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Labor Market Mobility and Expectation Management: Evidence from Enforceability of Non-Compete Provisions*

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ABSTRACT

This study examines how managers' use of expectation management is affected by their labor market mobility, which we measure by the enforceability of non-compete provisions in their employment contracts. Exploiting quasi-natural experiments, our difference-in-differences analyses show that managers in U.S. states that tightened enforcement of non-compete provisions are more likely to manage analyst expectations downward, consistent with labor market immobility exacerbating managers' incentives to ensure that earnings expectations are met. We also find that downward expectation management is used to a greater extent than other tools such as real and accrual-based earnings management. Additional analysis shows that the increase in expectation management is more pronounced for CEOs with lower general skills or shorter tenures, for firms with more independent boards, and for industries that are more homogeneous. Our path analysis suggests a significant link between increased use of expectation management after tightened non-compete enforcement and meeting and beating earnings expectations, which in turn is linked to lower executive turnover. Overall, our findings suggest that expectation management is an important channel through which non-compete enforcement reduces executive labor market mobility.

Keywords: Labor Market Mobility; Non-Compete Enforcement; Expectation Management; Managerial Career Concern
JEL Classification: G14; J60; K12; M41; M50
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Labor Market Mobility and Expectation Management: Evidence from Enforceability of Non-Compete Provisions

1. Introduction

This study examines the effect of managers' labor market mobility on their use of disclosures to lower analysts' earnings expectations to achievable levels prior to earnings announcements (henceforth referred to as expectation management). Failure to achieve analyst expectations has several well-documented negative consequences (Skinner and Sloan 2002; Bhojraj et al. 2009). Graham et al. (2005) survey over 400 corporate executives and document that intra-industry mobility is regarded as one of the most important motivations for avoiding missing earnings targets, because "failing to meet earnings benchmarks can inhibit the upward or intra-industry mobility" of executives for fear of being perceived as incompetent (p. 28). While Graham et al. (2005) do not explicitly ask how managers achieve earnings targets, the phrase "managing analysts' expectations" came up voluntarily in 11 of 20 interviews. Yet there is scant empirical evidence on the causal effect of job market mobility on managers' use of expectation management.

We exploit shocks to statewide labor market mobility that are plausibly exogenous to individual managers to test the impact on managers' use of expectation management. Specifically, we identify changes in state-level enforcement of non-compete provisions, which restrict employees' ability to join competitors or start new competing businesses after they leave their former employment (Garmaise 2011).[1] We conduct a battery of empirical analyses to provide the first direct evidence on (a) how labor market mobility impacts managers' use of expectation management, (b) the economic factors that moderate such an impact, thus shedding light on the underlying mechanisms, and (c) whether the use of expectation management in response to labor market mobility helps managers avoid forced turnovers.

Non-compete provisions restrict employees from joining or forming rival firms for a certain period after leaving former employers (Gilson 1999; Bishara et al. 2015), and they can be triggered by both voluntary resignations and forced terminations (Vanko 2002; Malsberger 2004; Schwab and Thomas 2006). The number of published court decisions involving non-compete agreements in the United States (U.S.)

[1] We use the terms non-compete provisions, agreements, or covenants interchangeably throughout this paper.

has risen by 61% to 760 cases between 2002 and 2012 (Simon and Loten 2013). Although such provisions are commonly used in employment contracts, particularly for top executives and specialized technicians, the enforceability of non-compete provisions in the U.S. is governed by state law, and thus varies across states. Some states enforce them more vigorously than others, resulting in cross-sectional variations exogenous to individual firms' actions (U.S. Department of the Treasury 2016).[2] Previous studies show that stricter enforcement of these provisions reduces managers' labor market mobility, limiting their alternative employment opportunities and imposing higher job-switching costs (Vanko 2002; Garmaise 2011). Yet, as a result of increased non-compete enforcement, the supply of executives in the labor market also shrinks, making it more costly for firms to hire new executives to replace outgoing ones, which may mitigate the incumbent managers' career concerns (Simon and Loten 2013). Hence, managers' career concerns can be either heightened or reduced after the enforcement of these provisions increases. Whether managers in states with higher non-compete enforcement engage in more or less expectation management to minimize the likelihood of missing earnings targets, and ultimately, to limit their risk of exposure to forced terminations in a less mobile labor market, is an important empirical question.

Using changes in an enforceability index of non-compete provisions across U.S. states available in prior studies (Garmaise 2011), supplemented by hand-collected data, we identify a list of events of non-compete enforcement changes during 1992–2013. These changes result from state legislatures or court rulings, and thus are exogenous to individual firm managers. We use a difference-in-differences (diff-in-diff) design to exploit these quasi-natural experiments, and we find that managers in states with increased enforcement are more likely to guide analysts' expectations downward to minimize their likelihood of missing earnings targets. This effect is also economically significant. An increase in enforcement leads to an increase in expectation management of about 14% of the mean value, *ceteris paribus*. This significant relationship is also robust when we consider various potential alternative explanations and control for

[2] Whether or not non-compete agreements are enforced in a given state is a relative concept. Specifically, states where non-competes appear not to be enforced (e.g., California) essentially impose higher requirements on non-competes to be deemed enforceable. While these states deem non-competes unenforceable unless they meet all the requirements (i.e., "red pencil" doctrine), a majority of states modify overbroad non-compete contracts to render them enforceable (e.g., "blue pencil" and "equitable reform" doctrines) (U.S. Department of the Treasury 2016).

confounding factors at different levels.[3] The results also hold in our trend analyses that account for other unobservable factors and do not hold in the falsification test as expected. Interestingly, we find no evidence that managers in states with decreased non-compete enforcement reduce their use of expectation management, which is consistent with the relatively lower cost of expectation management compared to other reporting tools in achieving earnings targets, and the stickiness of managerial career concern that does not relax after a decrease in non-compete enforcement (Skinner 1994, 1997; Graham et al. 2005; Koh et al. 2008). Our results continue to hold after controlling for CEO compensation structures, suggesting that the reduction of the supply of executives in the labor market after increased non-compete enforcement is likely not a dominate factor for managerial career concerns. Our additional analyses reveal that expectation management contributes to achieving earnings targets four times as much as alternative reporting tools do in our setting (e.g., accrual management or real earnings management) (Das et al. 2011; Chen et al. 2018). Overall, our findings are consistent with the argument that increases in the enforceability of non-compete provisions impose greater pressures on managers to achieve earnings targets, inducing them to engage in more downward expectation management.

Next, we conduct cross-sectional analyses to better understand whether and how various factors at the manager, firm, and industry levels moderate the effect of state-level labor market mobility on managers' use of expectation management. At the manager level, we expect CEOs with more general skills to be less affected by non-compete enforcement than CEOs with skills more specific to their given industries (Custódio et al. 2013), because generalists have more opportunities outside the industries restricted by non-compete provisions (Garmaise 2011). In addition, top managers' career concerns vary over time with their seniority. Prior research suggests that more junior CEOs are more susceptible to forced turnovers based on poor firm performance due to a lack of a solid track record (Gibbons and Murphy 1992; Hermalin and Weisbach 1998). We expect newly appointed CEOs working with other C-suite colleagues more senior to them to have a greater incentive to avoid reporting poor firm performance, and hence are more sensitive to

[3] The factors we consider include the manager level (the presence and amount of severance pay, Cadman et al. [2016]; compensation structure, Garmaise [2011]), the firm level (meeting-and-beating string in the past, Kross et al. [2011]; analysts' initial forecast errors, and thus the need to correct their expectation, Ke and Yu [2006]), the industry level (the tightness of the labor market, Parrino [1997]; the costs of hiring new managers, Gao et al. [2015]), and the state level (change in GDP).

increases in non-compete enforcement. Consistent with our predictions, we find that CEOs with less general managerial skills or shorter tenures at the current firm increase expectation management more when facing greater non-compete enforcement. At the firm level, we expect the monitoring of CEOs to increase their turnover-performance sensitivity (Weisbach 1988), which would magnify their motivation to mitigate the risks of forced turnover through expectation management and ensure that they meet earnings targets. By measuring the monitoring from the board of directors (the percentage of independent members) (Guo and Masulis 2015), we find that the effect of non-compete enforcement on expectation management is driven by managers that are monitored by more independent boards. Lastly, at the industry level, we contend that non-compete provisions restrict labor market mobility more effectively in more homogenous industries, because the provision is designed to prohibit employees from joining or starting a rival firm that *directly* competes with the former employer. Consistent with this, we find that the impact of non-compete provisions on expectation management is driven by firms in more homogeneous industries. These cross-sectional variations suggest that state-level enforcement of non-compete provisions interacts with various factors of managerial career concerns in shaping managers' use of expectation management, and therefore shed more light on the underlying mechanism through which labor market mobility affects disclosure choices.

Finally, Garmaise (2011) documents that executives in high-enforcement jurisdictions have longer tenures (i.e., lower turnover). We further examine whether the lower turnover can be explained by the actions taken by managers to secure jobs in response to non-compete enforcement increases (i.e., reaction effect). Using a path analysis, we find supporting evidence: after increases in non-compete enforcement, managers using more expectation management are more likely than their peers to meet or beat earnings expectations, and thus are less likely to be dismissed. Together, the results from our path analysis suggest that managers' use of expectation management—an important aspect of corporate disclosure practice—can be a crucial contributor to the lower executive turnover seen after an increase in non-compete enforcement.

Our study contributes to the prior literature in several ways. First, our study extends the emerging literature on managerial career concerns and corporate disclosure policy (Kothari et al. 2009; Brown 2015) by documenting a direct impact of labor market mobility on managers' use of expectation management (Bartov et al. 2002; Das et al. 2011). Unlike most prior research that uses manager or firm characteristics

(e.g., CEO age, tenure, industry competition, and severance pay) to measure managerial career concerns (Pae et al. 2016; Baginski et al. 2018),[4] our quasi-experimental setting and diff-in-diff design allow us to focus on intra-industry mobility, a factor that prior surveys find especially concerning to managers (Graham et al. 2005, 2006), and draw clearer inferences about its impact on managers' use of expectation management. Chen et al. (2018), who also use the same setting as ours, is a notable exception but they focus only on earnings management. Our study extends theirs by examining expectation management, an important tool used increasingly by firm managers to meet or beat earnings targets (MBE) (Matsumoto 2002; Das et al. 2011).[5] By considering multiple tools for MBE, we find that in response to reduced labor market mobility, managers use more expectation management than both accrual earnings management and real earnings management. In doing so, our study paints a fuller picture on the comprehensive effect of labor market mobility on firms' disclosure practices, and also provides direct empirical evidence to buttress Graham et al. (2005)'s survey finding that intra-industry mobility is one of managers' top concerns when deciding on their corporate disclosure practice, and that managers prefer expectation management to real earnings management to ensure that they meet earnings expectations.[6]

Second, our cross-sectional findings complement prior studies that focus on the average effect of managers' use of expectation management by providing a three-level framework to shed light on the underlying links between labor market mobility and firms' disclosure choices (Bartov et al. 2002; Das et al. 2011; Chen et al. 2018), a topic that warrants further investigation (Healy and Palepu 2001; Graham et al.

[4] For example, recent studies have used these proxies for managerial career concern to examine the effects on firms' financial disclosure choices in earnings management (Brown 2015) and in management guidance (Pae et al. 2016). Also, many recent studies examine another common contractual agreement, the severance pay (Rau and Xu 2013; Brown 2015; Cadman et al. 2016; Baginski et al. 2018). In later sections, we further discuss these studies and elaborate on how our measure of labor market mobility differs from these measures.

[5] Our study differs from Chen et al. (2018) in four additional ways. First, we refine the sample by hand-collecting events of non-compete enforcement in recent years and extend the sample from 2004 to 2013. Second, Chen et al. (2018) focus only on the *employee's* perspective of labor market mobility change, whereas our study broadens the notion of labor market mobility to also encompass the *employer's* perspective. Third, our cross-sectional analyses also study a firm-level factor (i.e., corporate governance), in addition to the manager- and industry-level factors as in Chen et al. (2018). Lastly, Chen et al. (2018) rely on Garmaise (2011)'s finding that stricter enforcement of non-compete provisions results in a less mobile labor market for executives, whereas we conduct a path analysis to examine whether the mobility effect is in fact a result of managerial reactions to enforcement changes. We elaborate on these later in the paper.

[6] For example, while the phrase "managing analysts' expectations" was voluntarily mentioned in most interviews in Graham et al. (2005)'s survey without the authors explicitly asking managers how they influence earnings targets, Graham et al. (2006)'s survey finds that most managers agree or strongly agree that their firms may use real earnings management to meet analysts' expectations.

2005; Beyer et al. 2010). Taking the firm-level factor (not examined in Chen et al. [2018]) as an example, we find a greater effect of labor market mobility for managers subject to stronger monitoring, which increases their turnover-performance sensitivity. This contrasts to the prior finding that corporate monitoring can reduce managers' opportunistic behaviors (see Armstrong et al. [2010] for a review), and suggests that monitoring may incentivize managers to manipulate information flow (Baginski et al. 2018) and manage performance targets to make themselves appear more able (Hermalin and Weisbach 2012). Our study has practical implications for corporate boards and shareholders when evaluating the overall effect of monitoring on managers' opportunistic behaviors.

Finally, our study contributes to the legal and management literature and policy debate on the costs and benefits of non-compete employment provisions (see Bishara and Thomas [2015] for a review). While many legal scholars contend that non-competes are valuable contractual mechanisms that protect employers from their investment in human capital being appropriated by competitors (Posner and Triantis 2001; Posner et al. 2004), researchers have also documented some negative consequences of non-compete enforcement in areas such as firms' reduced investment per-capita (Garmaise 2011), less efficient employer-employee matching (Marx et al. 2009), and non-compete enforcement being an impediment to entrepreneurship and innovation (Samila and Sorenson 2011; Chen et al. 2018). Our study considers the countervailing effects of labor market mobility on managerial career concerns from both the supply and demand side of the managerial labor market, and thus deepens our understanding of how contracting enforcement affects managers' incentives and disclosure practices. Our findings also suggest that such changes in managerial incentives and disclosure practices are an important reaction mechanism that explains the reduced labor market mobility as documented in prior literature. Given that non-compete provisions are common in the U.S., our evidence of a significant enforcement effect across states contributes to the growing literature that highlights the importance of enforcement among other forces (Christensen et al. 2013; Dou et al. 2013).

2. Background and hypothesis development

Achieving earnings targets, and expectation management

Research in corporate finance suggests that the managerial labor market uses firm performance to assess managers' ability (Hermalin and Weisbach 1998; Holmstrom 1982, 1999). Hence, poor firm

performance can damage managers' reputation and even lead to early terminations (Farrell and Whidbee 2003). In particular, missing analyst expectations is costly to managers' reputations (Graham et al. 2005), and therefore managers are under substantial pressure to achieve earnings targets to avoid unfavorable assessment in the labor market (Kim 1999; Nagar 1999; Verrecchia 2001).

To achieve earnings expectation targets that would otherwise not be met, managers can either manage earnings upward (i.e., earnings management) or manage expectations downward (i.e., expectation management) prior to reporting actual earnings.[7] Recent research suggests that firms increasingly shift from earnings management to expectation management to reach earnings targets, likely due to increased regulatory scrutiny and litigation risks for accounting fraud and manipulation (Graham et al. 2005; Koh et al. 2008). Yet, expectation management bears costs. When market expectations are managed downward, stock prices decline (Kothari et al. 2009). But on average, prior research suggests that the benefit of managing expectations (i.e., releasing bad news before earnings announcements) outweighs the cost of missing earnings targets (i.e., withholding bad news until announcing actual earnings) (Bartov et al. 2002; Brown and Caylor 2005; Das et al. 2011). Therefore, we expect managers with greater concerns about missing earnings expectations to engage in more expectation management (Matsumoto 2002).[8]

Graham et al. (2005, 28) note that failing to achieve earnings targets can "inhibit the upward or intra-industry mobility" of managers because they are viewed as incompetent. Graham et al. (2005, 42) also report that it is common for managers to guide analysts' forecasts "to a number that is less than the internal target so as to maximize the chances of a positive surprise." These findings suggest that labor market

[7] The expectation management literature suggests that managers are not passive observers of market expectations, but active guiders of analysts' expectations (Bartov et al. 2002; Matsumoto 2002; Das et al. 2011).

[8] With a different sample and focus, Kothari et al. (2009, 247) (also, Baginski et al. 2018 and Ali et al. 2019) suggest that managers with greater career concerns are more likely to withhold bad news "in the hope of an eventual turnaround." However, based on short-window market reactions to positive and negative management disclosures, their evidence only suggests that managers delay bad news *relative to good news*. Managers may still release bad news to manage expectations downward during the quarter, which we examine in our paper. Note that even if managers tend to withhold bad news relative to when they receive the bad news, they cannot delay it indefinitely (Roychowdhury and Sletten 2012). Having to release earnings in each quarter's mandatory reports gives managers an incentive to manage expectations downward to avoid a negative earnings surprise (Bartov et al. 2002; Matsumoto 2002; Das et al. 2011). Thus, despite prior evidence that managers with greater career concerns are more likely to withhold bad news (Kothari et al. 2009; Baginski et al. 2018; Ali et al. 2019), career concerns can also prompt them to avoid negative earnings surprises, implying more downward expectation management (Graham et al. 2005). Thus, it is possible that managers with greater career concerns are more likely to release bad news prior to actual earnings releases, while still delaying bad news relative to when they receive such bad news.

mobility is likely to have a first-order impact on managers' incentive to achieve earnings targets and the use of expectation management. However, the direction of such impact is unclear, as we discuss later in our hypothesis development section.

Managerial career concerns, labor market mobility and non-compete provisions

While the managers surveyed by Graham et al. (2005) acknowledge that intra-industry mobility is one of their top motivations to ensure meeting or beating analyst expectations, theoretical and empirical developments are lacking in the literature. We posit that labor market mobility can affect managers' career concerns through two distinct channels. The first is from the employees' (i.e., the managers') perspective. As labor market mobility decreases, it will take managers more time and effort to find a new employer should they leave their current job, making job-switching costlier. Hence, this channel predicts that managers' career concerns will be exacerbated as labor market mobility declines. The second channel is from the employers' (i.e., the firms') perspective. As labor market mobility decreases, firms also face a reduced supply of managers. Thus, firms are less willing to fire current managers, which mitigates these incumbent managers' career concerns. Given these two countervailing channels, it boils down to an empirical question as to how labor market mobility affects managerial career concerns.

Some prior studies have examined the effect of managerial career concerns (e.g., Pae et al. 2016) but not the effect of labor market mobility, which managers acknowledged as an important factor in their disclosure decisions (Graham et al. 2005). Moreover, these studies usually use managerial traits or behaviors that are endogenous to measure career concerns, making it difficult to draw causal inference. For example, common proxies for managerial career concerns in the literature, such as executive age or tenure (Cassell et al. 2013; Pae et al. 2016), are determined endogenously through either self-selection or omitted correlated variables. Career concern measures based on observed turnovers are also subject to selection bias because CEO turnovers occur for non-random reasons (e.g., poor firm performance); hence, CEOs that experienced turnovers are not representative of all CEOs (departing, incoming, and remaining CEOs) in terms of their intra-industry career mobility (Faleye et al. 2014). Although *ex ante* measures based on managers' performance, visibility, or severance pay partially mitigate the selection bias, they are still endogenous as they can be affected by managers' actions or ability (Rajgopal et al. 2006). Taken together,

it is difficult to examine the causal relationship between managerial labor market mobility and managerial responses based on the conventional proxies, due to the endogeneity concern described above.

In this study, we measure labor market mobility based on the enforceability of non-compete agreements in the U.S., which are common provisions in employment contracts to prohibit managers from joining or starting rival firms for a specified period after leaving their employers (Garmaise 2011). Enforcing these provisions reduces executives' job mobility by restricting their employment opportunities and imposing legal costs on executives who switch to jobs or establish new businesses in similar or related industries after leaving their former employers (Gilson 1999; Samila and Sorenson 2011).[9] Note that non-compete provisions can be triggered by resignations as well as forced terminations due to poor performance.[10] The number of published U.S. court decisions involving non-compete agreements rose by 61% to 760 cases between 2002 and 2012 (Simon and Loten 2013). One crucial fact contributing to the effect of non-compete enforcement on managers' intra-industry mobility is that the majority of CEOs work in no more than one industry over their entire career (Custódio et al. 2013; Pae et al. 2016), suggesting that switching jobs across industries is rare for CEOs, and hence non-compete provisions are likely binding for most CEOs seeking to switch jobs. The same argument also applies to firms seeking to hire CEOs externally.

Non-compete provisions are widely used in the U.S., but their enforceability varies across states (Samila and Sorenson 2011; Garmaise 2011). As managers' litigation risks and expected settlement costs of job-switching increase with the enforcement of non-compete provisions, the managerial labor market in high-enforcement states becomes less mobile, which affects managers' career concerns in two distinct ways as discussed earlier. First, from the managers' perspective, it is costlier to find a new job after losing jobs in high-enforcement states, and hence their career concern increases. Second, from the employers'

[9] Upon detecting violations of such provisions, former employers often demand hefty fines and impose restrictions on their former employees. For example, Capital One brought a lawsuit against two former employees, John Kanas and John Bohlsen, on charges of violating their non-compete provisions after they became CEO and Chief Lending Officer, respectively, of BankUnited. In June 2012, they agreed to pay a $20 million fine (about six times their annual salaries in 2011) to settle the case, and were restricted from expanding their business until January 2013 in the New York area where Capital One had a large presence. Another example is Amazon, which filed a suit against its former senior logistic leader Arthur Valdez, who was hired by Target after leaving Amazon. Although some new employers may shoulder some of the settlement costs, the higher job-switching cost from non-compete enforcement eventually reduces the labor market mobility of executives.

[10] Out of 100 randomly selected S&P 500 firms with signed employment contracts with their CEOs, 81 of them explicitly state that the non-compete provisions are applicable for any reason when the CEO is terminated, which is consistent with prior anecdotal and empirical evidence in the literature (Vanko 2002; Simon and Loten 2013).

perspective, they face a reduced supply of managers in the labor market in high-enforcement states, and hence face higher costs to fire and replace incumbent managers, which in turn can alleviate managers' career concerns. Unlike many other proxies of managerial career concern in the literature, non-compete enforcement depends on state jurisdictions that are exogenous to the actions of individual firms or managers, which mitigates the concern of omitted correlated variables and allows us to better draw causal inference from the quasi-natural experiments. We elaborate on the validity of this setting for our study in research design section, and discuss how managers are aware of non-compete provisions, especially changes in their enforcement.

Hypothesis development

In the previous sub-section, we introduced two channels through which non-compete enforcement can affect managers' career concerns. We expect the opposing effects to extend to the use of expectation management. The employees' (i.e., CEOs') channel predicts that managers' career concerns will be exacerbated as non-compete enforcement increases, as job opportunities will become more limited after a turnover. Managers in states with higher enforceability of non-compete provisions are under greater pressure to secure their jobs and minimize the risks of a forced turnover. One way to minimize the likelihood of missing analysts' earnings expectations when reporting actual earnings is to manage these expectation targets downward. This leads to our main hypothesis stated below in its alternative form:

> HYPOTHESIS 1. *Ceteris paribus, managers in states with stricter enforcement of non-compete provisions are more likely to manage analysts' earnings expectation downward than managers in states with no change in enforcement of non-compete provisions.*

From the employers' (i.e., firm) perspective, however, as non-compete enforcement increases, the supply of potential candidates to replace an outgoing manager is more limited, and hence employers are more reluctant to dismiss the incumbent CEO. This can partially offset incumbent CEOs' career concerns and dis-incentivize them to engage in more expectation management. As the employers' channel is indirect and may not offset the direct employee channel completely, we also state our main hypothesis in its null form below:

HYPOTHESIS 1 (null). *Ceteris paribus, managers in states with stricter enforcement of non-compete provisions are equally likely to manage analysts' earnings expectation downward compared with managers in states with no change in enforcement of non-compete provisions.*

Hypothesis 1 and Hypothesis 1 (null) above outline the average effects of changes in the enforceability of non-competes. Next, we posit how the effects of non-compete provisions may vary in the cross sections. To simplify our discussion, we formulate our cross-sectional predictions from only the employee's perspective, as it is more direct than the employer's perspective. We consider factors at the manager, firm, and industry levels.

First, we expect CEOs with more general skills to be less affected by non-compete enforcement than CEOs with skills more specific to their industries (Custódio et al. 2013), because generalists will have more opportunities outside the industries restricted by non-compete provisions (Garmaise 2011). In addition, top managers' career concerns are also likely to vary over time throughout their careers. CEOs with shorter tenures face greater pressure to establish or maintain a reputation as being competent as they lack a track record; hence, failing to meet earnings expectations is more devastating for these managers' job security (Gibbons and Murphy 1992; Rajgopal et al. 2006). In contrast, more experienced CEOs are less susceptible to terminations for temporary poor performance (Dikolli et al. 2014). Therefore, we expect non-compete enforceability to have a larger effect on CEOs with less general skills and shorter tenures, leading to the following cross-sectional hypothesis:

HYPOTHESIS 2(a). *Ceteris paribus, the effect in Hypothesis 1 (i.e., managers in states with stricter enforcement of non-compete provisions are more likely to manage analysts' earnings expectation downward) is more pronounced for CEOs with less general managerial skills and shorter tenures.*

Second, monitoring a firm's managers can moderate the effect of non-compete enforcement by altering the sensitivity of executive turnover to firm performance. In particular, prior research finds that having independent directors on a firm's board can serve a monitoring role and hence increase CEO turnover-performance sensitivity (Guo and Masulis 2015). Intensified monitoring puts more pressure on managers to meet earnings expectations. Thus, managers at these firms are more likely to be concerned about non-compete enforcement, because once terminated from their current employment, they will be

confronted with an even less mobile labor market. Therefore, we expect that non-compete enforceability has a greater effect on CEOs at firms with a greater percentage of independent directors on the board. This leads to our second cross-sectional hypothesis stated below:

> HYPOTHESIS 2(b). *Ceteris paribus, the effect in Hypothesis 1 (i.e., managers in states with stricter enforcement of non-compete provisions are more likely to manage analysts' earnings expectation downward) is more pronounced for CEOs at firms with boards that have a larger percentage of independent directors.*

Finally, we expect the effect of non-compete enforceability on labor market mobility to be industry-specific, because non-compete provisions specifically prohibit former employees from seeking new employment with employers that directly compete with the former employers. For industries that are highly homogeneous in their business models or that have common customer groups, the effect of non-compete provision enforcement is likely greater because the former employer can make a more compelling case that a former employee has caused damage by working for a competitor (Garmaise 2011; Bishara et al. 2015). This leads to our third cross-sectional hypothesis stated below:

> HYPOTHESIS 2(c). *Ceteris paribus, the effect in Hypothesis 1 (i.e., managers in states with stricter enforcement of non-compete provisions are more likely to manage analysts' earnings expectation downward) is more pronounced for CEOs at firms in more homogeneous industries.*

3. Empirical research design

Measurement of key variables

Non-compete enforceability

The enforcement of non-compete clauses is governed by state law and hence may change when state laws change or new court rulings emerge. We obtain enforceability changes of non-competes for all U.S. jurisdictions from two sources. First, following Garmaise (2011), we identify four enforceability changes during 1992-2003: Florida tightened non-compete enforceability in 1996 following a legislative act (*Fla. Sess. Law Serv. Ch. 96-257*); Texas loosened enforceability after its Supreme Court ruled on *Light v. Cellular Co.* (Texas 1994); Louisiana Supreme Court decreased non-compete enforceability in the *SWAT 24 Shreveport Bossier v. Board* case in 2001, but later the state legislature retightened non-compete enforceability in 2003 (*La. Sess. Law Serv. Act. 428*). These changes have been examined in prior studies

(Samila and Sorenson 2011; Chen et al. 2018). Second, we extend this initial sample beyond 2004 by manually searching for enforceability changes across the U.S.[11] We have identified eight more changes based on state court rulings and two more based on state legislations from 2004 to 2010.[12] Appendix 1 describes our searching procedures and reports the details of each enforcement change.[13] To ensure an effective diff-in-diff design, we require a post-event period of at least three years, and thus our analysis period is extended to 2013. We exclude observations of enforceability changes during 2011–2013 because they could confound a previous change.

To capture changes in the enforcement of non-compete provisions and their subsequent effects on expectation management, we construct the variable *IncreaseEnforce*, which is set to 1 (-1) for firms headquartered in states that tightened (loosened) non-compete enforceability (i.e., our treatment firms) for years after the enforcement changes. *IncreaseEnforce* is equal to 0 for all other observations, including treatment firms in the years prior to the change and control firms (i.e., those headquartered in states that did not experience any change in non-compete enforceability).[14]

We expect managers to have both the motivation and the means to stay informed of these events of non-compete enforcement changes. Managers' incentives to stay informed are two-fold. First, their own careers are affected by labor mobility, which is in turn affected by non-compete enforcement, as prior

[11] We start our hand-collection from 2004 as the enforcement index used by Garmaise (2011) ends in January 2004.

[12] For our main analyses, we include legislature changes in our sample for three reasons. First, doing so yields results that are more comparable to prior studies (e.g., nearly half of the events studied in Garmaise [2011] are legislature changes). Comparing our results to Garmaise (2011)'s findings helps explain CEO tenure changes following changes in non-compete enforcement. Second, including legislature-based change events produces a larger sample, increasing the statistical power of our tests. Third, understanding legislature-based changes has practical value for policy makers, considering that the U.S. Federal Trade Commission was recently under pressure by politicians, unions, and advocacy groups to ban non-compete clauses by federal laws. A potential concern of including the legislative changes is that they may result from firms' lobbying activities or hence may be anticipated by some managers. However, we find no evidence that firms increase expectation management in years prior to the state statutes, or that they relocate to states with (presumably) lower enforceability in years surrounding the changes. These findings mitigate endogeneity concerns of legislative changes of non-compete enforcement. For a further robustness check, we only focus on court-rulings-based changes; our results hold. Our results also hold if we only focus on legislature-based changes (untabulated).

[13] We conduct Google searches instead of more professional legal searches to ensure that our identified events are reasonably familiar to all managers, including those without a legal background. To ensure the validity of our events, we take additional measures as follows. First, we ask an independent legal expert to verify each event and its effects. Second, we conduct placebo tests to examine whether our results are driven by random events in our falsification tests and trend analyses section.

[14] To ensure that non-compete enforcement is relevant to our sample of firms, we exclude firms if they do not disclose the use of non-compete provisions for any of their executives during the entire sample period. See more details in our sample selection section. Our inferences do not change if we do not impose this restriction.

studies have shown (Garmaise 2011). Second, one responsibility of a CEO is to attract and retain key employees, who are likely the most valuable asset of a firm and are also affected by non-compete enforcement. Given both their personal interests and responsibilities, CEOs are motivated to closely and actively follow information on non-compete enforcement through several channels. First, executives are served by law firms that seek to sustain the relationship by periodically and routinely sending out news alerts to inform managers of the latest legal changes and highlighting the impact of these changes on their jobs. Second, frequent media coverage of non-compete provisions reminds managers to stay alerted about potential enforcement changes (Simon and Loten 2013; Starr 2019). Third, legal departments within firms also provide timely updates to executives about any legal changes, including on enforcement of non-compete agreements. We find supporting evidence that our results continue to hold for firms that have managers with law degrees or with general counsels in their C-Suites, despite a reduction in sample size by nearly 23% due to limited data coverage of executives' background (untabulated).[15] Furthermore, even if CEOs may be unaware of the non-compete enforcement change events in a timely manner, the *actual* changes in labor market mobility due to the enforcement changes can still influence managers' behaviors (Garmaise 2011). Finally, following prior legal studies (Marx 2011), we argue that it is the threat of potential lawsuits rather than the actual lawsuits that affects the preemptive actions managers take to mitigate adverse legal consequences.[16] In summary, given the various channels through which managers can learn about non-compete enforcement changes and their impacts, we expect that most CEOs are aware of them.[17]

Expectation management

[15] We collect data on both manager education background and whether a general counsel has been appointed to a firm's C-suite from BoardEx over 2000–2013, as its coverage is incomplete before 2000.

[16] The frequency of actual lawsuits reflects the outcome from both the threats of lawsuits and the actions taken by managers to counter such threats. Thus, a low number of observed lawsuits could be due to managers' successful actions in mitigating the likelihood of being sued. Consistent with this, prior accounting and legal studies suggest that managerial concerns of potential lawsuits are real (Skinner 1994, 1997; Marx 2011; Starr et al. 2019). Therefore, we *ex ante* do not expect the threat of non-compete enforcement to result in frequent actual lawsuits necessarily.

[17] Consistent with their awareness, there are many examples in business media that CEOs explicitly discuss their firms' use of non-competes. For example, Guy Bradshaw, Chairman and CEO of Bradshaw Medical discussed his non-compete provisions and their related impact in his interview with OrthoSpineNews (https://goo.gl/bk6pq3); Andy Roberts, CEO of TIG, discussed his personal experience about the impact of the non-compete provisions on his career (https://goo.gl/kMMa83); and Doug Macnaught discussed his initiation of a new company and how non-compete provisions affect his new business (https://goo.gl/oMfTcx).

Following prior studies (Bartov et al. 2002; Das et al. 2011), we use analysts' downward forecast revisions within a quarter as our main measure of expectation management instead of managerial earnings guidance for three reasons. First, many firms provide no earnings guidance, but still manage to influence market expectations using other types of guidance that are not observable or available to researchers (Wasley and Wu 2006; National Investor Relations Institute 2012; Chuk et al. 2013). Second, studies show that managers often influence investor expectations with qualitative or soft information, which is not captured by managerial guidance (Bozanic et al. 2018). Third, as sophisticated aggregators and processors of information, analyst forecast revision over time is used commonly in prior literature as a proxy for the news disclosed by management (Donelson et al. 2012).

We follow prior studies (Bartov et al. 2002; Das et al. 2011) to measure expectation management using analysts' (downward) forecast revisions calculated as analysts' first EPS consensus forecast minus their last consensus within each fiscal quarter, scaled by the difference between the first consensus and the actual EPS to ensure that *ExpMgt* is comparable across all firm-quarters.[18] We follow Das et al. (2011) and require that the first (last) forecast is issued at least one day after (before) the prior (current) quarter's earnings announcement. We further require at least 20 days between the first and last consensus forecasts. Besides *ExpMgt*, we use an indicator variable as an alternative measure of downward expectation management (*D_ExpMgt*), coded as 1 if *ExpMgt* > 0, and 0 otherwise.

Research design

First, we test the prediction that higher non-compete enforceability encourages managers to engage in more downward expectation management (Hypothesis 1) by estimating the regressions using a cohort-based matching approach specified below, following Bertrand and Mullainathan (2003) and Gormley and Matsa (2011):

$$ExpMgt_{iqc}\ (D\_ExpMgt_{iqc}) = \alpha_0 + \alpha_1 IncreaseEnforce_{iqc} + \alpha_k X_{iqc} + \gamma_{ic} + \sigma_{qc} + \varepsilon_{iqc} \quad (1)$$

For each quarter with at least one event of a non-compete enforcement change (i.e., a treatment event), we create a cohort consisting of both treatment firms (firms in states with a treatment event that quarter) and

[18] We choose this deflator to express guide down as the proportion of the gap between initial analyst consensus and actual earnings, but scaling by the beginning-of-quarter stock prices does not affect our inferences.

control firms (firms in states with no treatment event for the entire sample period). We retain observations from four years (16 quarters) before to four years (16 quarters) after the treatment event for each cohort. We pool observations in all cohorts to create our final sample for regression analyses.

In Eq. (1), *ExpMgt* or *D_ExpMgt* is a measure of expectation management and *IncreaseEnforce* is defined as before. We use the Ordinary Least Squares (OLS) estimation when *ExpMgt* is the dependent variable, and we adopt Chamberlain's Random Effects (CRE) Probit estimation when *D_ExpMgt* is the dependent variable (Wooldridge 2002). We include CEO-firm-cohort fixed effects (γ_{ic}) to control for unobservable time-invariant CEO and firm factors. We also include year-quarter-cohort fixed effects (σ_{qc}) to account for general trends that affect all firms across all states, which effectively renders model (1) a diff-in-diff research design. In addition, we allow the CEO-firm and year-quarter fixed effects to vary by cohort. Thus, the diff-in-diff coefficient α_1 captures the incremental change in the dependent variable (i.e., the use of expectation management) from before to after non-compete enforceability changes impact the treatment firms relative to control firms. Hypothesis 1 predicts that increased enforceability encourages managers to further guide down market expectations, and hence a positive coefficient on *IncreaseEnforce* ($\alpha_1 > 0$).

We follow Das et al. (2011) and include a set of control variables (*X*). *NOA* is net operating assets; *Sens* is the sensitivity of stock prices to earnings news; *NumEst* is the number of analyst estimates in the initial forecast consensus; *LogMktCap* is the natural logarithm of market capitalization; *Persist* is the percentage of the past four quarters in which analyst consensus forecasts were beaten; *AbsFE* is the absolute value of the forecast error of the initial analyst consensus; *ROA* is the return on total assets; *Loss* is an indicator for operating losses in the prior four quarters; and *AM* is accruals-based earnings management. We also control for the statewide economic condition (*ΔGDP*) to mitigate the potential concern that economic performance of a state may trigger non-compete enforcement changes. Lastly, we include CEO tenure (*Tenure*) to control for CEOs' general career concerns (Pae et al. 2016).[19]

[19] We do not include a Q4 indicator, Post Reg FD indicator, or an indicator to represent high litigious industries as in Das et al. (2011) because the CEO-firm and year-quarter fixed effects in our model supersede these indicators.

As non-compete enforcement varies by state, we follow prior studies and cluster standard errors by state in all specifications (e.g., Bertrand and Mullainathan 2003). Doing so mitigates the potential concern that time-varying omitted correlated variables may systematically influence firms within each state. We winsorize all continuous variables at the 1st and 99th percentiles. Appendix 2 reports the definitions and detailed measurements for all variables.

Sample and data

Our sample period spans from 1994 to 2013. We start with all U.S. non-financial and non-utility companies covered by the Institutional Brokers Estimate System (I/B/E/S). Following Bartov et al. (2002), we require analyst forecasts to be issued between the earnings release dates of the previous and current quarters. Furthermore, we require quarterly earnings to be released no later than 90 days after the quarter-end, and that at least two individual analysts must have issued forecasts for the quarter with a minimum of 20 days apart from one another (Das et al. 2011). This sample is then merged with the Compact Disclosure dataset of company headquarter locations, because top executives' non-compete provisions are enforced in the states where they work (Garmaise 2011). To evaluate the effect of non-compete enforcement more accurately, we exclude firms if we cannot verify whether their managers have signed non-compete agreements.[20] After dropping firms lacking data from Quarterly Compustat and the Center for Research in Security Prices (CRSP), and applying our cohort-based matching procedure (described in the previous sub-section), our final sample consists of 95,689 firm-quarter observations.

4. Main results: The effect of non-compete enforceability on expectation management

Baseline results

Table 1 Panel A reports the summary statistics of the variables in Eq. (1) for testing our main hypothesis (Hypothesis 1). Consistent with prior studies (Bartov et al. 2002; Das et al. 2011), the mean of

[20] We search our sample firms' Securities and Exchange Commission (SEC) filings for non-compete disclosures, including 10-K, 10-Q, DEF14A, 8K, and Exhibit-10. Our search keywords include "not to compete", "not-to-compete", "noncompetition", "noncompetitive", "non-competes", "non-compete", "non-competition", "non-competitive", "unfair competition", and "competing business", and these must appear within 10 words from "employment", "termination", "severance", "geographic", "employed", or "terminate". Once a firm has disclosed non-competes with any of its executives, we include all remaining quarters of the firm in our sample for three reasons. First, we find that whenever non-compete provisions apply to other executives at the same firm, they also apply to the CEO on whom our study focuses. Second, CEOs are more likely to pay attention to their own non-compete even when their firms disclose the non-competes of other executives. Third, once a CEO has a non-compete agreement, he or she likely will continue to have such an agreement even if the firm does not disclose it in the following years.

D_ExpMgt is equal to 0.277, implying that 27.7% of firm-quarters in our sample engage in expectation management. Pair-wise correlations in both Panel B (the "Enforcement Increase Events" sample) and Panel C (the "Enforcement Decrease Events" sample) are moderate, and untabulated variance inflation (VIF) tests also suggest that multi-collinearity is not a severe concern in our tests (all VIFs are below 10). We note that both *ExpMgt* and *D_ExpMgt* are positively associated with *IncreaseEnforce* only in the "Enforcement Increase Events" sample but not in the "Enforcement Decrease Events" sample. Moreover, *IncreaseEnforce* is also correlated with several other variables such as firm size (*LogMktCap*) and number of analysts following proxied by the number of analyst earnings estimates (*NumEst*), which highlights the importance of conducting multivariate analyses to isolate and evaluate the impact of non-compete enforcement increases.

Table 2 Panel A reports results from estimating Eq. (1). In Columns (1) and (2), where we include all treatment events of non-compete enforcement changes (both increase and decrease in enforcement), we find weak evidence for Hypothesis 1. Specifically, we only find a marginally significant coefficient on *IncreaseEnforce* in Column (2) for *D_ExpMgt* (*z*-stat. = 1.87) but not in Column (1) for *ExpMgt* (*t*-stat. = 1.42). The weak results raise the possibility that increases and decreases of non-compete enforcement may affect firms' use of expectation management in different ways. To examine this, we split the sample into subsamples for non-compete enforcement increases and decreases and repeat our analyses. We find the effect of changes in non-compete enforcement on expectation management is only statistically significant in the enforcement-increase sample and this effect holds for both specifications (*t*-stat. = 2.27 and *z*-stat. = 3.36). In addition, the economic magnitude is considerable as an increase in non-compete enforceability is associated with a 58% increase in expectation management on average, *ceteris paribus*.[21] In contrast, the results in Columns (5) and (6) show that an enforcement decrease does not significantly affect the use of expectation management in terms of the likelihood and the magnitude (*t*-stat. = -1.28 and *z*-stat. = -0.26). Our results suggest that when non-compete enforcement decreases, managers do not disengage from expectation management in the same way that they engage in it when non-compete enforcement increases.

[21] We divide the coefficient on *IncreaseEnforce* (0.083 in Table 2 Column 3) by the mean of *ExpMgt* (0.144 in Table 1 Panel A).

Managers' prolonged use of expectation management, even when their career concerns subside following lessened non-compete enforcement, is also consistent with expectation management having relatively low litigation costs and potential damage to firm value, compared with other tools managers can use to achieve earnings targets (Skinner 1994, 1997; Graham et al. 2005; Koh et al. 2008).

Taken together, our finding of an asymmetric response to non-compete enforcement increases vs. decreases suggests a subtler effect of labor market mobility on managers' use of expectation management than our Hypothesis 1 predicts. While managers are more likely to manage expectations downward if the states they work in strengthen non-compete enforcement, they do not refrain from such behavior when enforceability is loosened. Following this finding, our remaining analyses focus specifically on events leading to stricter non-compete enforcement.

Falsification tests and trend analyses

While our diff-in-diff design allows us to draw causal inferences on how labor market mobility affects managers' use of expectation management by exploiting plausibly exogenous changes in the enforceability of non-compete provisions, we conduct two additional analyses to further ascertain that it is these events that lead to firms' changes in expectation management.

We first conduct a falsification test following Bertrand et al. (2004) to examine whether our results are driven by some temporal changes other than exogenous changes in non-compete enforcement. To do so, we randomly select a set of firm-quarter observations as "pseudo-events of enforcement change" while assuming the "actual" changes did not occur. We then estimate the baseline regression model using the pseudo-events instead of the actual events. We repeat this test 100 times and find that the mean value of the coefficient estimates from the pseudo model is virtually zero, suggesting that the possibility that our documented effect is caused purely by chance is miniscule.[22]

Next, we conduct a trend analysis to determine the timing of the effect. Specifically, following the strategy in Gormley and Matsa (2011), we replace the indicator variable *IncreaseEnforce* in Eq. (1) with a series of variables, i.e., $IncreaseEnforce_{-2}$, $IncreaseEnforce_{-1}$, $IncreaseEnforce_{+1}$, $IncreaseEnforce_{+2}$,

[22] The average coefficient (untabulated) on *IncreaseEnforce* is -0.0005 (-0.0043) when *ExpMgt* (*D_ExpMgt*) is the dependent variable, and both are insignificantly different from zero.

IncreaseEnforce$_{+3}$ and *IncreaseEnforce*$_{+4}$. *IncreaseEnforce*$_{-2}$ (or *IncreaseEnforce*$_{-1}$) is an indicator set to one for firms in treatment states two years (or one year) prior to a quarter in which an event of enforceability increase, and zero otherwise. *IncreaseEnforce*$_{+1}$, *IncreaseEnforce*$_{+2}$, *IncreaseEnforce*$_{+3}$ and *IncreaseEnforce*$_{+4}$ are equal to one for firms in treatment states one, two, three and four years after the event quarter, and zero otherwise, respectively. Figures 1A and 1B plot the point estimates from the modified Eq. (1), where we exclude the observations of the event quarters and allow the effect of *IncreaseEnforce* to vary by year before and after a treatment event. We find no significant difference in either measure of expectation management prior to the increase in non-compete enforcement; the treatment firms do not appear to be more or less likely than the control firms to manage market earnings expectations before the increase in non-compete enforcement. However, after an increase in non-compete enforcement, treatment firms tend to use more expectation management than control firms. The absence of a significant effect immediately after non-compete enforcement changes suggests that there is little anticipation for these regulatory changes, mitigating the concern that these events are endogenously determined by firms' lobbying efforts or anticipated by managers.[23]

In summary, results from our falsification tests suggest that our main results are unlikely to be driven by random temporal shifts. The result from our trend analysis provides evidence that the parallel trend assumption underlying our diff-in-diff design holds and shows that non-compete enforceability exerts its effect gradually over time.

Robustness to alternative samples

The baseline results are subject to alterative explanations. For example, California and the Business Service sector are dominant in our sample.[24] It is thus possible that Californian firms are driving our results. Moreover, Internet firms (a subset of the Business Service sector) tend to use alternative compensation schemes, such as option grants, that may also affect managerial incentives besides non-compete

[23] Two factors explain why it takes managers time to adjust disclosures to the changes in non-compete enforcement: (1) disclosures are usually sticky (Rogers and Van Buskirk 2013); and (2) some managers likely respond to the *actual* changes in labor market mobility caused by the enforcement changes, which usually also take time.

[24] Untabulated distribution statistics by state and by industry (following the classification used by Fama and French [1997]) reveal that California (16,803 firm-quarters) and the Business Service sector (11,379 firm-quarters) have the largest representation.

enforceability. In addition, Regulation Fair Disclosure (Reg FD) may also affect the use of expectation management (Kross and Suk 2012). Moreover, compared with court rulings, regulatory changes in state laws may be affected by lobbying and pressure from firms and thus are less exogenous.

To investigate these alternative explanations, we perform our tests on a series of subsamples reported in Table 2 Panel B. For brevity, we report only the coefficient on *IncreaseEnforce*, that is, our key variable. First, we exclude observations from California (Rows 1 and 2) or in the Business Service sector (Rows 3 and 4). Our results remain significant at conventional levels in these subsamples, suggesting that it is unlikely that firms in California and/or the Business Service sector are driving our results. Second, we exclude events prior to Reg FD (1992–2000) (Rows 5 and 6). Again, our results remain significant at conventional levels in this subsample, suggesting that our findings are not affected by the passage of Reg FD. Third, we focus only on events based on court rulings and exclude events of regulatory changes in state laws (Rows 7 and 8). As before, our results continue to hold at conventional levels in this subsample.

Overall, we find robust results across alternative subsamples, mitigating concerns that our results are driven solely by certain dominant states or industries, a certain sample period, or events that are potentially endogenous to firms' lobbying efforts.

Robustness to alternative specifications and alternative explanations

In this sub-section, we conduct a battery of tests to investigate alternative explanations and check the robustness of our results to alternative specifications. We report these results in Table 2 Panel C.

The use of severance pay

Severance pay is another provision sometimes used in executive employment contracts (Schwab and Thomas 2006; Cadman et al. 2016). While a sizable literature has examined the impact of severance pay (see Section 2), we focus on non-compete provisions instead because their enforcement varies across states and is subject to changes exogenous to individual firm managers. To the extent that severance pay lowers managers' job-switching costs, it could partially offset the effect of non-compete enforcement increases, thereby biasing against us finding a significant association between non-compete enforceability and expectation management. Thus, we expect our results to hold after considering severance packages. To verify this, we account for the *presence* of severance pay, using data collected from textual analyses of firms'

disclosures in their SEC filings over the full sample period (1994–2013).[25] We further examine the post-2006 period when firms are required to disclose severance pay amounts (*TERM_PYMT* in ExecuComp) and non-compete provisions (*only* if related to severance pay) in Exhibit-10. Adding these data requirements reduces our sample size by nearly 20%, thus reducing testing power, but we continue to find a significant effect of non-compete enforcement. Overall, our inferences remain unchanged after controlling for either the presence or the amount of managers' severance pay (Rows 1–4).

Employer's ability to find replacements for incumbent managers

As discussed earlier, how labor market mobility affects managerial career concerns differs between the employee's and the employer's perspectives. While the increase in non-compete enforcement worsens employees' career concerns, it also reduces the supply in the executive labor market, making it more difficult for employers to find replacements for incumbent managers, which in turn reduces employers' incentives to dismiss incumbent CEOs, thereby mitigating incumbent CEOs' career concerns.

Prior studies find that after losing an executive to other firms, the remaining executives' pay tends to rise, especially if they have high employment mobility (Gao et al. 2015). If an increase in non-compete enforcement increases a firm's cost of recruiting new CEOs to replace their incumbents, we expect the firm to pay more to incumbents. Partially in line with this notion, prior studies find some evidence that incumbent CEOs' compensations rise with the *level* of non-compete enforcement (Garmaise 2011; Kini et al. 2018).[26]

To further investigate whether the employer's channel plays a significant role in managerial use of expectation management, we repeat our main analysis with additional controls related to CEO annual compensation structures (e.g., cash pay, other compensation, new option grants, cumulated options vested but unexercised, cumulated unvested options, stock ownership, and the average total compensation of CEOs in the same industry) (Rau and Xu 2013; Chen et al. 2018). Our inferences remain the same (Rows 5 and

[25] Similar to our procedure of identifying firms with disclosures of non-compete agreements, we search SEC filings including 10-K, 10-Q, DEF14A, 8K, and Exhibit-10 for severance pay. Our search keyword is "severance", and must appear within 10 words from "employment", "termination", "geographic", "employed", or "terminate".

[26] There is also evidence inconsistent with the "employers' channel". For example, Kini et al. (2018) find higher performance-turnover sensitivity for CEOs with non-compete agreements and working in high enforcement states. This suggests that employers are more willing to fire CEOs for poor performance if their CEOs are more constrained from joining a competitor, contrary to the concerns expected under the "employers' channel" of a potential decrease in labor supply of new CEOs. In line with this, we also find no significant change in new CEOs' compensations after non-compete enforcement increases (untabulated). We thank an anonymous referee for suggesting this investigation.

6).[27] Overall, the results from our additional tests suggest that the alternative channel from the employers' perspective appears insignificant, consistent with our main results that document a significant channel from the employees' (i.e., managers') perspective.

Analyst initial forecast errors

Our finding is consistent with our primary hypothesis that managers engage in more expectation management in response to increased non-compete enforcement, but one concern about our measures of expectation management is that they might capture analysts' biased initial forecasts rather than managers' deliberate actions. To mitigate this concern, we examine and find an insignificant association between analysts' initial forecast errors (*IFE*) and changes in non-compete enforceability (*IncreaseEnforce*) (*t*-stat. = 0.546, untabulated), suggesting that it is unlikely that *IFE* is an omitted correlated variable. We also repeat our main analyses adding analysts' initial forecast errors as a control variable (reported in Rows 7 and 8), and we continue to find a significant coefficient on *IncreaseEnforce* in both regressions (*p*-value < 0.05). The robustness of our results mitigates the concern that they are driven by analysts' initial forecast errors.

Alternative measures of expectation management

Another concern with our analyst-forecast-based measures of expectation management is that they can be confounded by information sources other than managerial disclosures (e.g., the press) that also cause analysts to lower their forecasts. To mitigate this concern, we repeat our analyses using several alternative measures of expectation management. First, we tease out industry-wide news from analysts' revisions, on which analysts tend to have more expertise, and hence managers likely can exert less influence (Hutton et al. 2012; Kadan et al. 2012). To do so, we replace the dependent variable with the residuals from a regression of *ExpMgt* on industry average returns (*ExpMgt_IndResid*). Our results continue to hold (see Row 9). Second, prior research suggests that analysts may routinely "walk down" their expectations or issue downward biased forecasts every quarter to assist managers in achieving earnings targets (Hilary and Hsu

[27] The robustness of our results also mitigates the concern that our main results are driven by the changes in CEOs' compensation contracts, which may alter managerial incentives to achieve earnings targets (Burns and Kedia 2006; Effendi et al. 2007) and disclosure policy (Nagar et al. 2003).

2013).[28] To account for the effect of such a routine pattern of "walk-down," we follow Das et al. (2011) and measure expectation management with the *innovation* component of analysts' downward revisions (*ExpMgt_LagResid*), which is the residual from regressing *ExpMgt* on its lagged value by one quarter. As Row 10 shows, our inferences remain unchanged.

Next, we use managers' explicit downward earnings forecasts and the intensity of the CEO's use of negative words on earnings conference calls as alternative measures. Focusing on a subsample where managers have issued at least one earnings forecast for the upcoming quarter, we set the indicator *MFBad* to 1 if the midpoint of the management forecast is below the prevailing analyst consensus forecast, and 0 otherwise. Despite a smaller sample size as many firms issue no management forecast, *IncreaseEnforce* remains significantly positive at the 0.05 level in Row 11, suggesting that managers' earnings forecast is more likely to convey negative news after non-compete enforcement increases in their state.[29] Similarly, using a subsample of firms hosting earnings conference calls, we measure expectation management using the management's tone during the calls. We define *NegTone_ConCall* as the number of negative words minus the number of positive words divided by the number of words spoken by the CEO during the Q&A session of a conference call, multiplied by 100. *IncreaseEnforce* remains significantly positive in Row 12, suggesting that managers use more negative words in earnings conference calls after their state increases non-compete enforcement. In summary, our results are robust to using alternative measures of expectation management, such as issuing management earnings forecasts to convey negative news and negative tones in earnings conference calls, both of which more explicitly capture manager-initiated disclosures than our main measure based on analyst forecasts. However, these alternative measures require additional data of management forecasts or conference calls, whose coverage is likely incomplete, especially before Reg FD and for qualitative guidance (National Investor Relations Institute 2012; Chuk et al. 2013).

CFO effect

[28] Such habitual downward revision by analysts is unlikely to drive our results because (1) we include firm fixed effects in our analyses, which account for habitual revisions at the firm level; and (2) the empirical auto-correlation in *ExpMgt* is quite small (rho = -0.002, untabulated), suggesting minimal habitual downward revision in our sample.

[29] Our sample size declines from 51,754 to 18,205 because we focus on the firms issuing management earnings forecasts in a certain quarter. Our results are not affected if we include all firm-quarters after 1998 and code *MFBad* equal to zero if a firm does not issue any earnings forecast in a quarter.

Throughout our main analyses, we have included CEO fixed effects and control for CEO tenure. Our focus on CEOs is motivated by the notion that CEOs likely play an important role in communicating with analysts and investors to ensure that market earnings expectations are realistic and attainable. This notion is supported by both anecdotes and academic studies (Lee et al. 2012; Bochkay et al. 2019).[30] As CFOs likely also play a role in expectation management, we re-estimate Eq. (1) by replacing CEO tenure with CFO tenure and by replacing CEO-firm-cohort fixed effects with CFO-firm-cohort fixed effects. As the results in Rows 13–14 show, our inferences remain unchanged.[31]

Overall, Table 2 Panel C shows that our results are robust to a battery of alternative specifications such as the presence and the amount of severance pay, executive compensation structures, initial analyst forecast errors, alternative measures of expectation management, and accounting for the influences of CFOs.[32]

5. Alternative tools to achieve earnings targets

Our analysis thus far focuses on expectation management as a tool to achieve earnings targets, but managers can also use other tools to report earnings that meet or beat analyst expectations (Chen et al. 2015; Chen et al. 2018). It thus leaves out a natural but important question on to what extent expectation management vis-à-vis earnings management helps managers in achieving earnings targets in the context of labor market mobility. Despite prior empirical evidence on wider use of expectation management than earnings management due to lower expected litigation costs and less potential damage to firms' long-term value (Koh et al. 2008), Graham et al. (2006)'s survey still finds that most managers agree or strongly agree

[30] For example, Lee et al. (2012) find that CEOs are more likely to be replaced if their earnings forecast accuracy is low. Bochkay et al. (2019) document that CEOs change their disclosure styles over their tenure, whereas non-CEO executives' disclosure styles remain time-invariant. Anecdotal evidence also suggests that CEOs play an important role in corporate communications. For example, in a CNBC interview on April 16, 2008, Jack Welch (chairman and CEO of General Electric from 1981 to 2001) criticized Jeff Immelt (CEO of General Electric in 2008) as lacking credibility for promising a performance level that he missed three weeks later (https://www.cnbc.com/id/24158810). Even though our discussion focuses on CEOs, we do not differentiate between CEOs and CFOs, because our setting is not particularly conducive for such differentiation for two main reasons: (1) based on our hand-collected sample, non-compete provisions apply equally to CEOs and CFOs; and (2) when non-compete enforcement changes, it also applies to both CEOs and CFOs.

[31] Our results are unchanged if we substitute CEO-CFO-firm-cohort fixed effects for CEO-firm-cohort or CFO- firm-cohort fixed effects (untabulated), mitigating concerns of time-invariant effects of CEO-CFO-firm triplets.

[32] Our inferences remain unchanged if we control for a past string of meeting/beating analyst expectations, which can affect managers' incentive to manage expectation (Kross et al. 2011). Our inferences also remain unchanged if we control for the percentage of CEOs promoted within their firms at the industry-level, which captures the tightness of the labor market (Parrino 1997; Cremers and Grinstein 2014).

that their firms use (real) earnings management to meet analysts' expectations. Thus, it is unclear which tool managers use more to help meet analysts' expectations.

To assess the relative use of expectation management and earnings management, we conduct a path analysis that gauges the relative contribution of accrual earnings management (*AM*), real earnings management (*RM*), and expectation management (*ExpMgt*) to achieving meeting and beating expectations (*MBExp*) in the setting of labor market mobility changes. The path analysis estimates a structural model to determine how a source variable (*IncreaseEnforce* in our case) affects an outcome variable (*MBExp* in our case) by decomposing their correlation and revealing the paths through mediating variables (in our case, *AM*, *RM*, and *ExpMgt*). Path analyses have been used in prior accounting research to examine the mediation effect of a source variable on an outcome variable (DeFond et al. 2016; Hilary et al. 2016).

Following these prior studies, we estimate the system of equations below:

$$ExpMgt_{iq} = a_0 + a_1 IncreaseEnforce_{iq} + a_k X_{iq} + \gamma_i + \sigma_q + \varepsilon_{iq}, \quad (2a)$$

$$AM_{iq} = b_0 + b_1 IncreaseEnforce_{iq} + b_k X_{iq} + \gamma_i + \sigma_q + \varepsilon_{iq}, \quad (2b)$$

$$RM_{iq} = c_0 + c_1 IncreaseEnforce_{iq} + c_k X_{iq} + \gamma_i + \sigma_q + \varepsilon_{iq}, \quad (2c)$$

$$MBExp_{iq} = d_0 + d_1 IncreaseEnforce_{iq} + d_2 ExpMgt + d_3 AM + d_4 RM + d_k X_{iq} + \gamma_i + \sigma_q + \varepsilon_{iq}, \quad (2d)$$

where Eq. (2a) – (2c) examine the effect of the change in non-compete enforcement on each of the three tools (*ExptMgt*, *AM*, and *RM*) to avoid missing earnings targets, and Eq. (2d) examines the mediated effects of these tools on *MBExp*. The outcome variable of the path analysis is *MBExp*, which is set to one if the reported earnings met or beat analysts' forecast consensus. In this framework, P(*IncreaseEnforce, ExpMgt*) × P(*ExpMgt, MBExp*), the product of the standardized coefficients a_1 and d_2 ($a_1 \times d_2$), measures the mediated effect from a non-compete enforcement change to *MBExp* by using expectation management. Similarly, $b_1 \times d_3$ ($c_1 \times d_4$) measures the mediated effect from a non-compete enforcement change to *MBExp* by using accrual earnings management (real earnings management). Using these numerical products, we can compare the extent to which each tool contributes to the *MBExp* game. The statistical significance of the mediating effect is assessed using Sobel (1982)'s statistics. Table 3 reports the results. We find that the mediating paths for both expectation management P(*IncreaseEnforce, ExpMgt*) × P(*ExpMgt, MBExp*) and real earnings management P(*IncreaseEnforce, RM*) × P(*RM, MBExp*) are significantly positive (*t*-stats = 2.85 and 2.04), whereas the mediating path for accrual earnings management P(*IncreaseEnforce, AM*) ×

P(*AM, MBExp*) is insignificant at conventional levels (*t*-stat = -0.32). These findings are consistent with the notion that accrual earnings management has become used less due to its higher expected litigation costs (Skinner 1994, 1997; Graham et al. 2005).

As for the relative magnitude of these tools in contributing to *MBExp*, we find that the absolute value of P(*IncreaseEnforce, ExpMgt*) × P(*ExpMgt, MBExp*) is nearly four times that of P(*IncreaseEnforce, RM*) × P(*RM, MBExp*). This finding is consistent with the prior finding that, compared with real earnings management, which is used more widely than accrual earnings management, expectation management is more extensively used by managers to achieve earnings targets likely because it is less likely to cause potential damage to firms' long-term value (Koh et al. 2008).

In summary, when we account for the alternative tools (accrual and real earnings management) to achieve earnings expectations, we continue to find a significant effect on expectation management. More importantly, our path analysis reveals that the use of expectation management surpasses that of other tools in achieving earnings expectations in the setting of increased non-compete enforcement. Our results on expectation management complement prior findings on earnings management (Chen et al. 2018).

6. Factors moderating the effect of non-compete enforceability on expectation management

To test Hypothesis 2 (a-c), we estimate the following model:

$$ExpMgt_{iqc}\ (D\_ExpMgt_{iqc}) = \alpha_0 + \alpha_1 IncreaseEnforce_{iqc} + \alpha_2 IncreaseEnforce_{iqc} \times Factor_{iqc} + \alpha_3 Factor_{iqc} + \alpha_k X_{iqc} + \gamma_{ic} + \sigma_{qc} + \varepsilon_{iqc}, \quad (3)$$

where *Factor* represents one of the manager-, firm-, or industry-level characteristics we predict in Hypothesis 2 (a-c) to moderate the effect of non-compete enforcement changes on managers' use of expectation management. Following our earlier analysis, we estimate an OLS (a CRE Probit) model when the dependent variable is *ExpMgt* (*D_ExpMgt*) (reported in Table 4 Panels A and B). Our discussion below focuses on the OLS results for brevity but the Probit results are largely similar.

Hypothesis 2(a) predicts that CEOs with more general managerial skills are likely less affected by an increase in non-compete enforceability because they are less affected by forced turnovers. Our first measure is an indicator variable (*HighGenAbility*) set to one if the managerial general skill index of a CEO

(Custódio et al. 2013) is above the median, and zero otherwise.[33] Our second measure is an indicator variable (*Seniority*) set to one if the difference between the number of years of a manager being the CEO at the current firm and the median number of years of other top executives in the C-suite working at the same firm is above the median, and zero otherwise. Hypothesis 2(a) predicts α_2 to be negative. Consistent with Hypothesis 2(a), we find a significantly negative coefficient on *IncreaseEnforce*×*HighGenAbility* at the 0.05 level (*t*-stat. = -2.31) in Column (1). Our main effect on *IncreaseEnforce* remains significant (*t*-stat. = 2.08), suggesting that CEOs with less general managerial skills are still significantly affected by non-compete enforcement changes. In Column (2), we replace *HighGenAbility* with *Seniority* and again find a significant and negative interaction term (*t*-stat. = -2.22) and a positive main effect of *IncreaseEnforce* (*t*-stat. = 2.33). Collectively, our results are consistent with Hypothesis 2(a).

Hypothesis 2(b) predicts that corporate governance at the firm level, measured as intensified monitoring from a more independent board of directors, will magnify the effects of non-compete enforceability because CEOs that are monitored more closely are under greater pressure to meet earnings expectations. We measure board independence using an indicator variable (*BoardIndep*) that is set to one if the percentage of independent board directors is above the median and zero otherwise. We obtain data on board composition from RiskMetrics. Hypothesis 2(b) predicts α_2 to be positive. Consistent with Hypothesis 2(b), we find a significantly positive coefficient on the interaction term *IncreaseEnforce*×*BoardIndep* at the 0.05 level (*t*-stat. = 2.11) in Column (3). Our main effect on *IncreaseEnforce* remains significant (*t*-stat. = 2.13), suggesting that CEOs at firms with less independent boards are still significantly affected by non-compete enforcement changes. Thus, our result is consistent with Hypothesis 2(b).

Hypothesis 2(c) predicts more pronounced effects of non-compete enforcement in more homogeneous industries because non-compete provisions are more likely to be binding in these industries. Following Gillan et al. (2009), we compute an industry homogeneity index as the median value of the R-squared from a regression of monthly stock returns on the returns from an equal-weighted industry index

[33] We thank Custódio et al. (2013) for generously sharing the General Ability Index data (http://jfe.rochester.edu/data.htm).

composed of all firms in the industry over the previous 10 years for Hypothesis 2(c). We then construct an indicator variable (*HighIndHomo*) set to one for firms in industries with a homogeneity index above the median and zero otherwise. Hypothesis 2(c) predicts α_2 to be positive. Consistent with Hypothesis 2(c), we find a significant and positive coefficient on the interaction term *IncreaseEnforce*×*HighIndHomo* at the 0.05 level (*t*-stat. = 2.54) in Column (4). The main effect on *IncreaseEnforce* is insignificant at conventional levels (*t*-stat. = 0.38), but the sum of *IncreaseEnforce* and *IncreaseEnforce*×*HighIndHomo* is significant at the 1% level. This suggests that the effect of non-compete enforceability changes is discernible only among firms in highly homogeneous industries, as they stand to suffer the most potential damages in the case that they lose their employees to their competitors, consistent with the objective of non-compete provisions.

Table 4 Panel B shows similar results from our Probit models where *D_ExpMgt* is the dependent variable. Together, the results in Table 4 are consistent with Hypothesis 2(a) – Hypothesis 2(c), which predict that the effect of non-compete enforceability on managers' use of expectation management varies in the cross section with various moderating factors at the manager-, firm-, and industry-levels.

7. Non-compete enforcement changes, the use of expectation management, and CEO turnover

Previous studies find that CEO turnover declines following intensified non-compete enforcement (e.g., Garmaise 2011), but the underlying mechanism remains unclear. In this section, we examine whether actions taken by managers to reduce the risk of forced turnover (i.e., reaction effect) can contribute to this finding. We find that managers use more expectation management to minimize the risk of missing earnings targets in response to an increase in non-compete enforceability. If such a managerial response is effective, CEOs achieving earnings targets via expectation management should be less likely to be replaced after an increase in non-compete enforceability. In this section, we examine this conjecture to shed more light on Garmaise (2011)'s finding. To do so, we conduct a path analysis of whether and how the use of expectation management (*ExpMgt*) mediates the link between an increase in non-compete enforcement (*IncreaseEnforce*) and the achievement of meeting and beating expectation (*MBExp*), which in turn is expected to reduce the likelihood of CEO turnover (*Turnover*).

Similar to Eq. (2) in the previous section, we estimate the following system of equations:

$$ExpMgt_{iq} = a_0 + a_1 IncreaseEnforce_{iq} + a_k X_{iq} + \gamma_i + \sigma_q + \varepsilon_{iq}, \quad (4a)$$

$$MBExp_{iq} = b_0 + b_1 IncreaseEnforce_{iq} + b_2 ExpMgt + b_k X_{iq} + \gamma_i + \sigma_q + \varepsilon_{iq}, \quad (4b)$$

$$Turnover_{iq} = c_0 + c_1 IncreaseEnforce_{iq} + c_2 ExpMgt + c_3 MBExp + c_k X_{iq} + \gamma_i + \sigma_q + \varepsilon_{iq}, \quad (4c)$$

In this framework, a_1 measures the strength of the direct path from *IncreaseEnforce* to *ExpMgt*. Likewise, b_2 (c_3) measures the strength of the direct path from *ExpMgt* to *MBExp* (from *MBExp* to *Turnover*) after controlling for other contemporaneous effects. Our key interest is the product of the standardized coefficients a_1, b_2, and c_3 (i.e., $a_1 \times b_2 \times c_3$), the mediated path of P(*IncreaseEnforce*, *ExpMgt*) × P(*ExpMgt*, *MBExp*) × P(*MBExp*, *Turnover*), which measures the mediated effect of a non-compete enforcement change on CEO turnover through the channel of meeting or beating earnings expectations aided by using expectation management.

Table 5 reports the results. The mediating path of P(*IncreaseEnforce*, *ExpMgt*) × P(*ExpMgt*, *MBExp*) × P(*MBExp*, *Turnover*) is significantly negative (*t*-stats = -2.00). It suggests that a significant factor contributing to the reduction in turnover following an increase in non-compete enforcement is the use of

expectation management and the achievement of earnings expectation (i.e., the "reaction effect").[34] Specifically, responding to an increase in non-compete enforcement, managers use downward expectation management more intensively as a tool to strategically achieve earnings targets, helping them retain their jobs and lowering their turnover rate. Our finding sheds light on the mechanism underlying the prior finding that CEO turnover declines following intensified non-compete enforcement (Garmaise 2011).

Overall, our path analysis of CEO turnover suggests that CEOs' reaction to increases in non-compete enforcement by engaging in more expectation management to minimize the risk of missing the analysts' consensus expectations appears successful in terms of reducing their likelihood of turnover.[35]

8. Conclusions

This study investigates whether and how managerial labor market mobility affects managers' use of expectation management to walk down analyst expectations to achievable levels. Exploiting changes in state-level non-compete enforcement as exogenous shocks to managerial labor market mobility, we find that managers use more downward expectation management after their states tightened the enforcement of non-compete provisions, consistent with an immobile labor market exacerbating managers' concerns about potential turnovers triggered by missing earnings expectations. Moreover, we find that in response to less mobility in the labor market, managers resort to downward expectation management more than other tools such as real and accrual earnings management to avoid missing analyst expectations. Shedding light on the underlying mechanisms, our cross-sectional analyses find more pronounced effects of non-compete enforceability on managers' use of expectation management for (a) managers with lower general ability or shorter tenure at their current employer, who likely face greater risks of forced turnovers; (b) managers at firms with more independent board directors, whose intensified monitoring likely exacerbates the pressure on managers to meet earnings expectations; and (c) managers in more homogenous industries, where non-

[34] Our inferences are unaffected if we measure *Turnover* based on whether a CEO is replaced by the end of the next year rather than by the next quarter.

[35] Consistent with this, in another untabulated analysis we find that investors do not appear to further discount the MBE premium for managers who engage in expectation management in response to stricter non-compete enforcement, relative to managers who do the same but without a change of non-compete enforcement. This suggests that shareholders are unaware of the effect of non-compete enforcement on managers' increased use of expectation management and consider all expectation management to be largely the same. Despite the discount, managers still benefit from this strategy, as we find that the negative market reaction to missing earnings targets outweighs the discount on the premium of meeting or beating earnings targets.

compete provisions are more likely to be binding. In addition, our path analysis suggests that managers' reaction to non-compete enforcement increases by engaging in more expectation management to minimize the risk of missing analyst consensus expectations appears successful in terms of reducing their turnover.

Our study provides the first large-sample evidence supporting the survey finding in Graham et al. (2005) that intra-industry mobility is one of managers' top concerns shaping their disclosure choices in the setting of expectation management where the expectation target is not fixed. By doing so, we also respond to Beyer et al. (2010) who point out in their review of the recent disclosure literature that "our understanding of how management's career concerns affect their disclosure strategies is still limited, as also previously noted in the survey by Healy and Palepu (2001)." Improving upon the emerging literature on managerial career concerns, our study identifies exogenous shocks in legal enforcement environments as potentially powerful settings for future research to better understand how managers' use of expectation management is shaped by one particular contracting term and its enforcement. Our study also points to a potentially promising path for future research—to examine the net costs or benefits of non-compete agreements to determine whether it serves as efficient contracting overall.

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Appendix 1

Changes in Enforcement of Non-Compete Provisions over 1992-2010

State (Event Time)	Event Details (Reference)	Effect	State (Event Time)	Event Details (Reference)	Effect
Delaware (2010 Jan.)	*Great American Opportunities, Inc. v. Cherrydale Fundraising, LLC*, 3718-VCP (Delaware Court of Chancery, Jan. 29, 2010)	Increase	Missouri (2006 Aug.)	*Healthcare Services of the Ozarks, Inc. v. Copeland*, 198 S.W.3d 604 (Missouri Supreme Court, Aug. 8, 2006)	Decrease
Florida (1996 Jul.)	New state law (Garmaise 2011)	Increase	New York (2006 Nov.)	*Morris v Schroder Capital Mgt. Intl. & Schroder Inv. Mgt. N. Am. Inc*., 2006 Slip Op. 08638, 2006 WL 3359077 (New York Court of Appeals, Nov. 21, 2006)	Increase
Idaho (2008 Jul.)	New state law (Idaho Statutes 44-2701)	Increase	Oregon (2008 Jan.)	New state law (Oregon Statutes 653.295)	Decrease
Illinois (2009 Sep.)	*Sunbelt Rentals, Inc. v. Ehlers*, 394 Ill.App.3d 421, 333 Ill.Dec. 791, 915 N.E.2d 862 (Illinois Supreme Court, Sep. 23, 2009)	Increase	South Carolina (2010 May)	*Poynter Inves., Inc. v. Century Builders of Piedmont, Inc.*, 387 S.C. 583, 587 (South Carolina Supreme Court, May 24, 2010)	Decrease
Louisiana (2001 Jun.)	*SWAT 24 Shreveport Bossier, Inc. v. Bond*, 808 So. 2d 294 (Garmaise 2011)	Decrease	Texas (1994 Jun.)	*Light v. Centel Cellular Co. of Tex.*, 883 S.W.2d 642, 644-45 (Garmaise 2011)	Decrease
Louisiana (2003 Aug.)	New state law (Garmaise 2011)	Increase	Texas (2006 Oct.)	*Alex Sheshunoff Mgmt. Servs., L.P. v. Johnson*, 209 S.W.3d 644, 655 (Texas Supreme Court, Oct. 20, 2006)	Increase
Massachusetts (2004 Feb.)	*Cypress Group, Inc. v. Stride & Associates, Inc*., 17 Mass. L. Rep. 436 (Massachusetts Supreme Court, Feb. 11, 2004)	Decrease	Wisconsin (2007 Dec.)	*H&R Block Eastern Enterprises, Inc. v. Swenson*, 745 N.W.2d 421 (Wisconsin Court of Appeals Dec. 20, 2007)	Decrease

Note: This table summarizes the state-level changes in the enforcement of non-compete agreements from 1992–2010. Procedures to hand-collect these listed changes in the post-Garmaise sample (i.e., after 2003) are as follows:

Following Garmaise (2011), we supplement the list of changes in enforceability of non-compete covenants beyond the period covered by Garmaise (2011) from 2004 to 2013. We take the following steps.

Step 1: Use "non-compete," with state name (e.g., "Alabama") and year (starting from "2004," the first year after Garmaise period) as the search keywords in Google Search.

Step 2: Focus on the results of new state laws or court rulings directly related to changes in enforcement of non-compete covenants. To maintain reasonable visibility of an event, we only scan the results in the first five pages from a search.

Step 3: For each result from Step 2, determine whether its effect can be classified as at least one of the twelve questions raised by Garmaise (2011).

Step 4: Repeat Steps 1-3 for all remaining years. If multiple events occur that result in changes in the same direction, we consider the earliest event as the event that triggers the change.

The three authors independently conducted the above procedures and recorded the same conclusion 95% of the time. The authors further requested an independent legal expert to verify each of the events and their effects.

Appendix 2

Variable Definitions

Variables of interest

IncreaseEnforce	A three-level, {-1, 0, +1}, measure of non-compete enforceability changes listed in Appendix 1. *IncreaseEnforce* takes the value of 1 for a firm located in a state over the period during which the state tightened the enforceability of non-competes, and -1 for a firm located in a state over the period during which the state loosened the enforceability of non-competes. For all other firm-year observations, *IncreaseEnforce* is equal to 0.

Measures of expectation management

ExpMgt	The walk-down of analyst forecasts, measured as the first consensus analyst forecast of Earnings Per Share (EPS) after prior quarter earnings are announced minus the last consensus analyst forecast for the quarter before earnings of the quarter are announced, scaled by the absolute value of the initial forecast error, which is defined as the difference between the first consensus analyst forecast of EPS and the actual EPS.
D_ExpMgt	An indicator variable for firm-quarters with downward expectation management set to 1 if *ExpMgt* >0, and 0 otherwise.
ExpMgt_IndResid	The residuals from the regression of expectation management (*ExpMgt*) on industry returns. Industry returns are equal-weighted average returns of firms with the same 2-digit Standard Industrial Classification (SIC) code in the contemporaneous window over which *ExpMgt* is calculated.
ExpMgt_LagResid	The residuals from the regression of expectation management (*ExpMgt*) on the prior quarter's expectation management ($ExpMgt_{q-1}$).
MFBad	An indicator variable equal to 1 if management forecast news is negative, and 0 otherwise. Management forecast news is measured as the management forecast of the upcoming quarter's earnings minus prevailing analyst consensus forecasts. Analyst consensus forecast is the mean of the analyst forecasts prior to management forecasts as in Institutional Brokers' Estimate System (IBES) guidance.
NegTone_ConCall	100 times the difference in the numbers of negative words versus positive words used by a CEO in the Q&A session of a conference call, scaled by the total number of words spoken by the CEO in the Q&A session of the conference call. In case of multiple conference calls in a quarter, we calculate the ratio by aggregating the numbers of words across multiple conference calls.

Measures of manager-, firm-, and industry factors

HighGenAbility	An indicator variable set to 1 if the general ability index from Custódio et al. (2013) is above the median, and 0 otherwise.
Seniority	An indicator variable set to 1 for CEOs whose tenure with their current employer is above the median relative to other top executives, and 0 otherwise. We measure CEOs' relative tenure by subtracting the median tenure of other executives from the CEO's tenure.
BoardIndep	An indicator variable set to 1 if the percentage of independent directors in the firm's board is above the median, and 0 otherwise.
HighIndHomo	An indicator variable set to 1 for CEOs who work in industries with an industry homogeneity index above the median, and 0 otherwise. Industry homogeneity is calculated as the median, across all firms in an industry, of the percentage variation in monthly stock returns that is explained by an equally weighted industry index over the previous 10 years.

Other variables

AbsFE	Forecast uncertainty, measured as the absolute value of [(the current quarter EPS minus the first consensus analyst forecast of EPS in the current quarter)/closing price of the previous quarter].
AM	Performance-adjusted discretionary accruals (Kothari et al. 2005), where discretionary accruals are measured as deviations from the predicted values of the industry-quarter regression $TAcc_q/A_{q-1} = \alpha_0 + \alpha_1(1/A_{q-1}) + \alpha_2(\Delta S_q/A_{q-1}) + \alpha_3(PPE_q/A_{q-1}) + \varepsilon;$ where $TAcc_q$ is measured as $(Data40_q - Data40_{q-1}) - (Data36_q - Data36_{q-1}) - (Data49_q - Data49_{q-1}) + (Data45_q - Data45_{q-1}) - Data5_q$; A_{q-1} is total assets in quarter q-1; ΔS_q is change in sales $(Data2_q - Data2_{q-1})$; PPE_q is property, plant, and equipment $(Data118_q)$.
CashPay	The log value of salary (SALARY) plus bonus (BONUS) of a CEO.
CashPay%	The sum of salary and bonus (SALARY and BONUS) as a percentage of total compensation (TDC1) of a CEO.
IFE	Initial analyst forecast bias, measured as [(the first consensus analyst forecast of EPS in the current quarter minus the current quarter EPS)/closing price of the previous quarter].
IndPay	The average of the log value of the total compensation of CEOs in the same industry.
LogMktCap	The natural logarithm of market capitalization at the beginning of the quarter, $(Data14 \times Data61)_{q-1}$.
Loss	An indicator variable set to 1 if a firm has a negative EPS for the past four quarters consecutively.
MBExp	An indicator variable set to 1 if actual EPS is equal to or greater than the last analyst consensus forecast of EPS prior to the earnings release, and 0 otherwise.
NewOptions%	The number of newly granted stock options to a CEO as a percentage of shares outstanding.
NumEst	The number of analyst estimates in the first analyst forecast consensus of the quarter.
NOA	Net operating assets scaled by total assets at the end of previous quarter (Hirshleifer et al. 2004).
Ownership	The number of stocks owned by a CEO as a percentage of shares outstanding.
OtherPay	The logarithmic value of other annual compensation (OTHANN, other annual compensation not properly categorized as salary or bonus) to a CEO.
OtherPay%	Other annual compensation (OTHANN) as a percentage of total compensation (TDC1) to a CEO.
Persist	The number of the past four firm-quarters when a firm met/beat analyst forecasts, divided by 4.
RM	Sum of abnormal discretionary expenses and abnormal production costs. Abnormal discretionary expenses is measured as deviations from the predicted values of the industry-quarter regression $Disx_q/A_{q-1} = \alpha_0 + \alpha_1(1/A_{q-1}) + \alpha_2(S_{q-1}/A_{q-1}) + \varepsilon,$ multiplied by -1. Abnormal production costs is measured as deviations from the predicted values of the industry-quarter regression $Prod_q/A_{q-1} = \alpha_0 + \alpha_1(1/A_{q-1}) + \alpha_2(S_q/A_{q-1}) + \alpha_3(\Delta S_q/A_{q-1}) + \alpha_3(\Delta S_{q-1}/A_{q-1}) + \varepsilon;$ A_{q-1} is total assets in quarter q-1; S_q is sales $(Data2_q)$; ΔS_q is change in sales $(Data2_q - Data2_{q-1})$;

	$Disx_q$ is discretionary expenses ($Data4_q$ + $Data45_t/4$ + $Data1_q$; there is no quarterly advertising expenses data, and we use the average advertising expenses during the year); $Prod_q$ is production costs ($Data30_q$ + $Data38_q$ - $Data38_{q-1}$).
ROA	Income before extraordinary items scaled by total assets at the beginning of the quarter, $Data18_q/Data6_{q-1}$.
Sens	Price sensitivity to earnings surprises, measured as the three-day-window cumulative abnormal return around the earnings announcement of the previous quarter, divided by the dollar surprise in the EPS of the previous quarter. Abnormal returns are stock returns minus returns on the CRSP value-weighted index; the dollar surprise in EPS is measured as the last consensus analyst forecast for quarter q-1 minus the actual EPS for quarter q-1. Whenever the estimated sensitivity is zero or negative, we estimate the same ratio in quarter q-2.
ΔGDP	Percentage change in the state's real gross domestic product for year *t*.
SeverancePay	An indicator variable set to 1 if a firm provides severance packages to its executives and 0 otherwise. We identify severance packages using a keyword search of a firm's SEC filings including 10-K, 10-Q, DEF14A, 8K, and Exhibit-10. Our search keyword is "severance", which must appear within 10 words of "employment", "termination", "geographic", "employed", or "terminate".
SeverancePayAmount	The natural logarithm of 1 plus severance payment from ExecuComp.
Tenure	The number of years since the CEO's year of appointment until the fiscal quarter end.
Turnover	An indicator variable set to 1 if the CEO leaves the firm within one quarter, and 0 otherwise.
UnExVOptions%	The number of unexercised vested options held by a CEO as a percentage of shares outstanding.
UnExUnVOptions%	The number of unexercised unvested options as a percentage of shares outstanding.

Figure 1A Trend Analysis of *ExpMgt*

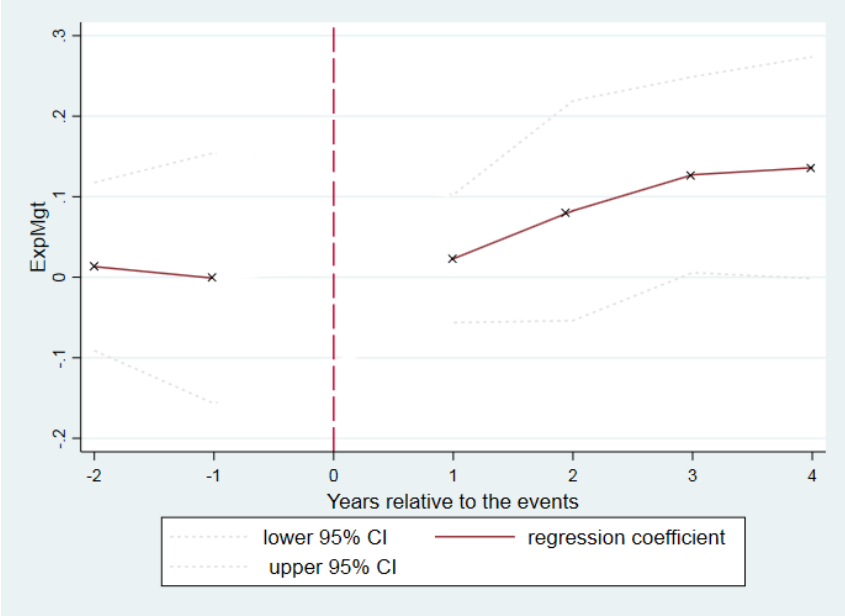

Figure 1B Trend Analysis of *D_ExpMgt*

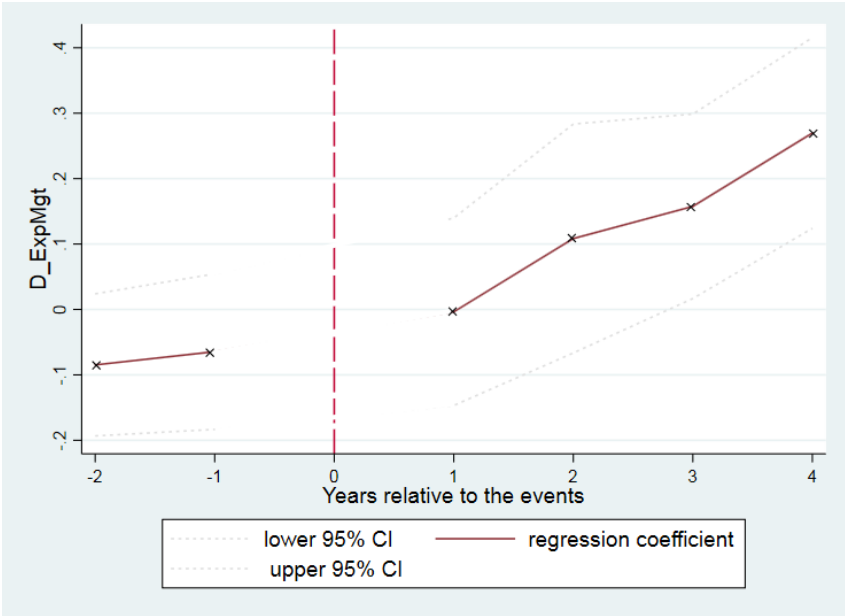

Figure 1A (1B) reports for the point estimates from a trend analysis of *ExpMgt* (*D_ExpMgt*) on the increase in non-compete enforcement, control variables, firm-CEO-cohort fixed effects, and year-quarter-cohort fixed effects based on the non-compete enforcement increase events. The specification is the same as that reported in column 3 (4) of Table 2 except that we exclude the observations of the event quarters and allow the effect of increase in non-compete enforcement to vary by year. 95% confidence intervals, adjusted for clustering at state levels, are also plotted.

TABLE 1
Descriptive statistics and correlation matrix
Panel A: Descriptive statistics

	All Events (95,689)		Enforcement Increase Events (51,754)		Enforcement Decrease Events (43,935)	
	Mean	Median	Mean	Median	Mean	Median
IncreaseEnforce	-0.001	0.000	0.014	0.000	-0.019	0.000
ExpMgt	0.145	0.000	0.144	0.000	0.146	0.000
D_ExpMgt	0.277	0.000	0.278	0.000	0.276	0.000
NOA	0.556	0.586	0.557	0.587	0.555	0.586
NumEst	9.243	8.000	9.292	8.000	9.184	8.000
LogMktCap	7.686	7.524	7.701	7.542	7.668	7.502
Persist	0.774	0.750	0.772	0.750	0.777	0.750
AbsFE	0.007	0.002	0.007	0.002	0.007	0.001
ROA	0.013	0.015	0.014	0.015	0.013	0.015
Loss	0.037	0.000	0.035	0.000	0.040	0.000
Sens	1.739	0.812	1.686	0.798	1.802	0.838
AM	-0.004	-0.004	-0.004	-0.003	-0.005	-0.004
ΔGDP	0.011	0.014	0.010	0.013	0.012	0.015
Tenure	6.484	5.000	6.485	5.000	6.481	5.000

Panel B: Correlation matrix (enforcement increase events)

		1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**
1	*IncreaseEnforce*		**0.015**	**0.011**	**-0.035**	0.006	**0.040**	**-0.014**	0.001	**-0.013**	**0.021**	**-0.014**	0.000	**-0.049**	**-0.009**
2	*ExpMgt*	**0.016**		**0.668**	**0.052**	**0.016**	**-0.019**	**-0.065**	**-0.008**	**-0.102**	0.001	0.004	**0.011**	**-0.028**	-0.003
3	*D_ExpMgt*	**0.011**	**0.838**		**0.074**	**0.049**	**-0.040**	**-0.127**	**0.061**	**-0.149**	-0.008	**-0.017**	0.000	**-0.031**	0.006
4	*NOA*	**-0.037**	**0.065**	**0.068**		**-0.061**	0.003	**-0.063**	**-0.065**	**-0.045**	**-0.108**	**0.021**	**-0.019**	**0.044**	**0.059**
5	*NumEst*	**0.023**	**-0.011**	**0.053**	**-0.064**		**0.531**	**0.158**	**-0.053**	**0.051**	0.006	-0.002	**-0.040**	**0.024**	**0.075**
6	*LogMktCap*	**0.038**	**-0.056**	**-0.039**	**-0.026**	**0.540**		**0.243**	**-0.223**	**0.188**	**-0.118**	**-0.050**	-0.003	**0.060**	**-0.028**
7	*Persist*	**-0.013**	**-0.112**	**-0.132**	**-0.077**	**0.178**	**0.241**		**-0.172**	**0.201**	**-0.127**	**-0.030**	**-0.013**	**0.060**	-0.005
8	*AbsFE*	**0.029**	0.003	**0.106**	**-0.101**	**-0.113**	**-0.369**	**-0.235**		**-0.283**	**0.184**	**-0.043**	-0.008	**-0.066**	0.001
9	*ROA*	**-0.019**	**-0.180**	**-0.177**	**-0.076**	**0.077**	**0.227**	**0.249**	**-0.334**		**-0.275**	**0.023**	**0.149**	**0.060**	**0.017**
10	*Loss*	**0.021**	0.005	-0.008	**-0.096**	0.001	**-0.110**	**-0.118**	**0.181**	**-0.242**		**-0.013**	0.002	-0.006	**-0.016**
11	*Sens*	**-0.022**	-0.000	**-0.024**	**0.045**	**-0.016**	**-0.064**	**-0.030**	**-0.128**	**0.063**	**-0.031**		**-0.018**	**0.020**	0.002
12	*AM*	-0.002	**0.021**	**0.012**	**-0.008**	**-0.051**	-0.006	**-0.021**	**0.011**	**-0.011**	**0.013**	**-0.024**		**-0.012**	**0.021**
13	*ΔGDP*	**-0.044**	**-0.031**	**-0.042**	**0.032**	**0.009**	**0.042**	**0.066**	**-0.112**	**0.069**	0.004	-0.003	**-0.010**		**0.023**
14	*Tenure*	**-0.009**	-0.003	0.003	**0.061**	**0.041**	**-0.032**	**0.015**	**-0.025**	**0.021**	**-0.038**	0.005	0.003	**0.029**	

Panel C: Correlation matrix (enforcement decrease events)

		1	2	3	4	5	6	7	8	9	10	11	12	13	14
1	*IncreaseEnforce*		0.003	-0.004	-0.004	-0.006	**-0.015**	**0.015**	-0.002	**0.018**	**-0.015**	-0.000	-0.001	-0.007	-0.004
2	*ExpMgt*	0.002		**0.678**	**0.051**	**0.011**	**-0.032**	**-0.070**	-0.005	**-0.108**	0.001	0.006	**0.012**	**-0.030**	-0.003
3	*D_ExpMgt*	-0.004	**0.838**		**0.072**	**0.039**	**-0.057**	**-0.129**	**0.067**	**-0.153**	-0.004	**-0.014**	0.004	**-0.041**	0.006
4	*NOA*	-0.006	**0.065**	**0.066**		**-0.077**	-0.003	**-0.063**	**-0.064**	**-0.025**	**-0.124**	0.005	**-0.021**	**0.028**	**0.061**
5	*NumEst*	**-0.016**	**-0.016**	**0.042**	**-0.079**		**0.522**	**0.166**	**-0.052**	**0.038**	**0.011**	0.006	**-0.046**	**0.015**	**0.081**
6	*LogMktCap*	**-0.018**	**-0.066**	**-0.055**	**-0.033**	**0.532**		**0.238**	**-0.223**	**0.186**	**-0.117**	**-0.042**	-0.008	**0.050**	**-0.014**
7	*Persist*	**0.014**	**-0.113**	**-0.134**	**-0.077**	**0.181**	**0.237**		**-0.175**	**0.203**	**-0.126**	**-0.023**	**-0.012**	**0.065**	**-0.010**
8	*AbsFE*	0.003	**0.022**	**0.128**	**-0.092**	**-0.111**	**-0.371**	**-0.242**		**-0.283**	**0.183**	**-0.048**	**-0.014**	**-0.068**	-0.001
9	*ROA*	**0.013**	**-0.193**	**-0.187**	**-0.068**	**0.074**	**0.231**	**0.254**	**-0.342**		**-0.310**	**0.024**	**0.166**	**0.048**	**0.018**
10	*Loss*	**-0.015**	0.009	-0.004	**-0.107**	0.009	**-0.108**	**-0.120**	**0.189**	**-0.261**		**-0.015**	-0.009	0.008	**-0.020**
11	*Sens*	0.003	-0.003	**-0.025**	**0.037**	-0.002	**-0.046**	**-0.022**	**-0.142**	**0.069**	**-0.037**		**-0.022**	**0.042**	-0.002
12	*AM*	-0.004	**0.021**	**0.014**	**-0.010**	**-0.059**	**-0.014**	**-0.024**	**0.012**	**-0.008**	0.004	**-0.025**		**-0.017**	**0.028**
13	*ΔGDP*	0.003	**-0.036**	**-0.051**	**0.020**	0.001	**0.026**	**0.071**	**-0.116**	**0.063**	**0.021**	**0.010**	**-0.018**		**0.018**
14	*Tenure*	**-0.012**	-0.004	0.002	**0.059**	**0.051**	**-0.027**	**0.012**	**-0.017**	**0.023**	**-0.041**	-0.002	**0.011**	**0.028**	

Panel A reports the descriptive statistics of variables. Panel B (Panel C) presents the correlation matrix for the sample of enforcement increase (decrease) events. Correlation coefficients in bold indicate significance at the 0.05 level or better. Pearson correlations are reported in the upper triangle, and spearman correlations are reported in the bottom triangle. All continuous variables are winsorized at the 1% and 99% percentiles. See Appendix 2 for variable definitions.

TABLE 2
Main effect of non-compete enforceability on expectation management

Panel A: Main tests

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Events	All Events		Increase Events Only		Decrease Events Only	
Dependent Var.=	*ExpMgt*	*D_ExpMgt*	*ExpMgt*	*D_ExpMgt*	*ExpMgt*	*D_ExpMgt*
IncreaseEnforce	0.049	0.063*	0.083**	0.103***	-0.038	-0.020
	(1.42)	(1.87)	(2.27)	(3.36)	(-1.28)	(-0.26)
NOA	0.146***	0.411***	0.163***	0.440***	0.127**	0.381***
	(6.75)	(3.95)	(2.82)	(4.32)	(2.19)	(3.43)
Sens	0.001**	-0.001	0.001	-0.001	0.001	-0.001
	(2.74)	(-0.24)	(0.31)	(-0.30)	(0.40)	(-0.17)
NumEst	0.024***	0.083***	0.024***	0.082***	0.023***	0.083***
	(19.11)	(14.56)	(9.54)	(13.85)	(9.86)	(15.03)
LogMktCap	0.058***	0.136***	0.067***	0.153***	0.048**	0.118***
	(4.47)	(3.75)	(3.79)	(4.32)	(2.46)	(3.00)
Persist	-0.161***	-0.301***	-0.156***	-0.292***	-0.168***	-0.312***
	(-17.60)	(-5.42)	(-6.38)	(-5.40)	(-7.01)	(-5.18)
AbsFE	-1.889***	-0.168	-1.897***	-0.272	-1.875***	-0.041
	(-15.99)	(-0.24)	(-3.79)	(-0.39)	(-3.81)	(-0.06)
ROA	-1.473***	-4.509***	-1.449***	-4.574***	-1.503***	-4.446***
	(-30.56)	(-6.82)	(-5.31)	(-6.81)	(-5.48)	(-6.64)
Loss	-0.051***	-0.233***	-0.043	-0.237***	-0.060	-0.230***
	(-6.11)	(-3.31)	(-1.03)	(-3.15)	(-1.58)	(-3.33)
AM	0.381***	0.642**	0.351***	0.557*	0.416***	0.742**
	(12.75)	(2.14)	(3.09)	(1.83)	(3.62)	(2.43)
ΔGDP	-0.779***	-1.883	-0.719	-1.678	-0.881	-2.154
	(-7.48)	(-1.42)	(-1.17)	(-1.29)	(-1.52)	(-1.53)
Tenure	-0.032***	0.010	0.002	0.006	0.022	0.015
	(-4.11)	(0.76)	(0.10)	(0.49)	(0.73)	(1.06)
CEO-Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2/Pseudo R^2	0.091	0.090	0.090	0.089	0.093	0.091
No. of Obs.	95,689	95,689	51,754	51,754	43,935	43,935

TABLE 2 CONT'D

Panel B: Robustness tests on alternative samples (enforcement increase events only)

	Dependent variable	Coefficient on *IncreaseEnforce*	*t- [z-]* statistic	N	R^2
Exclude Observations from California					
(1)	*ExpMgt*	0.078**	2.57	43,042	0.095
(2)	*D_ExpMgt*	0.083***	[3.81]	43,042	0.088
Exclude Observations in Business Service Sector					
(3)	*ExpMgt*	0.082**	2.50	45,700	0.095
(4)	*D_ExpMgt*	0.099***	[3.85]	45,700	0.090
Exclude Observations Prior to Reg FD					
(5)	*ExpMgt*	0.081**	2.23	50,480	0.090
(6)	*D_ExpMgt*	0.098***	[3.26]	50,480	0.089
Include Enforceability Changes by Court Rulings Only					
(7)	*ExpMgt*	0.080**	2.12	41,461	0.089
(8)	*D_ExpMgt*	0.088***	[3.08]	41,461	0.089
Control variables		Yes			
CEO-Firm-Cohort FE		Yes			
Year-Quarter-Cohort FE		Yes			

TABLE 2 CONT'D

Panel C: Robustness to alternative specifications (enforcement increase events only)

	Dependent variable	Coefficient on *Variable of interest*	*t- [z-]* statistic	N	R^2
Control for the Presence of Severance Pay (SeverancePay)					
(1)	*ExpMgt*	0.082**	2.24	51,754	0.090
(2)	*D_ExpMgt*	0.107***	[3.29]	51,754	0.090
Control for the Amount of Severance Pay (SeverancePayAmount)					
(3)	*ExpMgt*	0.041*	1.93	37,432	0.085
(4)	*D_ExpMgt*	0.073*	[1.85]	37,432	0.088
Control for CEO Compensation Structures (CashPay, OtherPay, CashPay%, OtherPay%, NewOptions%, UnExVOptions%, UnExUnVOptions%, Ownership, IndPay)					
(5)	*ExpMgt*	0.082*	1.95	49,539	0.096
(6)	*D_ExpMgt*	0.116***	[4.58]	49,539	0.094
Control for Initial Analyst Forecast Errors (IFE)					
(7)	*ExpMgt*	0.089**	2.34	51,754	0.094
(8)	*D_ExpMgt*	0.105***	[3.44]	51,754	0.101
Alternative Measures of Expectation Management					
(9)	*ExpMgt_IndResid*	0.084**	2.01	51,686	0.082
(10)	*ExpMgt_LagResid*	0.105***	2.86	42,336	0.096
(11)	*MFBad*	0.142**	[2.29]	18,205	0.055
(12)	*NegTone_ConCall*	0.020***	3.38	32,358	0.317
Consider CFO effects					
(13)	*ExpMgt*	0.096**	2.00	47,545	0.095
(14)	*D_ExpMgt*	0.100***	[3.24]	47,545	0.089
Control variables		Yes			
CEO-Firm-Cohort FE		Yes			
Year-Quarter-Cohort FE		Yes			

Panel A presents the effect of the change in non-compete enforceability on the use of expectation management. Appendix 2 lists variable definitions. Intercepts and fixed effects are not reported for brevity. The numbers in parentheses are *t*-statistics for the OLS regressions and *z*-statistics for Probit regressions, all estimated 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.

Panel B presents the effect of non-compete enforceability increases on the use of expectation management across various subsamples. Appendix 2 lists variable definitions. Only coefficients on *IncreaseEnforce* are reported. Intercepts, control variables, and fixed effects are not reported for brevity. Control variables included are the same as those in Table 2 Panel A. *t*-statistics for the OLS regressions and *z*-statistics for Probit regressions are estimated 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.

In Panel C, we examine alternative explanations and the robustness to alternative specifications. See Appendix 2 for all variable definitions, including new variables in parentheses. Intercepts, control variables, and fixed effects are not reported for brevity. Control variables in Table 2 Panel A are included in all models, except in Rows (7) and (8), where we drop *AbsFE* because it is highly correlated with *IFE*; and in Rows (13) and (14), where we replace CEO-firm-cohort fixed effects and CEO tenure with CFO-firm-cohort fixed effects and CFO tenure. In Rows (5) and (6), CEO compensation variables include 9 variables measured on an annual basis. Appendix 2 lists variable definitions. The sample size varies because some additional variables are not available for all observations. *t*-statistics for the OLS regressions and *z*-statistics for Probit regressions are estimated 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
Comparing Various Tools for Meeting- and-Beating-Expectation
A Path Analysis (Enforcement Increase Events Only)

	Path Coeff. (*t*-stat.)
P(*IncreaseEnforce*, *ExpMgt*) = a_1	0.013***
	(2.88)
P(*IncreaseEnforce*, *AM*) = b_1	-0.002
	(-0.33)
P(*IncreaseEnforce*, *RM*) = c_1	0.010**
	(2.18)
P(*ExpMgt*, *MBExp*) = d_2	0.090***
	(19.57)
P(*AM*, *MBExp*) = d_3	0.006
	(1.28)
P(*RM*, *MBExp*) = d_4	0.028***
	(5.94)
Mediated Path of P(*IncreaseEnforce*, *MBExp*)	
ExpMgt = $a_1 \times d_2$	1.184***
	(2.85)
AM = $a_1 \times d_3$	-0.009
	(-0.32)
RM = $a_1 \times d_4$	0.276**
	(2.04)
Control variables	Yes
CEO-Firm-Cohort FE	Yes
Year-Quarter-Cohort FE	Yes
# of obs.	47,533

In this table, we report the results of a path analysis for assessing relative importance of various tools to achieve meeting-and-beating-expectations. See Appendix 2 for variable definitions. P(X1, X2) stands for the standardized path coefficients. Intercepts, control variables, and fixed effects are included but not reported for brevity. Control variables included are the same as those included in Table 2 Panel A, except that we exclude *AM* from the models. The magnitude of the estimated mediated paths is multiplied by 1,000 for readability. *t*-statistics of the coefficients are reported in parentheses. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

TABLE 4
Moderating factors at the manager, firm, and industry levels (enforcement increase events only)
Panel A: Dependent variable = *ExpMgt*

Factor =	(1) *HighGenAbility*	(2) *Seniority*	(3) *BoardIndep*	(6) *HighIndHomo*
IncreaseEnforce	0.182**	0.149**	0.059**	0.007
	(2.08)	(2.33)	(2.13)	(0.38)
IncreaseEnforce×*Factor*	-0.129**	-0.135**	0.080**	0.151**
	(-2.31)	(-2.22)	(2.11)	(2.54)
Factor	0.006	0.030	-0.013	0.013
	(0.12)	(0.75)	(-0.68)	(0.68)
Tests on *IncreaseEnforce* + *IncreaseEnforce*×*Factor*	0.052	0.014	0.139**	0.159***
	(1.04)	(0.42)	(2.34)	(3.07)
Control variables	Yes	Yes	Yes	Yes
CEO-Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Quarter-Cohort FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.109	0.100	0.076	0.090
No. of Obs.	21,942	33,637	42,297	51,754

TABLE 4 (CONT'D**)**
Panel B: Dependent variable = *D_ExpMgt*

Factor =	(1) *HighGenAbility*	(2) *Seniority*	(3) *BoardIndep*	(6) *HighIndHomo*
IncreaseEnforce	0.182**	0.232***	0.065	-0.043
	(2.50)	(5.14)	(0.96)	(-0.79)
IncreaseEnforce×*Factor*	-0.190*	-0.085*	0.087**	0.199**
	(-1.69)	(-1.68)	(2.57)	(2.53)
Factor	0.008	0.125	0.011	0.024
	(0.16)	(1.11)	(0.33)	(0.65)
Tests on *IncreaseEnforce* + *IncreaseEnforce*×*Factor*	-0.008	0.146***	0.152**	0.156**
	(-0.15)	(3.47)	(2.02)	(2.32)
Control variables	Yes	Yes	Yes	Yes
CEO-Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Quarter-Cohort FE	Yes	Yes	Yes	Yes
Pseudo R^2	0.099	0.093	0.089	0.091
No. of Obs.	21,942	33,637	42,297	51,754

In this table, we exam the varying impact of non-compete enforcement increase across different manager, firm, and industry characteristics. *Factor* represents *HighGenAbility* in column (1), *Seniority* in column (2), *BoardIndep* in column (3), and *HighIndHomo* in column (4), respectively. The sample size varies because some cross-sectional variables are not available for all observations. Intercepts, control variables, and fixed effects are not reported for brevity. Control variables are the same as those included in Table 2 Panel A. See Appendix 2 for variable definitions. The numbers in parentheses are *z*-statistics, all estimated 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 5
Validation test of labor market mobility based on CEO turnover
A path analysis (enforcement increase events only)

	(1)
P(*IncreaseEnforce*, *ExpMgt*) = a_1	0.015***
	(3.45)
P(*ExpMgt*, *MBExp*) = b_2	0.094***
	(21.44)
P(*MBExp*, *Turnover*) = c_3	-0.011**
	(-2.48)
Mediated Path of P(*IncreaseEnforce*, *Turnover*)	
ExpMgt = $a_1 \times b_2 \times c_3$	-0.016**
	(-2.00)
Control variables	Yes
CEO-Firm-Cohort FE	Yes
Year-Quarter-Cohort FE	Yes
# of obs.	51,754

In this table, we report the results of a path analysis on CEO turnover. See Appendix 2 for variable definitions. P(X1, X2) stands for the standardized path coefficients. Constant terms, control variables, and fixed effects are included but not reported for brevity. Control variables included are the same as those included in Table 2 Panel A. The magnitude of the estimated mediated path P(*IncreaseEnforce*, *Turnover*) is multiplied by 1,000 for readability. *t*-statistics are reported in parentheses. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.