Several aspects of corporate governance have been shown to mitigate REM including ownership by sophisticated shareholders, structure of compensation contracts, and internal balance of power (Bushee 1998; Roychowdhury 2006; Chen, Cheng, Lo, and Wang 2015; Cheng, Lee and Shevlin 2016). We expect that the opportunity for ex post settling-up that litigation provides is likely to be a more valuable deterrent when governance is weaker and less effective in restraining managers from engaging in REM. The post-ruling decline in litigation risk thus implies that any increase in REM at Ninth Circuit firms is likely to be more pronounced among firms with weaker corporate governance. We focus on two aspects of governance: managerial entrenchment and institutional ownership. Greater managerial entrenchment implies weaker governance, as both the board of directors and the market for

corporate control find it more difficult to discipline entrenched managers (Bebchuk et al. 2009). This provides entrenched managers incentives to extract wealth from shareholders via various actions, including earnings management. We thus expect litigation's deterrence effect on REM to be more pronounced in firms with more entrenched managers. Turning to our second measure of governance, institutional owners are sophisticated investors who are likely to monitor managers' real choices more closely, and discourage actions that are detrimental for long-term value. Among firms with low institutional ownership, the lack of this external monitoring by shareholders makes it easier for managers to inflate current earnings myopically via REM (Bushee 1998; Roychowdhury 2006). We thus expect litigation's deterrence effect on REM to be more pronounced in firms with lower institutional ownership.

Empirically, we expect the decline in litigation risk after the court ruling to lead to greater increases in REM among firms with more entrenched managers and firms with lower institutional ownership. We first investigate whether post-ruling increases in REM at the Ninth Circuit firms relative to non-Ninth-Circuit firms are particularly acute when managers are more entrenched. As in Bebchuk, Cohen, and Ferrell (2009), we use the entrenchment index (*E-Index*) to capture managerial entrenchment. *E-Index* is based on six provisions, four of which constitute limitations on shareholders' voting power and the remaining two are measures against hostile takeovers. To conduct this test, we augment equation (1) with the following terms: *E-Index*, *Post*E-Index*, *Ninth Circuit*E-Index*, and *Post*Ninth Circuit*E-Index*. We are particularly interested in the coefficient on *Post*Ninth Circuit*E-Index*. Table 7, Panel A reports the results of this analysis. We find that the coefficient on *Post*Ninth Circuit*E-Index* is significant and positive in all three columns, which supports our prediction that the post-ruling increases in REM at the Ninth Circuit firms are stronger when managers are more entrenched.

We also expect the post-ruling increases in real earnings management to be especially pronounced when alternative sources of external monitoring, such as institutional owners, are less influential. Ninth Circuit firms with greater institutional ownership should thus experience weaker post-ruling increases in REM. To test this prediction, we expand equation (1) to supplement the main effect of institutional owners (IO) with the following interaction terms: $Post*IO$, $Ninth\ Circuit*IO$, and $Post*Ninth\ Circuit*IO$. Table 7, Panel B reports the results of this test. Consistent with our expectations, the coefficient on $Post*Ninth\ Circuit*IO$ is negative and significant in all three columns, indicating that the post-ruling increases in REM at the Ninth Circuit firms are stronger when institutional ownership is lower.

4.5 Results on real earnings management to report abnormal ROA

We next combine our measures of real earnings management with indicators for firm-years in which return on assets (ROA) is abnormally high, to ensure that we are indeed capturing managerial attempts to overstate earnings via real transactions (Kothari et al. 2016). Following Kothari et al. (2016), we estimate abnormal ROA as the residual from the following fixed-effect first-order autoregressive model:

$$ROA_{i,t} = \alpha_0 + \alpha_1 ROA_{i,t-1} + \alpha_2 1 / Assets_{i,t-1} + \varepsilon_{i,t} \quad (4)$$

where $ROA_{i,t}$ is operating income over lagged total asset; $ROA_{i,t-1}$ is its lagged value; $Assets_{i,t-1}$ is lagged total assets.

For every REM measure, we create a corresponding indicator variable equal to one when both abnormal ROA and the REM measure are greater than zero. This process yields three new binary variables ($AbnROA_Abndisx$, $AbnROA_Abnprod$, and $AbnROA_RM$). Using these variables as alternative dependent variables, we re-estimate our main, as well as cross-sectional, specifications. Table 8, Panels, A, B, and C report the results from these logit

estimations.¹⁷ Panel A reports the results of our main tests with these new dependent variables. Consistent with our inferences from Table 3, we find significant and positive coefficients on *Post*Ninth Circuit* in all three columns in Panel A of Table 8, indicating that the frequency of REM to overstate earnings increased after the ruling at the Ninth Circuit firms over and above any changes at the firms headquartered in other circuits. Panels B and C report cross-sectional results where the partitioning variables are managerial entrenchment and institutional ownership respectively. In Panel B, we find a significant and positive coefficient on *Post*Ninth Circuit*E-Index*, consistent with managerial entrenchment magnifying post-ruling increases in REM to report abnormally high ROA. It also appears that the increases in REM to report abnormally high ROA are weaker when a higher percentage of the firm's shares are owned by institutional investors. The coefficient on *Post*Ninth Circuit*IO* is significant and negative in all columns of Panel C.

4.6 Derivative lawsuits and real earnings management

In our final set of analyses, we examine another shock to the ability of shareholders to file lawsuits which involves derivative, rather than securities class action, litigation. The purpose of this analysis is to increase the generalizability of our results. Unlike the Ninth Circuit Court ruling, the shock to the filing of derivative lawsuits was staggered over time, which addresses potential concerns that the deterrence effect of litigation on REM is limited to one specific time-period or event.

Most accounting research to date has focused on securities class action lawsuits, likely because this type of litigation results mostly from violations of disclosure, financial reporting and insider trading regulations, and provides shareholders with an opportunity to

¹⁷ We include industry fixed effects in place of firm fixed effects in the logit model to avoid the incidental parameters problem (Wooldridge 2010), which can result in inconsistent coefficients in logit models with panel data because of a high number of “nuisance” parameters as the number of unique observations increases. When we estimate an OLS regression with firm fixed effects in place of industry fixed effects, we obtain similar results.

recover damages, making these lawsuits very common (Skinner 1994; Field, Lowry and Shu 2005). Derivative lawsuits allow shareholders to sue managers and/or directors *on behalf of the corporation* (rather than on behalf of shareholders') for a breach of fiduciary duty. As documented by Ferris, Jandik, Lawless and Makhija (2007), these lawsuits are filed for a variety of reasons, including violating the duty of care (41%), duty of loyalty (26%), mishandling corporate information (16%), and concerns about mergers and acquisitions (7%). Plaintiffs almost always prefer securities class action litigation but derivative lawsuits are often filed in addition to class action lawsuits whenever there are legal grounds for initiating both types of litigation (Erickson, 2010; Appel 2016).

Derivative lawsuits involve some procedural hurdles, including the “demand requirement”. Under this requirement, before filing a lawsuit, shareholders are required to demand first that the board of directors make corrective actions, including initiating litigation, which the board can accept or refuse. This requirement often represents a clear conflict of interest since some or all directors would be named as defendants in such a lawsuit. The futility exception allows shareholders to circumvent this demand requirement, and file a derivative lawsuit without consulting the board of directors if the directors cannot objectively evaluate the demand. However, critics of the futility requirement argue that this requirement results in filing frivolous lawsuits.

Staggered adoption of universal demand (UD) laws in 23 states between 1989 and 2005 was a response to the criticism that there are too many unsubstantiated lawsuits. The adoption made the filing of derivative lawsuits significantly more difficult by requiring that shareholders seek the board's approval before initiating litigation (Appel 2016). Appel (2016) shows that the filing of derivative lawsuits decreased significantly and corporate governance became weaker after the passage of UD laws for firms incorporated in states that adopted those laws (UD states) relative to firms in states that did not adopt the laws as of that year.

Moreover, Houston, Lin and Xie (2015) document that cost of capital increased for firms in UD states following the passage of these laws, and Bourveau, Lou and Wang (2014) find that free of litigation concerns, managers started to issue more optimistic management forecasts. Overall, prior evidence indicates that the passage of UD laws reduced the incidence and the deterrence effect of litigation.

In this section, we examine the effect of a reduction in the deterrence effect of derivative lawsuits on real earnings management. The main benefit of this test is that the adoption of UD laws was staggered across time and across states. As a result, this analysis is a good complement to our main tests in which we study a shock that affected one group of firms at one particular point in time and compare it to firms that were not affected by the shock.

To perform this analysis we create a sample of firm years encompassing years 1985-2009, which ranges from 4 years before the first state adoption of UD laws to four years after the last state adoption.¹⁸ We replace *Post*Ninth Circuit* in equation (1) with an indicator variable *Post(UD)*UD* which takes the value of 1 for firm-years incorporated in a state that passed UD laws after the passage of the laws.¹⁹ As in our previous tests, we include firm and year fixed effects which absorb separate indicator variables for firms incorporated in the states that passed UD laws and for the period following the passage of the laws by a particular state. We cluster standard errors at state of incorporation level. Table 8 reports the results from this specification. Because our sample for this analysis begins in 1985, certain state level control variables are not available. We thus present our results on a large sample of firm-years without state controls (columns (1) through (3)), and for the subsample of firm-

¹⁸ The years of individual state adoptions are as follows: GA and MI in 1989, FL in 1990, WI in 1991, MT, VA, UT in 1992, NH and MS in 1993, NC in 1995, AZ and NE in 1996, CT, ME, PA, TX, WY in 1997, ID in 1998, HI in 2001, IA in 2003, MA in 2004, RI and SD in 2005.

¹⁹ In these tests we use historical state of incorporation obtained from 10-K filings from Edgar online. Our sample excludes firms that switched states of incorporation, penny stocks and firms in regulated industries.

years with state controls available (columns (3) through (6)).²⁰ The coefficient on $POST(UD)*UD$ is significant and positive in all six columns, pointing to significant increases in REM for firms incorporated in states adopting UD laws relative to those for firms incorporated in other states. Consistent with our main results based on securities class action litigation, we conclude that litigation serves a deterrence role with respect to real earnings management and that REM increases when that role is reduced.

5. Conclusion

Shareholders can pursue various courses of action to prevent managers from violating their fiduciary duty and engaging in actions that transfer wealth away from shareholders. These actions include exercising governance through vote or exit (Parrino, Sias and Starks 2003; Edmans 2009; Edmans and Manso 2011; Appel, Gormley and Keim 2016). In certain circumstances, shareholders are also able to file lawsuits against the firm's management and/or directors. Because litigation has serious reputation and career implications for management and directors (Srinivasan 2005; Karpoff, Lee and Martin 2008; Brochet and Srinivasan 2014), it also serves a deterrence role. While prior literature has investigated the disciplining effect of litigation on management forecasts, as well as GAAP violations (Skinner 1994; Field Lowry and Shu 2005; Houston, Lin, Liu, and Wei 2015; Hopkins 2014), no prior research has explored whether this effect of litigation extends to real earnings management. Establishing whether it does is important because real earnings management can mislead shareholders and allow managers to report stronger current performance while sacrificing long-term firm value.

In this paper, we provide evidence that litigation serves an important deterrence role with respect to real earnings management. Using an exogenous shock that increased the

²⁰ We obtained robust results when we include state of location and year joint fixed effects in the model. The state of location and year joint fixed effects absorb any differences in REM that are driven by differences in state characteristics (e.g social-economic) over time.

difficulty of filing lawsuits in the Ninth Circuit Court, we find that real earnings management increased significantly after the ruling for the firms located in the Ninth Circuit relative to firms headquartered in other states and thus not subject to the shock. We find that these increases were particularly pronounced at firms with weaker corporate governance: when managers were more entrenched and institutional ownership was lower. Our results are robust to various alternative explanations and extend to another shock to the stringency of litigation – the passage of universal demand laws that increased the difficulty of filing derivative lawsuits. Together our results suggest that litigation aids corporate governance in deterring real earnings management. The ex post opportunity for settling up that litigation provides and its deterrence effect on REM is particularly valuable in firms that lack mechanisms such as corporate governance and monitoring by sophisticated shareholders that would ex ante constrain such activities.

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Appendix A Variable Definitions

Variables	Definition
<i>Ninth Circuit</i>	Indicator equal to one if firms are located in Ninth Circuit States.
<i>Post</i>	Indicator equal to one for years from 1999 and zero otherwise
<i>ROA</i>	Operating income over lagged total asset
<i>Total Assets</i>	Total assets (in million)
<i>Firm Size</i>	Log of total assets
<i>Dividend</i>	Dividend over total assets
<i>CAPEX</i>	Capital expenditures over total assets
<i>Leverage</i>	Short term debt plus long term debt, divided by total assets
<i>Cash</i>	Cash over total assets
<i>Debt Issue</i>	Long-term debt issuance minus long-term debt reduction, divided by total assets
<i>Repurchase</i>	Purchase of common and preferred stock-changes in preferred stock redemption value, divided by total assets
<i>IO</i>	Percent of shares outstanding held by institutional investors
<i>GDP Growth</i>	State GDP growth rate
<i>Unemployment</i>	State unemployment rate
<i>Income</i>	State income per capita
<i>Population</i>	Log of state population
<i>Gender</i>	Percentage of female population at state level
<i>Ethnicity</i>	Percentage of white population at state level
<i>Education</i>	Percentage of people with at least territory education at state level
<i>Vote Democrat</i>	Percentage of people vote for Democrat at last presidential election.
<i>Abndisx</i>	<p>Following Kothari et al (2016), we estimate abnormal discretionary expenditures with a fixed-effect first-order autoregressive model:</p> $DiscExp_{i,t} = \alpha_0 + \alpha_1 DiscExp_{i,t-1} + \alpha_2 1 / Assets_{i,t-1} + \alpha_3 Sales_{i,t} + \varepsilon_{i,t}$ <p>Where $DiscExp_{i,t}$ is the sum of advertising expenses, R&D expenses and SG&A expenses, all scaled by lagged total assets; $DiscExp_{i,t-1}$ is its lagged value; $Assets_{i,t-1}$ is lagged total assets; $Sales_{i,t}$ is sales during the year scaled by lagged total assets.</p> <p>To control for year-specific and firm-specific effects that induce model misspecification, we employ the model proposed by Kothari et al (2016). In their words: “First, every firm’s discretionary expenditure is differenced from the cross-sectional mean for that year. Second, for every firm, the annual deviation of discretionary expenditure from the cross-sectional mean is differenced from the corresponding deviation in the previous year. The explanatory variables in the model are also differenced twice in the same manner.” The model is estimated every year. The firm-year residual minus the mean value of the residual across all years for the corresponding firm, times minus one, yields abnormal discretionary expense or <i>Abndisx</i> for that firm-year. Higher values of <i>Abndisx</i> represent greater cuts in discretionary expenses and more earnings management.”</p>
<i>Abnprod</i>	<p>Following Kothari et al (2016), we estimate abnormal production cost with a fixed-effect first-order autoregressive model:</p> $Prod_{i,t} = \alpha_0 + \alpha_1 Prod_{i,t-1} + \alpha_2 1 / Assets_{i,t-1} + \alpha_3 Sales_{i,t} + \alpha_4 \Delta Sales_{i,t} + \alpha_5 \Delta Sales_{i,t-1} + \varepsilon_{i,t}$ <p>Where $Prod_{i,t}$ is the sum of COGS and change in inventory during the year scaled by lagged total assets; $Prod_{i,t-1}$ is its lagged value; $Assets_{i,t-1}$ is lagged total assets; $Sales_{i,t}$ is sales during the year scaled by lagged total assets; $\Delta Sales_{i,t}$ is the sale growth scaled by lagged total assets; $\Delta Sales_{i,t-1}$ is its lagged value.</p> <p>To control for year-specific and firm-specific effects that induce model misspecification, we employ the model proposed by Kothari et al. (2016) and described in the definition for <i>Abndisx</i> but using production cost (instead of discretionary expenses) as the starting point. The model is estimated every year. The firm-year residual minus the mean value of the residual across all years for the corresponding firm yields abnormal production expense or <i>Abnprod</i> for that firm-year.</p>
<i>RM</i>	$abndisx + abnprod$

Appendix A Variable Definitions, Continued

<i>Abn ROA</i>	<p>Following Kothari et al (2016), we estimate abnormal ROA with a fixed-effect first-order autoregressive model:</p> $ROA_{i,t} = \alpha_0 + \alpha_1 ROA_{i,t-1} + \alpha_2 1 / Assets_{i,t-1} + \varepsilon_{i,t}$ <p>Where $ROA_{i,t}$ is operating income over lagged total asset; $ROA_{i,t-1}$ is its lagged value; $Assets_{i,t-1}$ is lagged total assets;</p> <p>To control for year-specific and firm-specific effects that induce model misspecification, we employ the model proposed by Kothari et al. (2016) and described in the definition for <i>Abndisx</i> but using ROA (instead of discretionary expenses) as the starting point. The model is then estimated every year. The firm-year residual minus the mean value of the residual across all years for the corresponding firm yields abnormal ROA for that firm-year.</p>
<i>Abndisx Beat</i>	Indicator equal to one if a firm's <i>Abn ROA</i> >0 and <i>Abndisx</i> is above zero, and zero otherwise.
<i>Abnprod_Beat</i>	Indicator equal to one if a firm's <i>Abn ROA</i> >0 and <i>Abnprod</i> is above zero, and zero otherwise.
<i>RM_Beat</i>	Indicator equal to one if a firm's <i>Abn ROA</i> >0 and <i>RM</i> is above zero, and zero otherwise.
<i>E-Index</i>	Indicator equal to one if firms' Entrenchment index is above median and zero otherwise.
<i>D(t=-2)</i>	Indicator equal to one if the year is 1997 and zero otherwise.
<i>D(t=-1)</i>	Indicator equal to one if the year is 1998 and zero otherwise.
<i>D(t=0)</i>	Indicator equal to one if the year is 1999 and zero otherwise.
<i>D(t=1)</i>	Indicator equal to one if the year is 2000 and zero otherwise.
<i>D(t>=2)</i>	Indicator equal to one if the year is 2001 onwards and zero otherwise.
<i>UD</i>	Indicator variable for a "UD state"; that is, equal to one if a state eventually passes UD law for a firm in that state, and zero otherwise.
<i>Post(UD)</i>	Indicator variable for the post-UD-law-passage-period for a UD state; that is, equal to one for the period after the UD state passes UD law for a firm in that state, and zero otherwise.

Table 1 Descriptive Statistics

The sample period is from 1995 to 2002 (4 years around the year of the *Ninth Circuit* court ruling). We exclude the financial and utility industries (sic 4000-4999 and sic 6000-6999). We exclude penny stocks (share price less than one dollar) and firms with missing state of headquarter location. All variables are defined in Appendix A.

	N	Mean	Median	Std	P25	P75
<i>Abndisx</i>	20706	0.000	0.016	0.192	-0.041	0.056
<i>Abnprod</i>	19194	0.003	0.015	0.138	-0.038	0.059
<i>RM</i>	19177	0.002	0.025	0.236	-0.053	0.086
<i>Ninth Circuit</i>	20706	0.181	0.000	0.385	0.000	0.000
<i>ROA</i>	20706	0.082	0.130	0.324	0.048	0.203
<i>Total Assets(Million)</i>	20706	1053.6	153.9	2833.2	39.2	632.6
<i>Dividend</i>	20706	0.007	0.000	0.017	0.000	0.005
<i>CAPEX</i>	20706	0.064	0.044	0.068	0.023	0.078
<i>Leverage</i>	20706	0.254	0.209	0.260	0.036	0.379
<i>Cash</i>	20706	0.156	0.063	0.205	0.018	0.214
<i>Debt Issue</i>	20706	0.016	0.000	0.102	-0.016	0.031
<i>Repurchase</i>	20706	0.015	0.000	0.068	0.000	0.011
<i>IO</i>	20706	0.333	0.292	0.274	0.072	0.562
<i>GDP Growth</i>	20706	4.449	3.090	4.579	-0.042	7.850
<i>Unemployment</i>	20706	0.029	0.029	0.003	0.026	0.032
<i>Income</i>	20706	16.401	13.378	12.778	6.589	21.700
<i>Population</i>	20706	2.940	2.977	0.824	2.376	3.631
<i>Gender</i>	20706	0.504	0.506	0.008	0.499	0.510
<i>Ethnicity</i>	20706	0.844	0.871	0.086	0.772	0.926
<i>Education</i>	20706	0.215	0.216	0.036	0.194	0.233
<i>Vote Democrat</i>	20706	0.447	0.444	0.066	0.415	0.478

Table 2 – Univariate Tests

The sample period is from 1995 to 2002 (4 years around the year of the *Ninth Circuit* court ruling). We exclude the financial and utility industries (sic 4000-4999 and sic 6000-6999). We exclude penny stocks (share price less than one dollar) and firms with missing state of headquarter location. This table reports the univariate comparison between firms located in Ninth Circuit States and those in other circuit states. All variables are defined in Appendix A.

	Panel A Ninth Circuit States				Panel B Other Circuit States			Panel C Diff-in-Diff	
	Before	After	p-value		Before	After	p-value	diff-in-diff	p-value
<i>Abndisx</i>	0.001	0.036	0.000	<i>Abndisx</i>	0.009	-0.018	0.000	0.061	0.000
<i>Abnprod</i>	0.000	0.013	0.019	<i>Abnprod</i>	0.000	0.006	0.007	0.007	0.094
<i>RM</i>	-0.005	0.034	0.000	<i>RM</i>	0.009	-0.013	0.000	0.060	0.000

Table 3 Earnings Management

The sample period is from 1995 to 2002 (4 years around the year of the *Ninth Circuit* court ruling). We exclude the financial and utility industries (sic 4000-4999 and sic 6000-6999). We exclude penny stocks (share price less than one dollar) and firms with missing state of headquarter location. All variables are defined in Appendix A. T-statistics are presented beneath the coefficients within parentheses. *, ** and *** denote two-tailed significance level at 10%, 5% and 1% respectively. Standard errors are corrected for heteroscedasticity and are clustered at state of location level.

	(1)	(2)	(3)
	<i>Abndisx</i>	<i>Abnprod</i>	<i>RM</i>
<i>Post*Ninth Circuit</i>	0.029 (5.43)***	0.011 (2.00)*	0.045 (6.39)***
<i>ROA</i>	0.209 (5.11)***	-0.162 (-5.64)***	0.002 (0.03)
<i>Firm Size</i>	0.019 (1.68)*	0.019 (2.24)**	0.040 (2.23)**
<i>Dividend</i>	0.127 (0.99)	0.223 (1.89)*	0.403 (2.51)**
<i>CAPEX</i>	0.020 (0.38)	0.060 (1.77)*	0.063 (0.95)
<i>Leverage</i>	0.058 (1.60)	-0.057 (-2.31)**	-0.002 (-0.03)
<i>Cash</i>	-0.003 (-0.10)	-0.068 (-3.10)***	-0.049 (-1.35)
<i>Debt Issue</i>	-0.156 (-7.03)***	0.095 (4.90)***	-0.046 (-1.83)*
<i>Repurchase</i>	0.063 (1.63)	0.049 (1.17)	0.119 (2.45)**
<i>IO</i>	-0.054 (-2.61)**	-0.021 (-1.79)*	-0.050 (-2.74)***
<i>GDP Growth</i>	-0.001 (-0.14)	0.002 (1.18)	-0.001 (-0.32)
<i>Unemployment</i>	1.831 (0.64)	-0.936 (-0.57)	0.531 (0.18)
<i>Income</i>	0.001 (1.40)	-0.001 (-0.86)	0.001 (0.53)
<i>Population</i>	0.014 (0.39)	-0.000 (-0.02)	0.004 (0.17)
<i>Gender</i>	-0.916 (-0.54)	-0.231 (-0.15)	-1.154 (-0.52)
<i>Ethnicity</i>	-0.291 (-1.62)	0.272 (1.89)*	0.083 (0.38)
<i>Education</i>	0.876 (2.09)**	-0.075 (-0.23)	0.680 (1.25)
<i>Vote Democrat</i>	0.119 (0.60)	-0.012 (-0.12)	0.227 (1.15)
Observations	20,706	20,260	19,177
R-squared	0.415	0.351	0.428
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Table 4 Parallel Trends Analysis

The sample period is from 1995 to 2002 (4 years around the year of the *Ninth Circuit* court ruling). We exclude the financial and utility industries (sic 4000-4999 and sic 6000-6999). We exclude penny stocks (share price less than one dollar) and firms with missing state of headquarter location. All variables are defined in Appendix A. T-statistics are presented beneath the coefficients within parentheses. *, ** and *** denote two-tailed significance level at 10%, 5% and 1% respectively. Standard errors are corrected for heteroscedasticity and are clustered at state of location level.

	(1) <i>Abndisx</i>	(2) <i>Abnprod</i>	(3) <i>RM</i>
<i>D(t=-2)*Ninth Circuit</i>	0.012 (1.29)	-0.005 (-0.72)	-0.010 (-0.73)
<i>D(t=-1)*Ninth Circuit</i>	0.004 (0.30)	0.001 (0.08)	-0.005 (-0.28)
<i>D(t=0)*Ninth Circuit</i>	0.022 (2.10)**	0.008 (1.02)	0.031 (2.36)**
<i>D(t=1)*Ninth Circuit</i>	0.064 (3.62)***	0.019 (1.76)*	0.065 (2.66)**
<i>D(t>=2)*Ninth Circuit</i>	0.024 (2.85)***	0.005 (0.68)	0.034 (2.52)**
<i>ROA</i>	0.210 (5.11)***	-0.162 (-5.63)***	0.002 (0.03)
<i>Firm Size</i>	0.018 (1.67)	0.019 (2.24)**	0.040 (2.23)**
<i>Dividend</i>	0.117 (0.90)	0.223 (1.89)*	0.403 (2.53)**
<i>CAPEX</i>	0.018 (0.34)	0.059 (1.74)*	0.062 (0.94)
<i>Leverage</i>	0.059 (1.60)	-0.057 (-2.29)**	-0.001 (-0.02)
<i>Cash</i>	-0.002 (-0.06)	-0.068 (-3.04)***	-0.048 (-1.30)
<i>Debt Issue</i>	-0.157 (-7.02)***	0.095 (4.89)***	-0.047 (-1.83)*
<i>Repurchase</i>	0.063 (1.62)	0.049 (1.17)	0.119 (2.45)**
<i>IO</i>	-0.053 (-2.64)**	-0.021 (-1.80)*	-0.049 (-2.76)***
<i>GDP Growth</i>	-0.001 (-0.15)	0.002 (1.14)	-0.001 (-0.38)
<i>Unemployment</i>	1.188 (0.45)	-1.072 (-0.69)	0.438 (0.15)
<i>Income</i>	0.002 (1.46)	-0.001 (-0.87)	0.001 (0.46)
<i>Population</i>	0.014 (0.42)	0.000 (0.02)	0.005 (0.18)
<i>Gender</i>	-1.091 (-0.64)	-0.247 (-0.16)	-1.164 (-0.51)
<i>Ethnicity</i>	-0.292 (-1.64)	0.272 (1.88)*	0.084 (0.38)
<i>Education</i>	0.939 (2.19)**	-0.078 (-0.25)	0.656 (1.22)
<i>Vote Democrat</i>	0.147 (0.76)	-0.005 (-0.05)	0.231 (1.13)
Observations	20,706	20,260	19,177
R-squared	0.416	0.351	0.428
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Table 5 Robustness Tests

The sample period is from 1995 to 2002 (4 years around the year of the *Ninth Circuit* court ruling). We exclude the financial and utility industries (sic 4000-4999 and sic 6000-6999). We exclude penny stocks (share price less than one dollar) and firms with missing state of headquarter location. Panel A re-estimates Eq (1) excluding high-tech industries. High tech industries are defined as those industries with 2-digit SIC of 28, 35, 36, 37, 38 and 73. Panel B re-estimates Eq (1) excluding firms with negative compounded returns during 2000-2002 period. Panel C presents the results of adjacent-county analysis in which we restrict our sample to firms located within 60 miles of the Ninth Circuit state borders. All regressions contain same set of control variables as per Table 3. All variables are defined in Appendix A. T-statistics are presented beneath the coefficients within parentheses. *, ** and *** denote two-tailed significance level at 10%, 5% and 1% respectively. Standard errors are corrected for heteroscedasticity and are clustered at state of location level.

Panel A Exclude High Tech Industries			
	(1)	(2)	(3)
	<i>Abndisx</i>	<i>Abnprod</i>	<i>RM</i>
<i>Post*Ninth Circuit</i>	0.028 (4.94)***	0.016 (1.88)*	0.048 (5.39)***
Controls	Yes	Yes	Yes
Observations	11,140	10,795	10,419
R-squared	0.446	0.312	0.396
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Panel B Exclude Firms with Negative Returns			
	(1)	(2)	(3)
	<i>Abndisx</i>	<i>Abnprod</i>	<i>RM</i>
<i>Post*Ninth Circuit</i>	0.017 (2.60)**	0.033 (4.14)***	0.062 (3.92)***
Controls	Yes	Yes	Yes
Observations	5,422	5,166	5,005
R-squared	0.452	0.319	0.387
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Panel C Adjacent County Analysis			
	(1)	(2)	(3)
	<i>Abndisx</i>	<i>Abnprod</i>	<i>RM</i>
<i>Post*Ninth Circuit</i>	0.088 (2.83)***	0.036 (2.21)**	0.101 (3.49)***
Controls	Yes	Yes	Yes
Observations	15,146	13,472	12,658
R-squared	0.595	0.593	0.609
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Table 6 Propensity Score Matching

The sample period is from 1995 to 2002 (4 years around the year of the *Ninth Circuit* court ruling). We exclude the financial and utility industries (sic 4000-4999 and sic 6000-6999). We exclude penny stocks (share price less than one dollar) and firms with missing state of headquarter location. Panel A provides univariate comparison of the propensity score matched sample. Panel B estimates Eq. (1) using propensity score matched sample. All regressions contain same set of control variables as per Table 3. All variables are defined in Appendix A. T-statistics are presented beneath the coefficients within parentheses. *, ** and *** denote two-tailed significance level at 10%, 5% and 1% respectively. Standard errors are corrected for heteroscedasticity and are clustered at state of location level.

Panel A Univariate Comparison			
	Control	Treated	p-value
<i>Abndisx</i>	-0.017	0.007	0.000
<i>Abnprod</i>	0.000	0.027	0.000
<i>RM</i>	-0.017	0.034	0.000
<i>PSM Score</i>	0.209	0.206	0.154
<i>ROA</i>	0.057	0.059	0.841
<i>Firm Size</i>	4.742	4.781	0.429
<i>Dividend</i>	0.004	0.004	0.431
<i>CAPEX</i>	0.066	0.068	0.240
<i>Leverage</i>	0.216	0.225	0.218
<i>Cash</i>	0.227	0.213	0.129
<i>Debt Issue</i>	0.014	0.018	0.172
<i>Repurchase</i>	0.014	0.014	0.824
<i>IO</i>	0.311	0.314	0.628
Panel B Multivariate Regression			
	(1)	(2)	(3)
	<i>Abndisx</i>	<i>Abnprod</i>	<i>RM</i>
<i>Post*Ninth Circuit</i>	0.047 (2.37)**	0.024 (1.73)*	0.073 (3.20)***
Controls	Yes	Yes	Yes
Observations	6,054	6,054	6,054
R-squared	0.717	0.714	0.717
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Table 7 Managerial Entrenchment and Institutional Ownership

The sample period is from 1995 to 2002 (4 years around the year of the *Ninth Circuit* court ruling). We exclude the financial and utility industries (sic 4000-4999 and sic 6000-6999). We exclude penny stocks (share price less than one dollar) and firms with missing state of headquarter location. All regressions contain same set of control variables as per Table 3. All variables are defined in Appendix A. T-statistics are presented beneath the coefficients within parentheses. *, ** and *** denote two-tailed significance level at 10%, 5% and 1% respectively. Standard errors are corrected for heteroscedasticity and are clustered at state of location level.

Panel A Managerial Entrenchment			
	(1)	(2)	(3)
	<i>Abndisx</i>	<i>Abnprod</i>	<i>RM</i>
<i>Post*Ninth Circuit</i>	0.027 (4.30)***	0.007 (1.07)	0.014 (1.91)*
<i>Post*Ninth Circuit*E-Index</i>	0.023 (2.04)**	0.050 (2.69)***	0.065 (2.97)***
<i>Post*E-Index</i>	0.009 (1.30)	-0.009 (-0.95)	0.001 (0.08)
<i>Ninth Circuit*E-Index</i>	-0.027 (-1.91)*	0.020 (1.62)	-0.005 (-0.18)
<i>E-Index</i>	-0.009 (-0.98)	-0.003 (-0.30)	-0.017 (-1.09)
Controls	Yes	Yes	Yes
Observations	5,444	5,345	5,232
R-squared	0.455	0.307	0.420
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Panel B Institutional Ownership			
	(1)	(2)	(3)
	<i>Abndisx</i>	<i>Abnprod</i>	<i>RM</i>
<i>Post*Ninth Circuit</i>	0.045 (4.53)***	0.029 (2.99)***	0.083 (6.37)***
<i>Post*Ninth Circuit*IO</i>	-0.056 (-2.73)***	-0.053 (-2.87)***	-0.124 (-3.93)***
<i>Post*IO</i>	0.048 (5.21)***	0.002 (0.25)	0.036 (2.79)***
<i>Ninth Circuit*IO</i>	0.036 (1.33)	0.038 (1.18)	0.095 (1.88)*
<i>IO</i>	-0.081 (-3.44)***	-0.022 (-1.72)*	-0.073 (-3.80)***
Controls	Yes	Yes	Yes
Observations	20,706	20,260	19,177
R-squared	0.416	0.351	0.429
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Table 8 Meet and Beat

The sample period is from 1995 to 2002 (4 years around the year of the *Ninth Circuit* court ruling). We exclude the financial and utility industries (sic 4000-4999 and sic 6000-6999). We exclude penny stocks (share price less than one dollar) and firms with missing state of headquarter location. All regressions contain same set of control variables as per Table 3. All variables are defined in Appendix A. T-statistics are presented beneath the coefficients within parentheses. *, ** and *** denote two-tailed significance level at 10%, 5% and 1% respectively. Standard errors are corrected for heteroscedasticity and are clustered at state of location level.

Panel A Meet and Beat			
	(1)	(2)	(3)
	<i>Abndisx Beat</i>	<i>Abnprod Beat</i>	<i>RM Beat</i>
<i>Post*Ninth Circuit</i>	0.745 (11.85)***	0.302 (3.31)***	0.544 (8.60)***
<i>Ninth Circuit</i>	-0.144 (-1.93)*	0.010 (0.13)	-0.138 (-2.20)**
Controls	Yes	Yes	Yes
Observations	20,421	20,068	19,098
R-squared	0.08	0.035	0.065
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Panel B Meet and Beat-Managerial Entrenchment			
	(1)	(2)	(3)
	<i>Abndisx Beat</i>	<i>Abnprod Beat</i>	<i>RM Beat</i>
<i>Post*Ninth Circuit</i>	0.776 (4.17)***	0.396 (3.94)***	0.796 (5.12)***
<i>Post*Ninth Circuit*E-Index</i>	1.649 (2.86)***	1.882 (5.31)***	1.578 (4.14)***
<i>Post*E-Index</i>	0.339 (1.76)*	-0.036 (-0.21)	0.177 (0.99)
<i>Ninth Circuit*E-Index</i>	0.145 (0.55)	0.026 (0.10)	-0.189 (-0.79)
<i>E-Index</i>	-0.136 (-1.44)	0.031 (0.36)	-0.065 (-0.63)
<i>Ninth Circuit</i>	-0.192 (-1.10)	0.051 (0.32)	-0.099 (-0.69)
Controls	Yes	Yes	Yes
Observations	5,397	5,312	5,222
R-squared	0.185	0.0821	0.145
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Panel C Meet and Beat-Institutional Ownership			
	(1)	(2)	(3)
	<i>Abndisx Beat</i>	<i>Abnprod Beat</i>	<i>RM Beat</i>
<i>Post*Ninth Circuit</i>	1.013 (12.77)***	0.442 (5.73)***	0.725 (9.73)***
<i>Post*Ninth Circuit*IO</i>	-0.827 (-4.43)***	-0.411 (-1.72)*	-0.554 (-3.02)***
<i>Post*IO</i>	0.180 (1.19)	-0.253 (-1.93)*	0.003 (0.02)
<i>Ninth Circuit*IO</i>	-0.013 (-0.10)	0.021 (0.08)	0.080 (0.51)
<i>IO</i>	-0.176 (-1.43)	0.033 (0.25)	-0.127 (-1.07)
<i>Ninth Circuit</i>	-0.141 (-1.64)	-0.002 (-0.03)	-0.164 (-2.33)**
Controls	Yes	Yes	Yes
Observations	20,421	20,068	19,098
R-squared	0.0799	0.0353	0.0651
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Table 9 UD Law

The sample period is from 1985 to 2009 (4 years around first and last year of the *UD* ruling). We exclude the financial and utility industries (sic 4000-4999 and sic 6000-6999). We exclude penny stocks (share price less than one dollar) and firms with missing state of incorporation. All variables are defined in Appendix A. T-statistics are presented beneath the coefficients within parentheses. *, ** and *** denote two-tailed significance level at 10%, 5% and 1% respectively. Standard errors are corrected for heteroscedasticity and are clustered at state of incorporation level.

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Abndisx</i>	<i>Abnprod</i>	<i>RM</i>	<i>Abndisx</i>	<i>Abnprod</i>	<i>RM</i>
<i>Post(UD)*UD</i>	0.011 (2.24)**	0.010 (1.87)*	0.016 (2.56)**	0.014 (1.81)*	0.009 (1.73)*	0.020 (3.72)***
<i>ROA</i>	0.152 (11.99)***	-0.141 (-19.77)***	-0.001 (-0.13)	0.200 (8.86)***	-0.143 (-13.39)***	0.038 (1.68)*
<i>Firm Size</i>	0.009 (2.40)**	0.018 (4.55)***	0.025 (9.19)***	0.020 (4.56)***	0.015 (5.13)***	0.032 (6.17)***
<i>Dividend</i>	0.040 (0.77)	0.135 (3.74)***	0.156 (3.65)***	0.047 (0.56)	0.108 (2.83)***	0.141 (1.90)*
<i>CAPEX</i>	0.004 (0.14)	0.031 (2.40)**	0.017 (0.62)	0.053 (1.38)	0.033 (1.62)	0.059 (1.59)
<i>Leverage</i>	0.047 (4.99)***	-0.047 (-6.62)***	-0.001 (-0.05)	0.053 (3.28)***	-0.049 (-4.99)***	0.006 (0.23)
<i>Cash</i>	0.021 (1.68)*	-0.039 (-6.80)***	-0.040 (-2.99)***	0.049 (3.19)***	-0.052 (-6.72)***	-0.031 (-1.92)*
<i>Debt Issue</i>	-0.146 (-10.12)***	0.080 (8.87)***	-0.041 (-2.33)**	-0.167 (-7.86)***	0.094 (8.42)***	-0.061 (-2.47)**
<i>Repurchase</i>	0.023 (1.48)	0.027 (2.32)**	0.032 (1.48)	0.034 (1.59)	0.042 (3.24)***	0.057 (2.11)**
<i>IO</i>	-0.017 (-3.09)***	-0.009 (-1.73)*	-0.018 (-4.34)***	-0.036 (-5.37)***	0.003 (0.57)	-0.015 (-2.26)**
<i>GDP Growth</i>				0.001 (0.80)	0.001 (0.73)	0.001 (0.90)
<i>Unemployment</i>				3.686 (4.73)***	1.016 (1.76)*	3.731 (3.74)***
<i>Income</i>				-0.000 (-0.92)	-0.000 (-0.60)	-0.000 (-0.27)
<i>Population</i>				-0.013 (-2.42)**	-0.006 (-1.25)	-0.012 (-1.74)*
<i>Gender</i>				-0.472 (-0.59)	0.109 (0.22)	0.193 (0.25)
<i>Ethnicity</i>				-0.124 (-3.15)***	0.075 (2.46)**	-0.038 (-0.60)
<i>Education</i>				0.310 (1.68)*	-0.290 (-1.65)	0.063 (0.50)
<i>Vote Democrat</i>				-0.096 (-1.59)	-0.077 (-1.45)	-0.080 (-1.37)
Observations	63,808	61,941	59,911	37,344	36,423	34,929
R-squared	0.096	0.078	0.052	0.173	0.139	0.127
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes