Enforceability of non-compete covenants, discretionary investments, and financial reporting practices: Evidence from a natural experiment*

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ABSTRACT

Non-compete covenants are widely used in employment contracts to promote employee stability. Using legal amendments of non-compete enforceability as a natural experiment, we find that as non-compete enforceability increases, firms display an increased likelihood of meeting short-term earnings benchmarks, lower discretionary expenditures, and declining future performance. These effects are more pronounced when CEOs have lower ability or shorter tenures, and when firms have more growth opportunities or operate in localized industries. Our results suggest that managers actively adapt investment and financial reporting practices to the changing environment that affects their contractual relations with firms.

JEL classification: D22; D86; G30; K12; M41
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1. Introduction

Contract enforceability is vital in shaping contractual relations and influencing the behavior of contracting parties. This study explores how exogenously increased enforceability of non-compete clauses in employment contracts affects managers' incentives and behavior. In the U.S., non-compete clauses are widely used to forbid employees—particularly high-level executives and specialized technicians—to undertake jobs with competing firms following termination of current employment, and are regarded as one of the most useful mechanisms to safeguard employers' proprietary information and investment in human capital (Gilson, 1999).[1] Since non-compete clauses constrain opportunities for future employment and thus increase the cost of dismissal and job switching for managers, we expect them to have a significant impact on managers' investment and financial reporting practices (Graham et al., 2005).

The study makes use of a quasi-experiment, whereby three states (Texas, Florida, and Louisiana) amended non-compete enforceability between 1992 and 2004, to examine how constraints in the labor market affect managers' investment and reporting choices. As Garmaise (2011) indicates, these state-level legal changes address potential endogeneity problems in that they are initiated by state governments or court rulings and are not driven by firm- or manager-specific factors. An assumption maintained in this study, as in prior research (e.g., Garmaise, 2011), is that

[1] Garmaise (2011) finds that of the 500 firms randomly drawn from Execucomp, 70.2 percent have non-compete agreements with upper-level managers. Likewise, Bishara et al. (2015) find that 80 percent of the 874 CEO employment contracts from S&P 1500 firms, initiated between 1996 and 2010, contain such provisions. The actual numbers could be higher since firms are not required by regulation to disclose such agreements. Companies do indeed enforce non-competes against departing employees. One example is Microsoft's lawsuit against Kai-Fu Lee and Google in 2005, in which Microsoft asserted that Google's recruitment of Lee violated the non-compete clause in his employment contract with Microsoft. Another is Capital One Financial Corp's lawsuit against John Kanas and John Bohlsen (former employees who joined BankUnited, a Florida-based bank, as the CEO and the Chief Lending Officer in 2009) when they attempted to acquire New York-based Herald Nation Bank for BankUnited in 2011. Capital One asserted that Kanas and Bohlsen violated the geographical restriction in their non-compete clauses.

contracts require external institutions such as laws and courts to enforce. As the level of enforceability changes, the intended equilibrium under the original contact is no longer optimal, leading to adjustments in managers' behavior (as well as in contractual terms).

We first posit that tighter non-compete enforceability makes managers more attentive to accounting earnings that they report. Increased non-compete enforceability inhibits managers' mobility, thereby reducing their opportunities in the labor market. When facing greater career concerns, managers would make more efforts to avoid reporting poor performance (e.g., missing earnings targets) that could cause them to be dismissed from their jobs (e.g., Stein, 1989; Matsunaga and Park, 2001). By achieving performance targets, managers not only make their current jobs more secure but also enhance potential employment opportunities in industries not subject to the non-compete provisions (Brickley et al., 1999). Consistent with our prediction, we find that subsequent to increased non-compete enforceability, firms display an increased likelihood of meeting or beating earnings targets.

Next, we examine whether managers adjust discretionary expenditures as a means to attain performance targets. We envisage two possible reasons for managers doing so. One is managerial myopia. Strict enforcement of non-competes causes managers to place a greater emphasis on short-term performance. To boost reported earnings, managers may cut R&D, advertising, and other SG&A expenses, even to the extent of sacrificing long-term profitability, which is a form of real earnings management (Stein, 1988, 1989; Roychowdhury, 2006; Bhojraj et al., 2009).[2] The other reason is interest alignment; that is, when faced with higher costs of dismissal, managers are under greater pressure to work hard and make efficient use of resources.

[2] While we focus on discretionary expenses in the main tests, we also consider other forms of earnings management in supplementary analysis such as over-production and accounting accruals.

In this latter scenario, managers may also cut discretionary expenditures, but the aim is to eliminate wasteful projects and increase efficiency rather than to boost reported earnings per se.

In both scenarios above, firms are expected to reduce discretionary expenses as a way to meet earnings targets when non-competes become more strictly enforced. Consistent with our prediction, we find that upon tightened enforcement, firms significantly reduce the level of R&D, advertising, and other discretionary expenses.

The two reasons for cutting discretionary spending, while having similar implications for current earnings, lead to sharply differing consequences for future firm performance. In the managerial myopia scenario, cutting back on discretionary spending is suboptimal and is detrimental to innovation and long-term profitability. In contrast, in the interest-alignment scenario, restrictions on external employment opportunities act as a threat that propels managers to work more diligently and spend resources more efficiently; this helps to reduce agency costs and increase firm value.

To distinguish between the alternative forces that might be at work, we next examine future firm performance, and find the following results. First, as the enforceability of non-competes increases, cutting discretionary expenses leads to declining performance at the operational level. More specifically, we find that reductions in R&D expenses lead to lower innovation output (in terms of the number of patents granted, patent scopes, and citations per patent), reductions in advertising expenses lead to reduced market shares, and ROE goes down following reductions in (other) SG&A expenses. Second, cutting discretionary expenses also adversely affects future stock returns. On the whole, our results are more supportive of the notion that an exogenous increase in non-compete enforceability engenders myopic reporting through reducing discretionary expenditures, which sacrifices future firm performance,

rather than the notion that it fosters closer interest alignment and reduces agency costs. In supplementary analysis, we find corroborative evidence that firms also manage earnings through other real activities (e.g., overproduction) and, to a lesser extent, accrual manipulation.

In the cross-section, the effect of non-compete enforceability is more pronounced for firms that have more growth opportunities and firms that operate in more localized industries, consistent with the conjecture that non-competes are more binding on executives who face a heavier punishment from the stock market for failing to meet earnings targets or have more difficulties in finding out-of-state jobs. We also find that CEOs of lower ability and those with shorter tenures with their current employers react more strongly to increased non-compete enforcement, suggesting that non-competes are more costly for managers facing more limited external opportunities and managers who have yet to establish a personal reputation.

Our study contributes to better understanding the role of the contracting environment in influencing firms' investment and reporting practices. Firms make optimal contractual arrangements with managers given the level of enforceability. As the contract enforceability changes, the equilibrium outcome under the original contract is no longer optimal, causing the contracting parties to adjust their actions. In our specific setting, when legal amendments cause non-compete clauses to be more enforceable, managers display a greater tendency to meet earnings targets by cutting discretionary expenditures. Our study suggests that financial reporting practices are influenced not only by the explicit terms governing employment relations, which has been the focus of prior research, but also by the legal framework within which these terms are enforced. This message, from a contracting perspective, echoes that of Ball et al. (2003), who emphasize the roles of institutions as well as accounting standards

in determining financial reporting quality. In this regard, our study demonstrates a specific channel through which institutional factors can alter reported earnings.

Relatedly, the study also yields insights into how labor market opportunities influence financial reporting quality. Besides managerial incentives that stem from compensation contracts, labor market mobility has been proposed as another factor driving earnings manipulation, but empirical evidence has been lacking (Graham et al., 2005). Our study is among the first to provide evidence in support of this view.

Finally, this study also affords a better understanding of the economic consequences of non-compete covenants. Though non-competes are widely adopted in employment contracts to protect employers' intellectual property and human capital (Gilson, 1999), our findings suggest that such benefits are not without cost. More specifically, while managers facing severe labor market constraints might exert more efforts to avoid being dismissed, they typically resort to under-investment in discretionary expenditures as an important means to enhance short-term performance. This tactic is harmful to a firm's innovation output and long-term profitability. Although we cannot rule out that labor market restrictions also help to enhance managerial effort and encourage efficient use of resources, our overall evidence points to managerial myopia being the more dominant force at work. This, together with the prior finding that managers shy away from firm-specific capital investments (Garmaise, 2011), suggests that the net benefits of strict non-compete enforcement for firms is actually unclear.

The rest of the paper proceeds as follows. Section 2 provides background on non-compete covenants and develops our hypotheses. Section 3 discusses the sample. Section 4 describes our research design and presents the empirical results. Section 5 provides the robustness tests. Finally, we conclude in Section 6.

2. Hypothesis development

On the premise that individuals act rationally to advance self-interest, we postulate that shareholders (through the board of directors) offer an employment contract acceptable to managers that maximizes their own benefits. In classical contracting theory (e.g., Holmstrom, 1979), contractual terms are clearly specified with respect to all observable and verifiable states of the world, and there is no ambiguity about *ex post* enforcement—that is, once accepted by the contracting parties, a contract is assumed to be fully implemented and enforced.

In the real world, however, contracts often are incomplete (Hart and Moore, 1999),[3] and enforcement of contractual terms can be ambiguous, depending on legal institutions such as laws and courts (Djankov et al., 2003). This gives rise to the role of institutions in influencing economic activities. For example, in an environment of low enforceability, more incentives would have to be provided in order to motivate managers and bring their interests closer to those of investors. As enforceability increases, the original equilibrium is no longer optimal and the contractual arrangements would need to be adjusted. Thus, in response to increased enforceability, we expect to observe adjustments, both in the structure of employment contracts designed ex ante, and in the subsequent managerial behavior in fulfilling contractual obligations. In this study, we examine how exogenous changes in the enforceability of non-compete provisions affect managers' investment and financial reporting decisions.

In the U.S., employment contracts are governed by state laws, and the enforceability of non-compete provisions varies across states. For instance, while

[3] Contracts are incomplete for various reasons such as enforcement costs, writing costs, and unforeseen contingencies (Tirole, 1999).

enforceable at least to some extent in most states, these clauses are deemed legally void in California and North Dakota. State laws governing non-compete enforceability also change over time. For example, Florida tightened the enforceability of non-competes in 1996 following a legislative act (Fla. Sess. Law Serv. Ch. 96-257). On the other hand, Texas loosened enforceability after the Texas Supreme Court's ruling on Light v. Cellular Co. (Texas, 1994). Finally, the Louisiana Supreme Court decreased non-compete enforceability in SWAT 24 Shreveport Bossier v. Board in June 2001, and then the state legislature increased non-compete enforceability in 2003 (La. Sess. Law Serv. Act. 428). These exogenous changes provide a setting within which to explore how firms' financial reporting and investment decisions respond to the enforceability of non-compete provisions.[4] We develop our hypotheses below.

2.1. Non-compete covenants and the likelihood of firms' meeting earnings benchmarks

CEOs and CFOs should expect that the non-compete provisions in their employment contracts will have an effect when they leave current employers, voluntarily or otherwise. Based on a sample of 100 randomly selected firms in the S&P 500 index (as of January 1, 2010) that have explicit employment contracts with their CEOs,[5] we find that a majority (81%) explicitly state that the provision can be triggered when the CEO is terminated for any reason, including termination without cause, and the courts typically enforce non-competes when the discharged employee

[4] Across US states with varying non-compete enforceability, non-competes are found to promote employee stability (Marx et al., 2009; Garmaise, 2011) and facilitate M&A activities (Younge et al., 2015), but discourage employees from making firm-specific investments such as R&D (Garmaise, 2011; Amir and Lobel, 2011) and impede entrepreneurial activities (Stuart and Sorenson, 2003; Samila and Sorenson, 2011).

[5] We find that two years is the most common restriction in a CEO's non-compete agreement (consistent with Schwab and Thomas, 2006). Non-competes of CEOs typically contain no geographical specification, which significantly restricts their post-employment mobility (Gilson, 1999). See, for example, the contract between Harris Corporation (headquartered in Florida) and its CEO, Howard L. Lance.

receives monetary compensation for his/her dismissal (Vanko, 2002).[6] In practice, for high-ranking executives such as CEOs or CFOs (who are our focus in this study), terminations in bad faith or terminations without monetary compensation are rare (Yermack, 2006).

Managers have stronger incentives to boost short-term performance when their careers are at stake (Narayanan, 1985; Stein, 1988, 1989). As the enforceability of non-competes is strengthened, external employment opportunities become more restricted. As a result, managers would make greater efforts to achieve performance targets and keep their current jobs secure. Even if managers plan to switch jobs, attaining performance targets would help to enhance their opportunities in industries not subject to the non-compete provisions (Kaplan and Reishus, 1990; Brickley et al., 1999; Graham et al., 2005; Evans et al., 2010). Thus, we have the following hypothesis about the impact of non-compete enforceability on earnings performance.

H1. Increased non-compete enforceability is associated with a higher likelihood of firms meeting earnings benchmarks.

2.2. Non-compete covenants and investments in discretionary expenditures

In responding to stricter legal enforceability of non-competes, managers are likely to cut down on discretionary expenses. We envisage two possible reasons for managers to do so. The first is interest alignment; namely, tightened enforcement would better align interests between managers and their incumbent employers and

[6] In the case of "*Twenty-Four Collection, Inc. v. Keller*" (Florida, 1980), the Florida court specifically indicated that involuntary discharge is not a factor for consideration in determining the enforceability of non-competes (Malsberger, 2004). A few examples from Vanko (2002) include *Cellular One, Inc. versus John Brent Boyd versus Hamilton J. Lemoine* (Louisiana, 1995), *Gartner Group Inc. versus Mewes* (Connecticut, 1992), and *Cockerill versus Wilson* (Illinois, 1972).

hence reduce agency costs.[7] With fewer outside opportunities becoming available, managers are more bound to their current employers (Garmaise, 2011), and so they are compelled to work harder to increase performance. This suggests that managers should exercise more prudence in allocating resources. Following this argument, we expect that managers become less wasteful in spending resources (such as R&D and advertising expenditures) and are less prone to undertaking projects that are aimed to advance their personal interests (such as empire-building). Consequently, the overall level of discretionary expenditures should fall.

The second reason is managerial myopia. Instead of spending resources efficiently to genuinely improve firm performance, managers might undertake suboptimal investment decisions in order to boost short-term earnings. It is well known that when true performance is lacking, managers have incentives to increase reported performance by altering business transactions (Dechow and Sloan, 1991; Bhojraj et al., 2009; Kothari et al., 2016). As explained above, increased enforceability of non-compete clauses leads to diminished external employment opportunities for managers, which would shift managerial emphasis toward meeting short-term earnings targets. In this context, cutting discretionary expenses is particularly effective because these expenses are easily adjusted by managers. In contrast to the scenario of interest alignment, in cutting discretionary expenses aimed at raising current earnings, managers are now sacrificing future firm profits.

Whichever scenario prevails, however, we have similar predictions on how non-compete enforcement affects discretionary expenditures.[8]

[7] We thank the referee for pointing out this potential effect of non-compete clauses.

[8] Which scenario would prevail depends on the cost of writing strict contracts. To the extent that the cost of writing a tighter contract is high, one might expect the second scenario (managerial myopia) to be more likely when enforceability is increased, because otherwise a tighter contract could have been designed before the enforceability increase (we thank the referee for this point).

H2. Managers reduce discretionary expenses in response to increased non-compete enforceability.

2.3. The impact of non-compete covenants on the efficiency of discretionary expenditures and future performance

While the two motives for cutting expenditures both help to increase current earnings, they have dissimilar consequences for future performance. If managers' true intention is to enhance resource utilization when faced with increased non-compete enforceability, we would then expect firms to improve efficiency and increase profitability in the years following reduced investments.

In contrast, if reductions in discretionary expenses are driven by managerial myopia and earnings manipulation simply to meet the performance target, firm performance is expected to decline due to under-investment. In this scenario, tightened enforceability of non-competes increases managers' own career concerns, which causes them to alter investment and reporting practices in a way that is harmful to firms' long-term profit.[9] This leads to the prediction that stricter non-compete enforcement results in deteriorating efficiency with respect to discretionary expenditures and declining financial performance.

Which motive for cutting discretionary spending would dominate is an open question. We therefore state the two alternative predictions as follows:

H3a. Following the interest alignment argument, increased non-compete

[9] A maintained assumption here is that contracting alone does not fully eliminate investment inefficiencies (Myers and Majluf, 1984; Persons, 1994), and a shift in contract enforceability causes adjustments both in contract design (as shown in Garmaise, 2011) and in managers' behavior. Relatedly, Carter et al. (2009) and Cohen et al. (2013) show that the compensation contracts of CEOs and CFOs are revised with the passage of the Sarbanes Oxley Act of 2002, but nonetheless the contract revisions have not prevented managers from engaging in increased real earnings management.

enforceability is associated with greater output from discretionary expenditures and higher future profitability.

H3b. Following the managerial myopia argument, increased non-compete enforceability is associated with less output from discretionary expenditures and lower future profitability.

In testing H3a and H3b below, we consider separately three components of discretionary expenses that affect different aspects of a firm's operations—R&D, advertising, and other SG&A expenses.

2.4. The varying impacts of non-compete provisions across firms

Hypotheses H1 to H3 above outline the average effects of increased enforceability of non-competes. Next, we posit how the effects of non-compete provisions vary across firms with different types of CEOs. First, we expect CEOs employed in more localized industries to be more constrained by non-compete provisions as it is more difficult for these CEOs to find alternative employment outside the state (Garmaise 2011). Second, we predict that CEOs of lower ability could be more affected by non-compete clauses because these CEOs have fewer external opportunities and are more replaceable, rendering non-compete constraints more binding on them. Finally, we expect that CEOs with shorter tenure are more affected by non-compete provisions given that they have not established their reputation both within and outside their firms and thus have more need to rely on short-term accounting performance to prove their ability (Ali and Zhang, 2015). We formally state these hypotheses below:

H4. Increased non-compete enforceability has stronger effects on (i) CEOs employed in more localized industries; (ii) CEOs of lower ability; and (iii) CEOs with a shorter tenure.

3. Sample

Our initial sample consists of all non-financial and non-regulated companies in Compustat from the year 1992 through to the year 2004, which coincides with the period of Garmaise's (2011) enforceability index for all jurisdictional regions of the U.S. The enforceability index captures the strength of the legal enforcement of non-compete agreements, with higher values indicating stricter enforcement. Garmaise (2011) constructed this state-level enforceability index using 12 questions, proposed by Malsberger (2004), concerning the details of non-compete laws in the U.S. To ascertain a firm's jurisdictional area, we retrieve information on the location of each firm's headquarters from Compact Disclosure.[10] After excluding firms with missing data in Compustat, CRSP, I/B/E/S, or Thomson Reuters, we obtain our final sample comprising 5,244 firms with 27,964 firm-year observations.

As described in Section 2, Garmaise (2011) identifies three states that amended their non-compete enforcement policy during the period 1992-2004. First, Florida increased its enforceability of non-competes, effective from 1997. Second, Louisiana decided not to enforce non-compete clauses during the period 2002-2003 but reinstated its enforcement policy thereafter. Finally, Texas reduced the enforceability of non-compete agreements, effective from 1995. Of the 5,244 firms in our sample,

[10] We assume that a company's headquarter state is where non-compete lawsuits would be tried. We check through the CEO employment contracts of S&P 500 firms and find that 79 percent choose the state of the company's headquarters as the governing state. Nonetheless, we acknowledge that there is some controversy as to which state's law should apply when a lawsuit occurs, as shown by previous case law (e.g., *Advanced Bionics v. Medtronic* 2002; *Aspect Software Inc. v. Barnett* 2011; *In re Autonation Inc.* 2007). However, we believe that this uncertainty would add noise (while not systematic) to our tests, making it more difficult for us to obtain supportive results.

719 (13.71%) are located in one of these three states that experienced a shift in non-compete enforcement policy; they form our treatment sample. The remaining 4,525 firms constitute our control sample. Table 1 presents the enforceability index and the sample distribution across the 50 U.S. states and Washington D.C.

[Table 1 about here]

4. Main empirical results

4.1. Basic research design

We adopt the following basic regression model, used in Garmaise (2011), to test our hypotheses:

$$Y_{i,t} = \alpha_0 + \alpha_1 IncreaseEnforce_{s,t} + \beta X_{i,t} + \gamma_i + \sigma_t + \varepsilon_{i,t} \quad (1)$$

where Y is the dependent variable of interest (i.e., the probability of meeting an earnings target, a measure of cutting back on discretionary expenses, proxies for innovation output, or firm performance); our main explanatory variable, *IncreaseEnforce*, captures shifts in state laws governing the enforcement of non-compete provisions, which is set to 1 for firms in Florida over the period 1997-2004, set to -1 for firms in Texas over the period 1995-2004 and firms in Louisiana over the period 2002-2003, and set to 0 for all other firm-years; X is a vector of control variables; and γ_i and σ_t are the firm and year fixed effects, respectively. In some tests that employ a logit model (such as those concerning the likelihood of meeting earnings benchmarks), we include industry and state fixed effects instead of firm fixed effects because it is not feasible to estimate the coefficients for firms with no variation in the dependent variable. We calculate standard errors based on the Huber-White sandwich estimate of variances, adjusted for clustering by state.

4.2. The effect of increased non-compete enforceability on meeting earnings targets

H1 predicts that firms are more likely to meet earnings benchmarks when facing tighter non-compete enforceability. Following prior literature (Matsunaga and Park, 2001; Cheng and Warfield, 2005; Jiang et al., 2010; Doyle et al., 2013), we focus on firms' incentives to meet or beat the analyst consensus forecast as a target. A firm-year is classified as meeting earnings targets (*Meet*) if actual earnings are equal to or above the consensus forecast, defined as the mean of individual analysts' final forecasts issued between the beginning of the fiscal year and the earnings announcement date (Roychowdhury, 2006). We also pay particular attention to those firm-years that only slightly beat the earnings benchmarks and thus are most likely to involve managed earnings. We classify a firm-year as *Suspect* if it meets the consensus analyst forecast exactly or beats it by just one cent.

Upon identifying the "*Meet*" and "*Suspect*" firms, we estimate the following logit model, as previously employed by Bartov et al. (2002), Skinner and Sloan (2002), Cohen and Zarowin (2010), and Zang (2012):

$$\begin{aligned} Meet\ /\ Suspect_{i,t} = {} & \alpha + \beta_1 IncreaseEnforce_{s,t} + \beta_2 HabitualBeater_{i,t} \\ & + \beta_3 AnalystFollowing_{i,t-1} + \beta_4 StockIssue_{i,t+1} \\ & + \beta_5 BigAud_{i,t} + \beta_6 IO_{i,t-1} + \beta_7 CFO_{i,t-1} + \beta_8 ROA_{i,t} \\ & + \beta_9 AnnRet_{i,t} + \beta_{10} MB_{i,t-1} + \beta_{11} Shares_{i,t} + \beta_{12} TA_{i,t-1} + \gamma_i \\ & + \mu_s + \sigma_t + \varepsilon_{i,t} \end{aligned} \quad (2)$$

where *Meet*, *Suspect*, and *IncreaseEnforce* are as defined above; *HabitualBeater* is the number of times the firm met the consensus analyst forecast over the past four quarters; *AnalystFollowing* is the natural logarithm of 1 plus the number of analysts following the firm in year *t*-1; *StockIssue* is a dummy variable equal to 1 if the firm issues equity in year *t*+1, and 0 otherwise; *BigAud* is a dummy variable equal to 1 if the firm is audited by a Big 4/5/6 auditor in year *t*, and 0 otherwise; *IO* is the

percentage of the firm's shares held by institutional investors; *CFO* is cash flow from operations deflated by the total assets at the beginning of the year; *ROA* is return on assets, computed using net income for the rolling four quarters ending in the third quarter of year *t*; *AnnRet* is the monthly compounded stock return over year *t*; *MB* is the market-to-book ratio of equity; *Shares* is the natural logarithm of the number of shares outstanding; and *TA* is the natural logarithm of total assets in year *t*-1. γ_i, μ_s, and σ_t are industry, state, and year fixed effects, respectively.

Panel A of Table 2 reports the summary statistics of the variables used in equation (2): 52% (12.9%) of 27,964 firm-years are labeled *Meet* (*Suspect*), a ratio similar to those reported in previous studies (e.g., Bhojraj et al., 2009). The univariate comparison in Panel B reveals that while treatment companies have a similar likelihood of meeting their earnings benchmark in the low- and high-enforceability periods, the chances of them beating analyst forecasts by just one cent is higher in high- than in low-enforceability periods.

[Table 2 about here]

Panel C of Table 2 reports the estimation results for equation (2). The dependent variable is *Meet* in column (1), where we compare firms that meet earnings benchmarks with those that do not. The coefficient on *IncreaseEnforce* is significantly positive (0.056, $z = 2.32$) with a marginal effect of 1%, indicating that relative to these same firms in low-enforceability periods and the firms in the control sample, the probability of firms meeting earnings targets is higher in high-enforceability periods.

When we use *Suspect* as the dependent variable, the coefficient on *IncreaseEnforce* remains significantly positive (0.176, $z = 3.46$) with a marginal effect of 3%. This means that the probability of just meeting or slightly beating earnings targets increases by 25% in high-enforceability periods (=3%/12%, where 12% is the

average likelihood of meeting or slightly beating analyst forecasts for all firms during low-enforceability periods).[11] These results suggest that managers pay more attention to performance benchmarks when non-compete clauses are more strictly enforced, consistent with H1.[12] The coefficients on the control variables, where statistically significant, have the predicted signs.

4.3. The effect of increased non-compete enforceability on discretionary expenses

To investigate the effects of non-compete provisions on discretionary expenses (H2), we estimate the following model (Healy and Wahlen, 1999; Barton and Simko, 2002; Cohen and Zarowin, 2010):

$$\begin{aligned} Cut\_Disx_{i,t} = {} & \alpha + \beta_1 IncreaseEnforce_{s,t} + \beta_2 AnalystFollowing_{i,t-1} \\ & + \beta_3 Litigation_{i,t} + \beta_4 StockIssue_{i,t+1} + \beta_5 HerfIndx_{i,t-1} \\ & + \beta_6 MktShr_{i,t-1} + \beta_7 Distress_{i,t-1} + \beta_8 Bloat_{i,t-1} + \beta_9 BigAud_{i,t} \\ & + \beta_{10} AuditTenure_{i,t} + \beta_{11} MBE_{i,t-1} + \beta_{12} ROE_{i,t-1} + \beta_{13} Lev_{i,t} \\ & + \beta_{14} IC_{i,t} + \beta_{15} TA_{i,t-1} + \beta_{16} \Delta GDP_{i,t} + \beta_{17} AM_{i,t} + \gamma_i + \sigma_t + \varepsilon_{i,t} \end{aligned} \quad (3)$$

where *Cut_Disx* is a proxy for abnormal reductions in discretionary spending including R&D, advertising, and SG&A expenses as defined below; *AnalystFollowing*, *StockIssue*, and *BigAud* are as described above; *Litigation* is a dummy variable equal to 1 if the firm is in an industry at high risk of litigation (including biotechnology, computers, electronics, and retail); *HerfIndx* is the Herfindahl index, equal to the sum of the squared share of each firm's sales within the industry in year *t*-1; *MktShr* is the share of a firm's sales in the industry in year *t*-1 (where industry classification follows Fama and French, 1997); *Distress* is a dummy variable equal to 1 if a firm's Altman (1968) Z-score is less than 2.675 in year *t*-1, and 0 otherwise; *Bloat* is the industry

[11] We obtain the 12% likelihood of meeting or slightly beating earnings targets by pooling together the control firms in the entire sample period and the treatment firms in the low-enforceability periods.

[12] We winsorize all continuous variables at 1% and 99% percentiles for our tests. Our conclusions remain the same without winsorizing the variables. Also, our results are similar if we recognize the actual amount of enforcement changes instead of using directional dummy variables, or if we replace *IncreaseEnforce* by the continuous state-level enforceability index (shown in Table 1).

median-adjusted balance sheet bloat in year *t*-1, with balance sheet bloat calculated as net operating assets divided by total sales at the end of year *t*-1; *AuditTenure* is a dummy variable equal to 1 if the auditor has audited the client for more than six years (the sample median), and 0 otherwise; *MBE* is a dummy variable equal to 1 if the firm meets its consensus analyst earnings forecast in year *t*-1, and 0 otherwise; *ROE* is the industry-adjusted return on equity for year *t*-1; *Lev* is the leverage ratio for year *t*; *IC* is the inverse of the firm's interest coverage ratio for year *t*; *ΔGDP* is the percentage change in the state's real gross domestic product for year *t*; and *AM* is the performance-adjusted discretional accruals (Jones, 1991; Kothari et al., 2005).

Following Roychowdhury (2006), we use equation (4) to gauge abnormal levels of discretionary expenses:

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

where $Disx_{i,t}$ is the sum of R&D, advertising, and SG&A expenses;[13] and $A_{i,t-1}$ is total assets in year *t*-1. Regression (4) is estimated cross-sectionally for each industry-year that contains at least 15 observations, where industry membership is defined as in Fama and French (1997). Our proxy for how much a firm reduces discretionary spending is constructed as the residuals of regression (4) multiplied by -1, denoted as *Cut_Disx*; a positive value of *Cut_Disx* indicates an abnormal cut in discretionary expenses, which has the effect of raising reported earnings in the current year.

The descriptive statistics of the variables used in equation (3), also reported in Panel A of Table 2, are comparable to those in previous studies (e.g., Roychowdhury, 2006; Cohen and Zarowin, 2010). Univariate comparison in Panel B reveals that the mean value of *Cut_Disx* for treatment companies is significantly higher in the

[13] In Compustat, the SG&A item includes advertising and R&D. Following Banker et al. (2011), we compute our SG&A component as SG&A net of advertising and R&D expenses to ensure that our three components of discretionary expenses are mutually exclusive.

high-enforceability periods (t = 2.24) than in the low-enforceability periods, indicating that managers tend to cut discretionary expenses when non-compete provisions are more strictly enforced.

The results of estimating equation (3) are presented in Panel D of Table 2. The coefficient on *IncreaseEnforce* is significantly positive in explaining *Cut_Disx* (t = 3.45). This is consistent with hypothesis H2, which predicts that increases in non-compete enforceability cause managers to cut discretionary expenditures.

Next, we explore the economic consequences of cutting discretionary expenses.

4.4. The impact of non-competes on the efficiency of discretionary expenditures and future performance

To test hypotheses H3a and H3b, we compare firms' efficiency of discretionary expenditures and future performance for the periods after, versus before, the event of increased non-compete enforceability, and recognize the extent of reductions in discretionary expenses in the tests.

We conduct analyses separately for the three components of discretionary expenses—R&D, advertising, and SG&A, as they each have distinct consequences for firm performance. Abnormal reductions of R&D, advertising, and SG&A expenses, denoted as *Cut_RD*, *Cut_Adv*, and *Cut_SGA*, respectively, are estimated with model (4) and constructed analogously to *Cut_Disx* above.[14]

4.4.1. Efficiency of R&D expenses

[14] The Pearson correlation between *Cut_RD* and *Cut_Adv* is 0.056, that between *Cut_RD* and *Cut_SGA* is 0.277, and that between *Cut_Adv* and *Cut_SGA* is 0.098, all significant at the 0.01 level.

We examine firms' innovation output, which proxies for the efficiency of R&D expenses, using the following equation (Hirshleifer et al., 2012; Chemmanur et al., 2014):

$$\begin{aligned} Ln(1+Innovation_{i,t}) = {} & \alpha + \beta_1 IncreaseEnforce_{s,t} + \beta_2 IncreaseEnforce_{s,t} \times Cut\_RD_{i,t} \\ & + \beta_3 PPE_{i,t-1} + \beta_4 Tobinsq_{i,t-1} + \beta_5 Ln(FirmAge)_{i,t-1} + \beta_6 Lev_{i,t-1} \\ & + \beta_7 Ln(Sales)_{i,t-1} + \beta_8 AnnRet_{i,t-1} + \beta_9 Ln(PPE/\#Employees)_{i,t-1} \\ & + \beta_{10} IO_{i,t-1} + \beta_{11} AnalystFollowing_{i,t-1} + \beta_{12} HerfIndx_{i,t-1} \\ & + \beta_{13} HerfIndx^2_{i,t-1} + \gamma_i + \sigma_t + \varepsilon_{i,t} \end{aligned} \quad (5)$$

where *Innovation* is one of the following four measures: (1) *Patent*, the total number of patent applications filed in a given year that are later granted patent; (2) *Citation_PP*, the average number of citations received in future years per patent applied for in a given year (and later granted approval); (3) *Claims*, the average number of new inventions claimed by the firm in each patent that is applied in a given year (and later granted approval); and (4) *Grantlag*, the time period in days between the patent filing date and the subsequent approval date. Because a patent's citations accumulate over time, in computing *Citation_PP*, we adjust the raw citation counts with the weighting index of Hall et al. (2001; 2005) to correct for the truncation bias.[15]

The variable *Cut_RD* captures the (abnormal) reduction in R&D expenditures, and the interaction term in equation (5), *IncreaseEnforce*×*Cut_RD*, shows whether cutting R&D expenditures affects the innovation output in the years of increased non-compete enforceability. This term helps to distinguish between the two alternative motives for cutting discretionary expenditures: interest alignment (H3a) versus

[15] The number of patent claims measures the patent scope and is found to be one of the most significant indicators of ultimate patent value (Allison et al., 2004; Lemley and Shapiro, 2005). We retrieve data on patents and citations from the Patent and Citation Database maintained by the National Bureau of Economic Research, Inc. (NBER). The latest year covered in this dataset is 2006, so the patents applied in 2004 may not all be included in the database as there is generally a two-year gap between application and granting of a patent. Following Hall et al. (2001), we thus end our sample for this test at 2003. We set the number of patents to zero for firm-years covered by the NBER database that have no patent applications. For the tests on patent citations, the number of claims per patent and time lag in obtaining patent approval, we only include firm-years with at least one patent.

myopic reporting (H3b).

As in Hirshleifer et al. (2012), Chang et al. (2015), and Koh and Reeb (2015), we control for property, plant, and equipment (*PPE*), firms' growth options (*Tobinsq*), firm age (*Firmage*), leverage ratio (*Lev*), sales revenue (*Sales*), annual stock returns (*AnnRet*), capital intensity (*PPE/#Employees*), institution ownership (*IO*), analyst following (*AnalystFollowing*), and industry competition (*HerfIndx*, $HerfIndx^2$).

The results of estimating equation (5) are shown in Panel A of Table 3. In column (1) where we explain *Patent*, the coefficients on both *IncreaseEnforce* and *IncreaseEnforce*×*Cut_RD* are significantly negative, suggesting that fewer patent applications are filed as non-compete enforceability increases, especially for firms cutting R&D expenses severely. However, a decreasing amount of patent counts alone does not necessarily suggest myopic behavior; instead, it could mean that with reduced R&D expenses, managers concentrate resources on core technology developments vital for the company.

To gain further insights into the characteristics of R&D investments, we next examine the impact (*Citation_PP*) and scope (*Claims*) of patents together with the time lag between patent filing and approval. The coefficient on *IncreaseEnforce* is significantly negative in explaining citations per patent (column 2) and the time lag (column 4), and the coefficient on *IncreaseEnforce*×*Cut_RD* is significantly negative in explaining scope (column 3) and time lag (column 4). These results generally indicate that as non-competes are more strictly enforced, the scope and the impact of patents decline and the time between patent filing and approval is shortened, especially for firms cutting R&D expenses more severely.[16]

[16] Relatedly, Younge and Marx (2012) find that subsequent to the increased enforceability of non-competes in Michigan, firms experience decreases in both the technical diversity of the research team and the amount of explorative patents, also suggesting more myopic R&D activities.

Overall, the results in Panel A suggest that when non-competes become more enforceable, managers engage in innovations that have narrower scopes. And due to smaller scopes, these applications are granted patents more quickly, but they turn out to be less impactful. The evidence thus suggests that innovation performance declines subsequent to increased non-compete enforceability, especially for firms that severely reduce R&D expenses, and that managers act in a more risk-averse manner in undertaking R&D investments.[17]

[Table 3 about here]

4.4.2. Efficiency of advertising expenses

We use equation (6) below to examine whether, after an increase in non-compete enforceability, firms cutting advertising expenses experience a change in market share (Smith and Park, 1992):

$$\begin{aligned} MktShr_{i,t+k} = {} & \alpha + \beta_1 IncreaseEnforce_{s,t} + \beta_2 IncreaseEnforce_{s,t} \times Cut\_Adv_{i,t} \\ & + \beta_3 Ln(Firmage)_{i,t-1:t+2} + \beta_4 Ln(Debt)_{i,t-1:t+2} + \beta_5 Ln(Assets)_{i,t-1:t+2} \\ & + \beta_6 Ln(Sales)_{i,t-1:t+2} + \beta_7 Ln(State\ unemployment)_{s,t:t+3} \\ & + \beta_8 Ln(State\ personal\ income)_{s,t:t+3} + \gamma_i + \sigma_t + \varepsilon_{i,t}, \end{aligned} \quad (6)$$

where $MktShr_{i,t+k}$ (k = 0, 1, 2, 3) is firm i's market share in year t+k, computed as the firm's annual sales divided by total annual sales generated by all firms in the same industry, and *Cut_Adv* refers to the abnormal reduction in advertising expenses. As in Pan et al. (1999) and Garmaise (2011), we control for firm age (*Firmage*), book value of debt (*Debt*), assets (*Assets*), sales (*Sales*), state unemployment rate (*State unemployment*), and state personal income per capita (*State personal income*). The regression includes year and firm fixed effects.

[17] Using the entire patent database, we find positive correlations between *Claims*, *GrantLag*, and *Citation_PP* (all significant at the 1% level), indicating that patent applications with narrower scopes are granted approval more quickly, but they receive fewer citations in the future. Untabulated results indicate that when non-competes become more strictly enforced, firm risks (measured by stock return or earnings volatility) also decrease, which further confirms that managers become more risk-averse.

The results of estimating equation (6) are shown in Panel B of Table 3. The coefficient on *IncreaseEnforce* tends to be negative, though insignificant, in explaining market shares. Importantly, the coefficient on *IncreaseEnforce*×*Cut_Adv* is significantly negative in explaining $MktShr_t$ and $MktShr_{t+2}$, while it is negative but insignificant in explaining $MktShr_{t+1}$ and $MktShr_{t+3}$. Thus, as non-competes become more enforceable, affected firms generally experience declines in market share, and this phenomenon is mainly displayed by those firms that severely cut advertising expenses. Overall, the results show that as non-compete enforceability increases, firms reducing advertising spending suffer losses in market share.

4.4.3. Efficiency of SG&A expenses

To evaluate the efficiency of SG&A, we follow Banker et al. (2011) and run a regression analogous to equation (6) but using Adj_ROE_{t+k}, (k = 0, 1, 2, 3) as the dependent variable and replacing *Cut_Adv* with *Cut_SGA*. We use Adj_ROE_{t+k}, (k = 0, 1, 2, 3) to measure a firm's pre-manipulated ROE, which is calculated each year by adding back the abnormal reduction in discretionary expenses in the year (*Cut_Disx*) to the reported earnings.[18] The results are shown in Panel C of Table 3. To conserve space, we only include our key explanatory variables. The coefficient on *IncreaseEnforce*×*Cut_SGA*, while insignificant in explaining a firm's (adjusted) ROE in the current year and the two subsequent years, is significantly negative in explaining the *Adj_ROE* in year t+3. This indicates that as non-competes become more enforceable, firms cutting SG&A expenses display lower future performance in terms of ROE.[19]

[18] Banker et al. (2011) is the only study that we are aware of in the literature examining the effect of SG&A expenses on accounting earnings. Our results are similar if we use ROE or the natural log of ROE (excluding observations with negative ROEs) as the dependent variable (Garmaise, 2011).

[19] Untabulated results also show that reductions in total discretionary expenses (*Cut_Disx*) have

To summarize, the results in Panels A to C of Table 3 indicate that as non-compete enforceability increases, firms that reduce discretionary expenditures experience declines in operational performance in terms of innovation output, market share, and overall financial performance. Thus, our evidence is more consistent with the managerial myopia hypothesis (H3b) than interest alignment (H3a).[20]

4.4.4. Effect of increased enforceability on firms with high versus low growth opportunities

We now explore whether the impact of non-compete enforceability differs for high- and low-growth firms. We expect this impact to be more pronounced for firms with high-growth opportunities whereby executive compensation is more closely tied to stock prices (through the use of stock options) and missing earnings benchmarks would trigger sharper declines in stock prices (Skinner and Sloan, 2002). We use the market-to-book ratio of equity (*MB*) in year t-1 as a proxy for growth opportunities. We run a regression analogous to equation (6) but using Adj_ROE_{t+k} (k = 0, 1, 2, 3) as the dependent variable and replacing *Cut_Adv* with *MB*. In addition to the accounting performance tests, we also examine current and future stock performance measured by industry-adjusted return over a year (*ARET*). We control for firm size (*TA*), leverage (*Lev*), growth opportunities (*MB*), and momentum (*MOM*) (Carhart, 1997).

As shown in Panel D of Table 3, the coefficient on *IncreaseEnforce*×*MB* is significantly negative in explaining both Adj_ROE_{t+3} and $ARET_{t+3}$, while insignificant

a negative effect on *Adj_ROE* in year t+3 for the treatment firms.

[20] In addition to examining operational performance measures, we also find that increased non-compete enforceability has a negative effect on future stock returns, especially for firms that cut discretionary expenses more severely, and that this result holds with respect to the individual components of discretionary expenses as well as at the aggregate level. To conserve space, we do not tabulate these return-based results in the main tests. The negative effects on stock returns are also apparent later where we explore cross-sectional variations in the impact of non-compete enforceability (Table 4).

in other columns (except $ARET_t$). This indicates that the adverse impact of non-compete enforceability on future firm performance is more pronounced for firms with higher growth opportunities.

4.5. The varying impacts of non-compete provisions across different types of CEOs

Next, we examine hypothesis H4 that predicts the effect of non-compete provisions on different types of CEOs. First, to explore whether CEOs employed in more localized industries are more constrained by non-compete provisions, we measure firm *i*'s localization by the intensity of local (in-state) competition, denoted as *InStateCompete*, calculated as the sum of sales revenues of all competing firms in the same state and with the same industry membership as firm *i*, divided by the total sales of that industry (Garmaise, 2011). We then include both *InStateCompete* and its interaction with *IncreaseEnforce* in equations (2), (3), (5), and (6).[21] Panel A of Table 4 shows that the coefficient on *IncreaseEnforce*×*InStateCompete* is significantly positive in explaining *Meet* and *Cut_Disx*, and is significantly negative in explaining patent measures, market share, and firm overall performance Adj_ROE_{t+3} and $ARET_{t+3}$.[22] This indicates that the effects of increased non-compete enforcement are mainly driven by firms in more localized industries.

Next, to explore whether CEOs of lower ability are more affected by non-compete clauses, we rely on the managerial ability measure developed by Demerjian et al. (2012) (*Ability*) and include it together with its interaction with

[21] For all cross-sectional tests in Section 4.5., we do not include the interaction terms of *IncreaseEnforce* and *Cut_RD*/*Cut_Adv* in equations (5) and (6) as the purpose of these tests is to see whether non-competes have a greater effect on certain types of CEOs.

[22] For brevity, we only highlight in Table 4 our performance measures in years that are significantly affected by increased non-compete enforceability as shown in Table 3 (such as $MktShr_t$, Adj_ROE_{t+3}, and $ARET_{t+3}$). The results for performance measures in other years are consistent with those in Table 3.

IncreaseEnforce in equations (2), (3), (5), and (6).[23] The results are reported in Panel B of Table 4. The coefficients on *IncreaseEnforce* remain significant in all columns, suggesting that in general all CEOs react to increased non-compete enforceability. On the other hand, the coefficient on *IncreaseEnforce*×*Ability* is significantly negative for *Meet* and *Cut_Disx*, and is significantly positive for all performance measures (except for *Citation_PP* and $ARET_{t+3}$). This finding confirms that non-compete agreements are more binding for low-ability CEOs.

[Table 4 about here]

Finally, to explore whether CEOs with longer tenure are less influenced by non-compete provisions, we add the variable *Tenure* into equations (2), (3), (5) and (6), which represents the number of years that a CEO has held his/her post, and its interaction with *IncreaseEnforce*. The results reported in Panel C of Table 4 show that the coefficient on *IncreaseEnforce*×*Tenure* is significant in all columns except for *Patent*, and the sign of this coefficient is opposite to the main effect of *IncreaseEnforce*. Thus, while in general all CEOs are affected by non-compete provisions, the effect is less (more) pronounced for CEOs with a longer (shorter) tenure.

5. Additional tests and robustness checks

5.1. Accrual manipulation and other types of real transactions management

In addition to real transactions management, managers may also manipulate accounting accruals to achieve a desired level of reported earnings. Unlike real transactions management, accruals manipulation does not have a first-order effect on

[23] Using Data Envelopment Analysis (DEA), Demerjian et al. (2012) measures managerial ability based on managers' efficiency in using corporate resources to generate sales revenue relative to their industry peers. In validating this ability measure, they show that firms that appoint more able CEOs are associated with better future performance. Demerjian et al. (2013) further find that managers with higher ability are associated with better earnings quality.

cash flows, and can be carried out more readily near the end of a fiscal period if true (unmanaged) earnings happen to be below an earnings target. Since tighter enforcement policies cause managers to attach greater importance to short-term earnings benchmarks, we also expect increased occurrences of accruals manipulation. However, accruals involve accounting judgment, and an abnormal level of accruals (in relation to sales, cash flow, or assets) will attract the attention of auditors, investors, and regulators (Dechow et al., 2010). Desai et al. (2006) and Hazarika et al. (2012) show that accruals manipulation tends to have adverse consequences for managers such as forced executive turnover. Thus, how increased enforceability of non-competes affects managers' use of accruals management is an open empirical question.[24]

We follow Kothari et al. (2005) and use performance-adjusted discretionary accruals to proxy for accruals management (*AM*). The result is provided in Panel A of Table 5; for brevity, only the key variables are reported. The coefficient on *IncreaseEnforce* is positive (0.005, t = 1.78), significant at the 10% level. This suggests that discretionary accruals increase somewhat, subsequent to a tightening of non-compete enforceability.

According to Roychowdhury (2006), managers may also manage earnings through overproduction (*RM_Prod*), which reduces the overhead cost charged to costs of goods sold, or through sales manipulation by offering lenient credit terms or large sales discounts to customers, which cause a large gap between sales and operating cash flow (*RM_CFO*). We compute *RM_Prod* and *RM_CFO* as in prior studies (e.g., Cohen et al., 2008) (with abnormal cash flow from operations multiplied by -1 so that a higher value of *RM_CFO* translates to a greater amount of earnings management).

[24] Our approach here of considering multiple channels of earnings management together responds to the call by Beyer et al. (2010, page 298).

Thus, we also test the effect of non-compete enforcement on these other types of real activity management, with the results also shown in Panel A of Table 5. The coefficient on *IncreaseEnforce* is significantly positive when the dependent variable is *RM_Prod*, but is insignificant when the dependent variable is *RM_CFO*.

Overall, our results show that besides adjusting discretionary expenses, managers also manage earnings using other types of real transactions and, to some extent, accruals manipulation.[25]

5.2. Examining the exogenous nature of legislative (versus judicial) events

Our study utilizes four events triggering changes in the enforceability of non-competes. Two (Florida, 1996 and Louisiana, 2003) were led by legislative changes; the other two (Texas, 1994 and Louisiana, 2001) were court rulings. In our tests, we regard all four as exogenous events for firms. One may argue that legislative events might have been anticipated by managers if, say, firms were involved in lobbying policymakers.

To address this concern, we examine the time trend of earnings management behavior before and after the events, which allows us to gauge the point in time at which managers start to alter their behavior. Specifically, we modify equations (2) and (3) by using *Pre(1)*, *Post(0)*, *Post(1)*, and *Post(2+)* as the main explanatory variables, where *Pre(1)* is an indicator variable for firms in Florida, Texas, or Louisiana one year prior to an event of enforceability change, and *Post(0)*, *Post(1)*, and *Post(2+)* are indicator variables for firms in a treatment state in an event year (year *t*), one year

[25] Firms may alter compensation contracts in response to regulatory changes in non-compete enforceability (Garmaise, 2011), which in turn can affect earnings management (Burns and Kedia, 2006; Effendi et al., 2007). We thus also check the robustness of our findings by controlling for CEO bonus, new options granted, number of unexercised vested options held, number of unexercised unvested options, and CEO ownership (Cohen et al., 2008). We also control for severance pay to the departing executive (Yermack, 2006; Rau and Xu, 2013). Our results remain qualitatively the same.

after an event (year t+1), and two or more years after an event (year t+2 or beyond), respectively. We perform this test separately for the three treatment states; for Louisiana, only the 2003 legislative event is considered.

The results are presented in Panel B of Table 5, where we omit the control variables for brevity. For all three states, the coefficients on both *Pre(1)* and *Post(0)* are generally insignificant in both columns, except for Louisiana (for which the coefficient on *Pre(1)* is negative),[26] suggesting that managers have not anticipated upcoming events, legislative as well as judicial. In contrast, we find significant coefficients on *Post(1)* and *Post(2+)*, suggesting that managers started to reduce R&D and discretionary expenses in the years *after* the legal amendments, both legislative and judicial, leading to increased non-compete enforceability. These results alleviate the concern that legislative changes might not be as exogenous as judicial events.

[Table 5 about here]

6. Conclusions

In this study, we examine how exogenous changes in enforceability of non-compete clauses affect managers' incentives to undertake investment activities, their financial reporting practices, and firm performance. We employ legal changes in non-compete enforceability in Florida, Louisiana, and Texas during the period 1992-2004 and hypothesize that increased non-compete enforceability may either reduce the agency cost by promoting a closer interest alignment between managers and investors or increase the agency cost by aggravating managerial myopia as

[26] The *Pre(1)* variable captures Louisiana firms in the year 2002. Notably, as indicated in Section 2.1., Louisiana experienced a decreased enforceability of non-competes in 2001 due to a court ruling, and then an increased enforceability of non-competes in 2003 due to a legislative event. According to H1 and H2, we should observe a lower likelihood of meeting earnings benchmarks and an increase in discretionary expenses when enforceability of non-competes is lowered, which may explain why the coefficients on *Pre(1)* are significantly negative.

managers become more focused on short-term performance. We predict that in both scenarios, tighter enforceability of non-competes leads to increased short-term earnings performance and reduced R&D, advertising and other discretionary expenses, but the consequences diverge for firm performance in the subsequent years.

Consistent with our expectations, we find that when the enforceability of non-compete clauses is tightened, the likelihood of firms achieving short-term earnings targets increases. We also find that firms reduce R&D, advertising, and other SG&A expenses. The impact of increased non-compete enforceability is stronger for firms operating in more localized industries, where non-compete provisions are likely to have a stronger effect, and when CEOs have lower ability or shorter tenure with current employers, suggesting more limited employment opportunities.

To distinguish between the alternative motives that might be at work, we examine the efficiency of discretionary expenditures and firm performance in the years after, versus before, the legal changes of non-compete enforceability. We find that when non-compete enforceability increases, firms cutting R&D expenses display lower innovation output (fewer patent applications), with the impact of innovation becoming smaller (fewer citations per patent) and the scope of innovation narrower (fewer claims per patent), and firms cutting advertising and other SG&A expenses experience declines in market share and future profitability. Furthermore, these types of real activity management lead to lower stock returns in future years. Taken together, our empirical evidence is more consistent with the notion that tighter enforceability of non-competes exacerbates myopic behavior, which is detrimental to a firm's future performance, than the notion of it fostering closer interest alignment.

An important message from the study is that a firm's economic activities and financial reporting behavior depend not only on the terms stipulated by employment

contracts but also on the legal framework within which the contractual terms are enforced. Ball et al. (2003) explain that financial reporting quality is a function of both the accounting standards in force and the legal and economic framework that influences reporting incentives. By examining financial reporting practices from a contracting perspective, our study echoes that fundamental message. In particular, this study demonstrates a concrete channel through which institutional factors shape the managerial reporting incentives that ultimately cause economic consequences.

Appendix A: Variable descriptions

Variable	Definition
Ability	The managerial ability score obtained from Demerjian et al. (2012). The score is estimated by Data Envelopment Analysis (DEA) on how efficiently managers use a firm's resources to generate revenues. The data are available at https://community.bus.emory.edu/personal/PDEMERJ/Pages/Home.aspx.
Adj_ROE	Return on equity before abnormal discretionary expense reduction, computed as net income before the abnormal reduction in discretionary expenses estimated from equation (4), divided by last year's book equity.
AM	Performance-adjusted discretionary accruals, where discretionary accruals are measured as deviations from the predicted values of the industry-year regression $TAcc_{i,t}/A_{i,t-1} = \alpha_0 + \alpha_1(1/A_{i,t-1}) + \alpha_2(\Delta S_{i,t}/A_{i,t-1}) + \alpha_3(PPE_{i,t}/A_{i,t-1}) + \varepsilon$ (Jones, 1991), where $TAcc_{i,t}$ equals earnings before extraordinary items and discontinued operations minus the operating cash flows reported in the statement of cash flows in year t; $A_{i,t-1}$ is total assets in year t-1; and $\Delta S_{i,t}$ is as previously defined. Following Kothari et al. (2005), we subtract the change in accounts receivable from $\Delta S_{i,t}$ in estimating the above equation. $PPE_{i,t}$ is gross property, plant, and equipment in year t. The classification of industry membership follows Fama and French (1997). Performance-adjusted discretional accruals (*AM*) are the difference between firm i's discretionary accruals and the median discretionary accruals of all other firms in the same ROA quintile in the industry (Badertscher, 2011).
AnalystFollowing	Natural logarithm of 1 plus the number of analysts following the firm.
AnnRet	Monthly-compounded stock return over year *t*.
ARET	The industry-adjusted stock return over the year.
Assets	The book value of the firm's total assets.
AuditTenure	1 if the auditor has audited the client for more than six years (the sample median), and 0 otherwise.
BigAud	1 if the firm is audited by a Big 4/5/6 auditor in the year, and 0 otherwise.
Bloat	Industry median adjusted balance sheet bloat, where balance sheet bloat is calculated as net operating assets divided by total sales at the end of the year. Net operating assets are operating assets minus operating liabilities, where operating assets are total assets minus cash and short-term investments, and operating liabilities are total assets minus the sum of short-term debt, long-term debt, minority interest, preferred stock, and common equity.
CFO	Cash flow from operations, deflated by the total assets at the beginning of the year.
Citation_PP	Average number of citations received in the future years per patent applied in a given year (and later granted approval); each patent's number of citations is multiplied by the weighting index from Hall et al. (2001; 2005).
Claims	Average number of new inventions claimed by the firm in each patent that is applied in a given year (and later granted approval).
Cut_Adv	Abnormal cut in advertising expenses, measured as deviations from the predicted values of the industry-year regression $Adv_{i,t}/A_{i,t-1} = \alpha_0 + \alpha_1(1/A_{i,t-1}) + \alpha_2(S_{i,t-1}/A_{i,t-1}) + \varepsilon$, multiplied by -1, where *Adv* is the advertising expenses.
Cut_Disx	Abnormal cut in discretionary expenses, measured as deviations from the predicted values of the industry-year regression $Disx_{i,t}/A_{i,t-1} = \alpha_0 + \alpha_1(1/A_{i,t-1}) + \alpha_2(S_{i,t-1}/A_{i,t-1}) + \varepsilon$, multiplied by -1, where *Disx* is the sum of R&D, advertising, and SG&A expenses.
Cut_RD	Abnormal cut in R&D expenses, measured as deviations from the predicted values of the industry-year regression $RD_{i,t}/A_{i,t-1} = \alpha_0 + \alpha_1(1/A_{i,t-1}) + \alpha_2(S_{i,t-1}/A_{i,t-1}) + \varepsilon$, multiplied by -1, where *RD* is the R&D expenses.
Cut_SGA	Abnormal cut in SG&A expenses excluding advertising and R&D expenses (*SGA*), measured as deviations from the predicted values of the industry-year regression $SGA_{i,t}/A_{i,t-1} = \alpha_0 + \alpha_1(1/A_{i,t-1}) + \alpha_2(S_{i,t-1}/A_{i,t-1}) + \varepsilon$, multiplied by -1.
Debt	Book value of debt.
Distress	1 if a firm's Altman (1968) Z-score is less than 2.675, and 0 otherwise, where Z-score = 3.3 (Net income/Assets) + 1.0 (Sales/Assets) + 1.4 (Retained Earnings/Assets) + 1.2 (Working Capital/Assets) + 0.6 (Stock Price × Shares

	outstanding)/Total Liabilities.
Firmage	Number of years since a firm first appears on Compustat.
GrantLag	Time period in days between the patent filing date and the subsequent approval date.
HabitualBeater	Number of times the firm met the consensus analyst forecast over the past four quarters.
HerfIndx	Herfindahl index, equal to the sum of the squared share of each firm's sales within the industry, where the classification of industry membership follows Fama and French (1997).
$HerfIndx^2$	Square of *HerfIndx.*
IC	Inverse of the firm's interest coverage ratio, calculated as interest expense in year *t* divided by operating income before depreciation in year *t*-1.
IncreaseEnforce	A three-level, {-1, 0, +1}, measure of non-compete enforceability changes.
InStateCompete	The sum of the sales revenues of all competing firms in the same state and with the same industry membership (Fama-French 48 industry) divided by the total sales of that industry.
IO	The percentage of the firm's shares held by institutional investors.
Lev	Leverage ratio, calculated as short-term debt plus long-term debt in year *t*, scaled by total assets in year *t*-1.
Litigation	1 if the firm is in a high-litigation risk industry, including biotechnology, computers, electronics, and retail (Francis et al., 1994).
MB	Market-to-book ratio of equity.
MBE	1 if the firm meets the consensus analyst earnings forecast in the year, and 0 otherwise.
Meet	1 if a firm-year meets or beats the consensus analyst forecast, and 0 otherwise.
MktShr	Market share, measured as a firm's annual sales divided by total annual sales generated by firms in the same industry, where the classification of industry membership follows Fama and French (1997).
MOM	The industry-adjusted stock return over the previous year.
Patent	Total number of patent applications filed in a given year that are later granted.
Post(0)	1 for firms in Florida, Texas, or Louisiana in an event year of enforceability change (i.e., year *t*), and 0 otherwise.
Post(1)	1 for firms in Florida, Texas, or Louisiana one year after the event of enforceability change (i.e., year *t*+1, where *t* is the event year for an enforceability change), and 0 otherwise.
Post(2+)	1 for firms in Florida, Texas, or Louisiana two or more years after the event of enforceability change (i.e., year *t*+2 or beyond, where *t* is the event year for an enforceability change), and 0 otherwise.
PPE	Property, plant, and equipment divided by total assets.
PPE/#Employees	Property, plant, and equipment divided by the number of employees.
Pre(1)	1 for firms in Florida, Texas, or Louisiana one year prior to an event of enforceability change (i.e., year *t*-1, where *t* is the event year for an enforceability change), and 0 otherwise.
RM_CFO	Abnormal cash flow, measured as deviations from the predicted values of the industry-year regression $CFO_{i,t}/A_{i,t-1} = \alpha_0 + \alpha_1(1/A_{i,t-1}) + \alpha_2(S_{i,t}/A_{i,t-1}) + \alpha_3(\Delta S_{i,t}/A_{i,t-1}) + \varepsilon$, multiplied by -1, where *CFO* is cash flow from operations.
RM_Prod	Measured as deviations from the predicted values of the industry-year regression $Prod_{i,t}/A_{i,t-1} = \alpha_0 + \alpha_1(1/A_{i,t-1}) + \alpha_2(S_{i,t}/A_{i,t-1}) + \alpha_3(\Delta S_{i,t}/A_{i,t-1}) + \alpha_3(\Delta S_{i,t-1}/A_{i,t-1}) + \varepsilon$, where *Prod* is the sum of cost of goods sold and change in inventory.
ROA	Return on assets, computed using net income for the rolling four quarters ending in the third quarter of year *t*.
ROE	Industry-adjusted return on equity, computed as net income divided by last year's book equity.
Sales	A firm's annual sales.
Shares	Natural logarithm of the number of shares outstanding.
State unemployment	The state's annual unemployment rate.
State personal income	The state's personal income per capita in the year.
StockIssue	1 if the firm issues equity in year *t*+1, and 0 otherwise.
Suspect	1 if a firm-year just meets or beats the consensus analyst forecast by 1 cent,

	and 0 otherwise.
TA	Natural logarithm of total assets.
Tenure	Number of years the CEO has held his/her post.
Tobinsq	Tobin's Q, measured as (market value of equity + book value of total debt) / total assets.
ΔGDP	Percentage change in the state's real gross domestic product for year *t*.

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Table 1
Non-compete enforceability index and the sample distribution by state.

State	Enforceability index	Number of Obs.	State	Enforceability index	Number of Obs.
Alabama	5	235	Missouri	7	551
Alaska	3	23	Montana	2	25
Arizona	3	363	Nebraska	4	177
Arkansas	5	143	Nevada	5	174
California	0	5,346	New Hampshire	2	134
Colorado	2	608	New Jersey	4	1,033
Connecticut	3	632	New Mexico	2	25
Delaware	6	93	New York	3	1,753
DC	7	60	North Carolina	4	460
Florida 1992-1996	7	343	North Dakota	0	534
Florida 1997-2004	9	680	Ohio	5	1,010
Georgia	5	816	Oklahoma	1	136
Hawaii	3	17	Oregon	6	255
Idaho	6	96	Pennsylvania	6	1,112
Illinois	5	1,269	Rhode Island	3	100
Indiana	5	530	South Carolina	5	128
Iowa	6	329	South Dakota	5	18
Kansas	6	164	Tennessee	7	397
Kentucky	6	182	Texas 1992-1994	5	545
Louisiana 1992-2001, 2004	4	136	Texas 1995-2004	3	1,881
Louisiana 2002-2003	0	27	Utah	6	207
Maine	4	29	Vermont	5	39
Maryland	5	316	Virginia	3	459
Massachusetts	6	1,700	Washington	5	361
Michigan	5	602	West Virginia	2	41
Minnesota	5	1,161	Wisconsin	3	420
Mississippi	4	88	Wyoming	4	1

Notes: The non-compete enforceability index is taken from Garmaise (2011). The sample consists of 27,964 firm-year observations (5,244 firms) from 1992 to 2004. Our treatment states include three (Florida, Louisiana, and Texas) that amended their non-compete enforcement policy during this period; the others serve as controls.

Table 2
Non-compete enforceability and the likelihood of meeting/beating analysts' forecasts consensus.

Panel A: Descriptive statistics

	Obs.	Q1	Mean	Median	Q3	Std.
Dependent variables						
Meet	27,964	0.000	0.520	1.000	1.000	0.500
Suspect	27,964	0.000	0.129	0.000	0.000	0.335
Cut_Disx	27,964	-0.063	0.066	0.074	0.227	0.304
Control variables						
HabitualBeater	27,964	1.000	2.330	2.000	4.000	1.360
AnalystFollowing	27,964	1.099	1.734	1.792	2.303	0.866
StockIssue	27,964	0.000	0.516	1.000	1.000	0.500
BigAud	27,964	1.000	0.938	1.000	1.000	0.242
IO	27,964	0.229	0.438	0.441	0.638	0.248
CFO	27,964	0.021	0.076	0.090	0.156	0.151
ROA	27,964	-0.009	0.015	0.048	0.098	0.170
AnnRet	27,964	-0.250	0.187	0.056	0.408	0.742
MB	27,964	1.459	3.466	2.323	3.904	3.748
Shares	27,964	2.285	3.178	3.030	3.860	1.207
TA	27,964	4.270	5.558	5.408	6.689	1.745
Litigation	27,964	0.000	0.381	0.000	1.000	0.486
HerfIndx	27,964	0.032	0.058	0.047	0.063	0.045
MktShr	27,964	0.000	0.007	0.001	0.005	0.019
Distress	27,964	0.000	0.284	0.000	1.000	0.451
Bloat	27,964	-0.153	0.305	0.029	0.312	1.330
AuditTenure	27,964	0.000	0.579	1.000	1.000	0.494
MBE	27,964	0.000	0.597	1.000	1.000	0.491
ROE	27,964	0.003	0.003	0.096	0.162	0.406
Lev	27,964	0.022	0.234	0.185	0.357	0.241
IC	27,964	0.002	0.130	0.071	0.194	0.280
ΔGDP	27,964	0.042	0.057	0.056	0.074	0.023
AM	27,964	-0.060	-0.006	-0.007	0.045	0.109

Panel B: Likelihood of meeting/beating analysts' forecasts consensus and the level of abnormal discretionary expenses for the treatment states (Florida, Louisiana, and Texas) in low- and high-enforceability periods

	High Enforceability	Low Enforceability	Diff. *t*-stat.
Meet	0.485	0.507	-1.31
Suspect	0.127	0.107	1.83*
Cut_Disx	0.092	0.070	2.24**
Obs.	1,361	2,251	

Notes: The sample period is 1992 to 2004. A firm-year is classified as meeting earnings targets (*Meet*) if it meets or beats the consensus analyst forecast. A firm-year is classified as *Suspect* if it just meets or slightly beats the consensus analyst forecast by just one cent. All continuous variables are winsorized at the 1% and 99% percentiles. See Appendix A for the definitions of the variables. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 2 *(continued)*

Panel C: Logit regression modeling the probability of meeting/beating analysts' forecasts consensus

	Dep. = *Meet*	Marginal effects	Dep. = *Suspect*	Marginal effects
	(1)		(2)	
IncreaseEnforce	0.056**	0.01	0.176***	0.03
	(2.32)		(3.46)	
HabitualBeater	0.477***	0.12	0.402***	0.05
	(37.25)		(18.60)	
AnalystFollowing	-0.292***	-0.07	-0.164***	-0.02
	(-9.64)		(-3.34)	
StockIssue	0.018	0.00	0.125***	0.02
	(0.63)		(3.45)	
BigAud	0.143**	0.04	0.080	0.01
	(2.21)		(0.89)	
IO	0.011	0.00	0.358**	0.05
	(0.12)		(2.28)	
CFO	2.008***	0.50	1.777***	0.24
	(12.50)		(5.65)	
ROA	-0.280	-0.07	2.276***	0.31
	(-1.60)		(5.39)	
AnnRet	0.751***	0.19	0.663***	0.09
	(20.46)		(16.98)	
MB	0.014***	0.00	0.008	0.00
	(4.35)		(1.12)	
Shares	-0.041**	-0.01	0.174***	0.02
	(-1.99)		(4.49)	
TA	0.088***	0.02	-0.087**	-0.01
	(5.90)		(-2.50)	
State fixed effects	Yes		Yes	
Industry fixed effects	Yes		Yes	
Year fixed effects	Yes		Yes	
Pseudo R^2	0.139		0.154	
No. of Obs.	27,964		16,957	

Notes: The sample period is 1992 to 2004. A firm-year is classified as meeting earnings targets (*Meet*) if it meets or beats the consensus analyst forecast. A firm-year is classified as *Suspect* if it just meets or slightly beats the consensus analyst forecast by just one cent. Column (1) includes all available firm-year observations. Column (2) includes firm-years that just meet or slightly beat the consensus analyst forecast and those that miss the consensus analyst forecast. All continuous variables are winsorized at the 1% and 99% percentiles. See Appendix A for the definitions of the variables. Intercepts are not reported. The numbers in parentheses are *z*-statistics, based on the Huber-White sandwich estimate of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 2 *(continued)*

Panel D: Regression of abnormal discretionary expenses on changes in non-compete enforceability

	Cut_Disx
IncreaseEnforce	0.024***
	(3.45)
AnalystFollowing	-0.009**
	(-1.98)
Litigation	0.098***
	(2.86)
StockIssue	0.002
	(0.68)
HerfIndx	-0.235**
	(-2.21)
MktShr	-0.854***
	(-3.24)
Distress	0.008
	(1.52)
Bloat	0.006***
	(3.16)
BigAud	-0.024**
	(-2.22)
AuditTenure	-0.007*
	(-1.94)
MBE	-0.001
	(-0.57)
ROE	0.013**
	(2.01)
Lev	-0.112***
	(-7.68)
IC	-0.019**
	(-2.41)
TA	0.150***
	(7.49)
ΔGDP	0.218*
	(1.82)
AM	-0.230***
	(-7.71)
Firm fixed effects	Yes
Year fixed effects	Yes
Adjusted R^2	0.739
No. of Obs.	27,964

Notes: The sample period is 1992 to 2004. All continuous variables are winsorized at the 1% and 99% percentiles. See Appendix A for the definitions of the variables. Intercepts are not reported. The numbers in parentheses are *t*-statistics, based on the Huber-White sandwich estimate of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 3
Consequences of non-compete enforceability: Differentiating hypotheses H3a and H3b.

Panel A: R&D expenses efficiency				
	Ln(1+Patent)	*Ln(1+Citation_PP)*	*Ln(1+Claims)*	*Ln(1+GrantLag)*
	(1)	(2)	(3)	(4)
$IncreaseEnforce_t$	-0.039***	-0.063**	-0.017	-0.037***
	(-2.91)	(-2.55)	(-0.57)	(-3.70)
$IncreaseEnforce_t \times Cut\_RD_t$	-0.111**	-0.149	-0.162*	-0.213***
	(-2.48)	(-0.27)	(-1.77)	(-3.14)
PPE	-0.483***	-0.224	-0.076	0.071
	(-3.87)	(-1.17)	(-0.83)	(1.07)
Tobinsq	0.020***	0.009**	0.002	0.004***
	(5.55)	(2.03)	(0.47)	(3.50)
Ln(Firmage)	0.059***	-0.106***	-0.017	-0.029***
	(3.36)	(-3.36)	(-0.85)	(-3.83)
Lev	-0.072	0.061	-0.011	-0.035
	(-1.15)	(0.62)	(-0.11)	(-0.77)
Ln(Sales)	0.123***	-0.081***	0.022	0.002
	(5.90)	(-2.79)	(1.27)	(0.44)
AnnRet	0.011	-0.003	0.008	0.000
	(1.65)	(-0.25)	(1.15)	(0.05)
Ln(PPE/#Employees)	0.118***	0.017	0.018	-0.010
	(4.91)	(0.39)	(0.93)	(-0.89)
IO	-0.079*	0.024	0.074	-0.012
	(-1.86)	(0.36)	(1.54)	(-0.80)
AnalystFollowing	0.094***	0.016	-0.001	0.003
	(4.63)	(0.64)	(-0.09)	(0.38)
HerfIndx	-1.210	-0.502	0.439	-0.049
	(-1.59)	(-0.81)	(0.87)	(-0.19)
$HerfIndx^2$	1.976	1.608	-1.573	0.182
	(0.80)	(0.78)	(-1.23)	(0.24)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Adj. R-square	0.839	0.597	0.373	0.373
No. of Obs.	20,929	11,252	11,252	11,252

Notes: The sample period is 1992 to 2003. All continuous variables are winsorized at the 1% and 99% percentiles. See Appendix A for the definitions of the variables. Intercepts are not reported. The numbers in parentheses are *t*-statistics, corrected based on the Huber-White sandwich estimate of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 3 *(continued)*

Panel B: Advertising expenses efficiency

	$MktShr_t$	$MktShr_{t+1}$	$MktShr_{t+2}$	$MktShr_{t+3}$
	(1)	(2)	(3)	(4)
$IncreaseEnforce_t$	-0.027	-0.023	-0.024	-0.035
	(-1.59)	(-1.16)	(-0.91)	(-0.96)
$IncreaseEnforce_t \times Cut\_Adv_t$	-1.287**	-0.506	-0.699**	-0.671
	(-2.38)	(-1.51)	(-2.26)	(-0.96)
Ln(Firmage)	-0.098**	-0.122**	-0.129	-0.119
	(-2.47)	(-2.03)	(-1.58)	(-1.16)
Ln(Debt)	0.034**	0.018	0.024	0.037**
	(2.17)	(1.13)	(1.32)	(2.04)
Ln(Assets)	0.095***	0.142***	0.140***	0.152***
	(3.18)	(4.48)	(5.18)	(5.08)
Ln(Sales)	0.161***	0.154***	0.166***	0.140***
	(7.07)	(5.07)	(4.87)	(3.59)
Ln(State unemployment)	0.206	-0.025	-0.399	-1.015
	(0.29)	(-0.03)	(-0.37)	(-0.75)
Ln(State personal income)	0.140	0.031	-0.081	-0.276
	(0.67)	(0.14)	(-0.32)	(-0.91)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.950	0.950	0.951	0.955
No. of Obs.	27,893	27,870	25,917	22,350

Panel C: SG&A (excluding advertising and R&D) expenses efficiency

	Adj_ROE_t	Adj_ROE_{t+1}	Adj_ROE_{t+2}	Adj_ROE_{t+3}
	(1)	(2)	(3)	(4)
$IncreaseEnforce_t$	0.031	0.003	0.002	-0.018
	(0.85)	(0.19)	(0.05)	(-0.89)
$IncreaseEnforce_t \times Cut\_SGA_t$	0.112	-0.042	-0.078	-0.051***
	(0.33)	(-0.20)	(-1.14)	(-3.20)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.656	0.680	0.661	0.668
No. of Obs.	27,893	27,615	25,441	21,753

Notes: The sample period is 1992 to 2004 for Panels B and C. The regression model in Panel C includes the same set of control variables as those included in Panel B. All continuous variables are winsorized at the 1% and 99% percentiles. See Appendix A for the definitions of the variables. Intercepts, and control variables in Panel C are not reported. The numbers in parentheses are *t*-statistics, corrected based on the Huber-White sandwich estimate of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 3 *(continued)*

Panel D: Growth opportunity and the impact of non-compete enforceability on future firm performance

	Adj_ROE_t	Adj_ROE_{t+1}	Adj_ROE_{t+2}	Adj_ROE_{t+3}		$ARET_t$	$ARET_{t+1}$	$ARET_{t+2}$	$ARET_{t+3}$
	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)
$IncreaseEnforce_t$	0.017	0.034	-0.003	0.021	$IncreaseEnforce_t$	-0.112	-0.114	-0.103**	-0.032
	(0.28)	(1.02)	(-0.08)	(0.43)		(-1.25)	(-1.43)	(-2.51)	(-1.17)
$IncreaseEnforce_t \times MB_{t-1}$	0.001	-0.014	0.004	-0.007**	$IncreaseEnforce_t \times MB_{t-1}$	0.023*	0.010	0.018	-0.005**
	(0.08)	(-1.29)	(0.43)	(-2.65)		(1.72)	(1.58)	(1.46)	(-2.47)
MB_{t-1}	0.105***	0.029***	0.008	0.030	MB_{t-1}	-0.070***	-0.028***	-0.013***	-0.001
	(13.03)	(4.18)	(1.42)	(0.58)		(-15.02)	(-19.78)	(-3.10)	(-0.22)
Ln(Firmage)	0.125***	0.044	0.016	0.588***	*TA*	-0.589***	-0.405***	-0.444***	-0.366***
	(4.04)	(1.27)	(0.34)	(16.45)		(-16.37)	(-23.24)	(-16.75)	(-24.85)
Ln(Debt)	0.596***	0.648***	0.678***	-1.209***	*Lev*	0.938***	0.402***	0.364***	0.360***
	(19.79)	(17.78)	(12.42)	(-24.84)		(12.31)	(9.39)	(4.91)	(7.52)
Ln(Assets)	-1.120***	-1.308***	-1.374***	0.307***	*MOM*	-0.104***	-0.169***	-0.235***	-0.094***
	(-23.67)	(-23.53)	(-17.05)	(12.07)		(-11.16)	(-15.13)	(-13.55)	(-9.35)
Ln(Sales)	0.180***	0.289***	0.341***	-0.088					
	(7.28)	(12.01)	(13.92)	(-0.22)					
Ln(State unemployment)	0.545	0.596*	-0.450	-0.209					
	(1.04)	(1.91)	(-0.91)	(-1.09)					
Ln(State personal income)	-0.329	-0.110	-0.338*	0.030					
	(-1.23)	(-0.79)	(-1.81)	(0.58)					
Firm fixed effects	Yes	Yes	Yes	Yes	Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Year fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.725	0.686	0.663	0.667	Adjusted R^2	0.143	0.123	0.090	0.087
No. of Obs.	27,893	27,615	25,441	21,753	No. of Obs.	27,235	25,358	25,129	23,001

Notes: The sample period is 1992 to 2004 for Panel D. All continuous variables are winsorized at the 1% and 99% percentiles. See Appendix A for the definitions of the variables. Intercepts are not reported. The numbers in parentheses are *t*-statistics, corrected based on the Huber-White sandwich estimate of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 4
The effect of non-compete enforceability: subsample tests based on industry and CEO characteristics.

Panel A: Localized versus national industries

	Meet	*Cut_Disx*	*Ln(1+Patent)*	*Ln(1+Citation_PP)*	*Ln(1+Claims)*	*Ln(1+GrantLag)*	$MktShr_t$	Adj_ROE_{t+3}	$ARET_{t+3}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
IncreaseEnforce	0.002	-0.002	0.027	0.052	0.011	-0.032***	-0.032	0.033	0.034
	(0.03)	(-0.23)	(1.47)	(1.48)	(0.28)	(-2.84)	(-0.80)	(1.33)	(0.79)
IncreaseEnforce×InStateCompete	0.686**	0.391***	-0.060**	-1.927***	-0.384*	-0.027*	-0.715**	-0.865***	-1.236***
	(2.33)	(3.68)	(-2.06)	(-4.72)	(-1.78)	(-1.73)	(-2.40)	(-6.31)	(-5.03)
InStateCompete	0.762	0.004	0.023	-0.347	0.078	-0.010	-1.132**	-0.051	-0.077
	(1.50)	(0.04)	(0.86)	(-1.35)	(0.83)	(-0.72)	(-2.05)	(-0.28)	(-0.46)
Pseudo R^2 / Adjusted R^2	0.287	0.743	0.843	0.602	0.382	0.382	0.898	0.484	0.093
No. of Obs.	25,678	25,678	19,183	10,431	10,431	10,431	25,609	19,915	21,052

Panel B: CEO ability

	Meet	*Cut_Disx*	*Ln(1+Patent)*	*Ln(1+Citation_PP)*	*Ln(1+Claims)*	*Ln(1+GrantLag)*	$MktShr_t$	Adj_ROE_{t+3}	$ARET_{t+3}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
IncreaseEnforce	0.046**	0.021**	-0.106***	-0.098***	-0.058**	-0.038***	-0.075*	-0.232*	-0.082**
	(2.02)	(2.52)	(-9.18)	(-3.36)	(-1.99)	(-4.32)	(-1.81)	(-1.74)	(-2.26)
IncreaseEnforce×Ability	-0.220**	-0.041**	0.033**	0.020	0.075*	0.096**	0.403***	0.524**	0.033
	(-1.98)	(-2.35)	(2.46)	(0.42)	(1.74)	(2.46)	(2.93)	(2.03)	(1.61)
Ability	-0.175	-0.010	0.006	0.006	-0.082***	0.025	0.262***	0.536***	0.019**
	(-1.58)	(-0.38)	(0.38)	(0.30)	(-6.63)	(1.04)	(2.85)	(3.04)	(2.24)
Pseudo R^2 / Adjusted R^2	0.148	0.710	0.819	0.598	0.360	0.373	0.879	0.219	0.095
No. of Obs.	27,789	27,789	20,889	11,236	11,236	11,236	27,881	21,691	22,936

Table 4 *(continued)*

Panel C: CEO tenure

	Meet	*Cut_Disx*	*Ln(1+Patent)*	*Ln(1+Citation_PP)*	*Ln(1+Claims)*	*Ln(1+GrantLag)*	$MktShr_t$	Adj_ROE_{t+3}	$ARET_{t+3}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
IncreaseEnforce	0.174**	0.029**	-0.021**	-0.151**	-0.116***	-0.028*	-0.072**	-0.326***	-0.077**
	(2.00)	(2.47)	(-2.38)	(-2.15)	(-4.96)	(-1.70)	(-2.11)	(-2.85)	(-2.35)
IncreaseEnforce×*Tenure*	-0.007**	-0.004**	0.112	0.013**	0.098*	0.031**	0.005***	0.006**	0.006***
	(-2.18)	(-2.24)	(0.95)	(2.54)	(1.70)	(2.49)	(5.60)	(2.49)	(5.84)
Tenure	-0.002	-0.008***	-0.015	-0.007**	-0.018	-0.003	0.002	0.001	0.002
	(-0.77)	(-7.74)	(-0.58)	(-2.49)	(-1.29)	(-0.36)	(1.37)	(0.41)	(1.23)
Pseudo R^2 / Adjusted R^2	0.163	0.754	0.872	0.643	0.384	0.420	0.952	0.245	0.105
No. of Obs.	12,051	12,051	7,841	5,355	5,355	5,355	11,545	9,382	10,230

Notes: This table presents the results of cross-sectional tests. The sample period is 1992 to 2003 for innovation output tests reported in columns (3) to (6) and 1992 to 2004 in all other columns. Within-state competition is measured as the sum of the sales revenues of all competing firms in the same state with the same industry membership (Fama-French 48 industry) divided by the total sales of that industry. CEO ability measure is obtained from Demerjian et al. (2012), and CEO tenure is from Execucomp. Control variables, firm fixed effects, state fixed effects, industry fixed effects, and year fixed effects are the same as those reported in Tables 2 and 3 in the columns of the same dependent variables. All continuous variables are winsorized at the 1% and 99% percentiles. See Appendix A for the definitions of the variables. Intercepts and control variables are not reported. The numbers in parentheses are *z*-statistics in column (1) and *t*-statistics in other columns, and are corrected based on the Huber-White sandwich estimates of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 5
Additional analysis and sensitivity tests

Panel A: Other earnings management tools

	AM	*RM_Prod*	*RM_CFO*
IncreaseEnforce	0.005*	0.011**	0.004
	(1.78)	(2.17)	(0.69)
Adjusted R^2	0.199	0.715	0.490
No. of Obs.	27,964	27,964	27,964

Panel B: Time-dynamic analysis

Florida [Increased enforceability]

	Meet	*Cut_Disx*
Pre(1)	-0.017	0.006
	(-0.52)	(1.29)
Post(0)	-0.020	0.007
	(-0.51)	(1.26)
Post(1)	0.177***	0.014*
	(3.43)	(1.85)
Post(2+)	0.126***	0.018**
	(2.91)	(2.09)
Pseudo R^2 / Adjusted R^2	0.147	0.735
No. of Obs.	25,375	25,375

Texas [Decreased enforceability]

	Meet	*Cut_Disx*
Pre(1)	0.027	-0.006
	(0.60)	(-1.17)
Post(0)	0.031	0.005
	(0.41)	(0.83)
Post(1)	-0.150**	-0.013*
	(-2.04)	(-1.88)
Post(2+)	-0.120**	-0.019**
	(-2.11)	(-2.43)
Pseudo R^2 / Adjusted R^2	0.132	0.733
No. of Obs.	26,779	26,779

Louisiana [Increased enforceability]

	Meet	*Cut_Disx*
Pre(1)	-0.670***	-0.034***
	(-5.66)	(-2.60)
Post(0)	0.376***	0.001
	(4.81)	(0.05)
Post(1)	0.298***	0.045**
	(2.98)	(2.53)
Pseudo R^2 / Adjusted R^2	0.105	0.766
No. of Obs.	5,170	5,170

Notes: This table presents the results of various sensitivity tests. Panel A reports the results on accruals and other real transactions management. The sample period is 1992-2004. The control variables, firm fixed effects, and year fixed effects are the same as those in Panel D of Table 2, except that we control for total real earnings management (the sum of *Cut_Disx*, *RM_Prod*, and *RM_CFO*) instead of *AM* when the dependent variable is *AM*. Panel B reports the results of time-trend tests. The sample period is 1992-2004 for Florida and Texas, and 2001-2004 for Louisiana. *Pre(1)* is a dummy variable equal to 1 for firms in Florida, Texas, or Louisiana one year prior to an event of enforceability change (i.e., year

t-1, where *t* is the event year for an enforceability change), and 0 otherwise. *Post(0)*, *Post(1)*, and *Post(2+)* are indicator variables that are set to 1 for firms in a treatment state in an event year (year *t*), one year after an event (year *t*+1), and two or more years after an event (year *t*+2 or beyond), respectively, and 0 otherwise. The control variables, firm fixed effects, state fixed effects, industry fixed effects, and year fixed effects in Panel B are the same as those in Tables 2 and 3 in the columns of the same dependent variables. All continuous variables are winsorized at the 1% and 99% percentiles. See Appendix A for the definitions of the variables. Intercepts and control variables are not reported. The numbers in parentheses are *z*-statistics in column (1) in Panel B and *t*-statistics in the other columns, and are corrected based on the Huber-White sandwich estimates of variances and adjusted for clustering by state. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.