# Ties that Truly Bind: Non-competition Agreements, Executive Compensation and Firm Investment

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**Abstract**

We study the effects of non-competition agreements by analyzing time series and cross-sectional variation in the enforceability of these contracts across U.S. states. We find that increased enforceability reduces executive compensation and shifts its form towards greater use of salary. We also show that tougher non-competition enforcement reduces research and development spending and capital expenditures per employee. Non-competition agreements promote executive stability and board participation, but higher quality managers apparently shun firms in high-enforcement jurisdictions. Our results have implications for theories of executive compensation and firm organization.

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# 1 Introduction

For most firms, the human capital of their employees is a core asset, but it is one over which they cannot exercise full ownership. Non-competition agreements (also known as covenants not to compete) are contracts that restrict workers from joining (or forming) a rival company, and they represent one of the most important mechanisms binding employees to a firm. In this paper we make use of time series and cross-sectional variation in non-competition enforceability across the states of the U.S. to analyze the effects of these agreements. We find that increased enforceability significantly reduces executive compensation and shifts its form towards a heavier reliance on salary. We also show that tougher non-competition enforcement reduces research and development (R&D) spending and capital expenditures per employee. These findings provide evidence on several theories of executive compensation and firm organization. Our results demonstrate that non-competition agreements promote executive stability and increase the probability that managers will serve on their own firms' boards, but they also come with a cost not emphasized in the theoretical literature: higher quality managers apparently shun firms in high-enforcement jurisdictions. Non-competition regulations thus help to determine optimal firm choices on a wide set of issues including executive pay and recruitment, governance and investment strategy.

The inalienability of human capital (Hart and Moore, 1994, Diamond and Rajan, 2000) is universally accepted in developed economies as a basic personal right. This right comes, however, with certain accompanying costs. Allowing employees to depart from companies at will discourages employers from making investments in their workers (for example, in the form of training) and reduces the probability that employees will be made privy to firm secrets or strategies (Rajan and Zingales, 2001). Theories of the role of human capital in corporations suggest that non-competition agreements can alter firm boundaries by more closely tying employees to a company and its physical assets.

Specifically, we consider two potential effects of non-competition arrangements on the relationship between the firm and its employees. First, there is the direct effect that a manager who has signed such an agreement will find it difficult to leave his current firm. From the firm's perspective, this effect gives the manager's human capital more of the character of a fixed asset, but it also affects the manager's incentives to exert effort. The direct effect will hold for any given manager with a fixed level of human capital.

Second, there is a selection or indirect effect. High quality managers who have private information about their type will place a higher value on the option to access the labor market after

proving their worth (Holmstrom and Ricart i Costa, 1986, Gibbons and Murphy, 1992b), and will thus avoid joining firms that require covenants not to compete, thereby generating a negative selection effect in the quality of managers at companies that require non-competition agreements. The indirect effect is that managers who have signed non-competition arrangements may be unwilling to invest in their own human capital, because it is essentially partially owned by the firm. Both the selection and indirect effects suggest that managers who have signed non-competition agreements will have less human capital than those who have not. Since it is hard to distinguish the selection and indirect effects empirically, we group them together. The direct and selection/indirect effects both generate a number of implications for executive compensation, board participation and firm investment, some of which coincide and some of which are distinct. We find support for both effects, but the evidence, particularly on R&D spending, suggests that the selection/indirect effect is of larger magnitude.

We make use of data on state regulations and the Execucomp database of executive compensation to analyze the effects of non-competition enforceability. We perform two types of tests. Our times series tests consider changes in non-competition enforceability law that took place in three U.S. states. These tests employ firm fixed effects to analyze the impact of the legal shifts, controlling for all firm-specific variables. Our cross-sectional tests analyze differences in enforceability across all states. We argue that non-competition law is particularly important to firms with substantial within-state competition, since covenants not to compete typically have limited geographic scope and are easiest to enforce in the same legal jurisdiction. We then use the interaction between enforceability and the extent of in-state competition as a measure of the power and relevance of non-competition law for a given firm. We include state fixed effects in our cross-sectional tests to control for differences between states unrelated to non-competition enforceability, and we also control for industry effects.[1]

Our first finding is that enforceability strongly reduces executive mobility, particularly decreasing the likelihood that a firm will experience a within-industry managerial transfer (either in or out). In a related result, we show that executives in high-enforcement jurisdictions have longer job tenures. Thus, covenants not to compete do serve to attach human capital assets to companies in the way emphasized by recent theories of the firm.

Increased non-competition enforceability allows for a richer set of contracts between firms and employees, so it is plausible to argue that executive compensation should be higher in high-

[1] In previous empirical work on non-competition enforceability, Stuart and Sorenson (2002) link non-competition law to local business founding rates and Kaplan and Stromberg (2003) find that venture capitalists frequently require entrepreneurs to sign non-competition agreements.

enforcement areas. Conversely, increased enforceability may shift bargaining power to firms by reducing managers' outside options, as suggested by the direct effect, and thereby result in lower compensation. The selection/indirect effect emphasizes that managers who have signed non-competition agreements may have lower quality human capital and may thus merit lower compensation. Our empirical findings are that executive compensation is both lower and more salary-based in high enforcement jurisdictions. These results hold in specifications with fixed effects at the executive level, which provides support for the direct effect, and in the cross-sectional regressions, thereby generating evidence consistent with the selection/indirect effect. More broadly, these findings indicate that the regulatory environment can have an important effect on compensation levels and complement the recent empirical literature on optimal executive contracts (Kole, 1997, Hermalin and Wallace, 2001, Gillan, Hartzell and Parrino, 2005).

The managerial stability and lower compensation associated with non-competition contracts yield clear advantages to firms. We show, however, that enforceability also has a negative impact on the quality of managers, as suggested by the selection/indirect effect. We find that managers in firms in high-enforcement areas receive relatively smaller salary increases when they transfer firms (even though their current salaries are also relatively low) and are less likely to be named CEO at their new firms. When they are appointed to a CEO position, it is typically at a smaller firm. We view these findings as evidence in support of the selection/indirect effect which predicts that managers in tough enforcement jurisdictions will have lower human capital.

Our work draws a link between managerial incentives and compensation and firm investment strategy (Gibbons and Murphy, 1992a, Oyer, 1998). The direct effect suggests that firms in high enforcement areas should be relatively more willing to make R&D investments, because they are able to secure the intellectual assets of their employees. By contrast, the selection/indirect effect implies that the high human capital employees required to make R&D investments successful will only be available to firms in low-enforcement jurisdictions. Controlling for fixed effects at either the firm (time series analysis) or industry and state (cross-sectional analysis) levels, we find that non-competition enforceability reduces R&D spending. When a firm determines its R&D investment policy, the negative selection effects of non-competition agreements outweigh the direct benefits. Our findings indicate that low-enforcement jurisdictions have a comparative advantage in funding R&D. We also show that firms in low-enforcement states engage in production that is relatively capital-intensive. This is consistent with these firms' hiring a relatively skilled labor force or with a strategy on their part to reduce the number of potentially deserting employees.

Managers who sign a non-competition agreement can more safely be entrusted with the authority

required to make long-term decisions, as suggested by the direct effect. In support of this argument, we find that managers are more likely to be board members at their own firms in high-enforcement jurisdictions. The increased presence of inside directors might be thought to induce agency problems (Weisbach, 1988, Rosenstein and Wyatt, 1990). The data do not support that hypothesis, however: non-competition enforceability is statistically unrelated to the Gompers, Ishii and Metrick (2003) governance measure.

We find no evidence that the enforceability regime affects either firm market to book ratios or profitability. The relative benefits and costs of non-competition agreements appear to balance in practice. This is perhaps not surprising, given that firms in high enforcement jurisdictions have the option to require non-competition contracts but need not do so. It is plausible that in equilibrium just enough firms choose to insist on non-competition contracts such that the net benefits from doing so are zero.

Our study applies the insight of the law and finance literature (La Porta, Lopez-de-Silanes, Shleifer and Vishny 1997, 1998, 1999, 2000, Beck, Demirguc-Kunt and Levine, 2003) that legal institutions can have a strong effect on the structure and workings of the firm. In our setting, however, regulations vary at the state level within a given country and firms can opt-out of the local enforcement regime by not requiring non-competition agreements from workers. Labor and capital mobility across states can thus lead to circumstances under which firms in different jurisdictions have noticeably dissimilar executive compensation and investment strategies but enjoy similar profitability.

In examining the implications of non-competition enforceability for merger and acquisition activity, we show that firms in high enforcement areas are not more likely to be acquired, despite the assurance they offer acquirers that employees will not quickly depart the firm. It may be that acquirers do not value the stable and cohesive cultures at high-enforcement-jurisdiction firms, in part because they plan to refashion operations at the target. We find mixed evidence on whether firms in tough enforcement regions are more likely to make an acquisition in any given year, but we do find that they engage in more extensive acquisitions in years in which they are active. This may reflect their ability to more successfully assimilate large targets, though the data do not permit clear conclusions on this question.

The remainder of the paper is organized as follows. Section 2 delineates the predictions we test. Section 3 details the data, and we outline the empirical strategy in Section 4. The results are analyzed in Section 5, and we conclude in Section 6.

## 2 Predictions

Non-competition agreements are contracts signed by employees and firms that prohibit employees from joining (or forming) a competing firm. The agreements usually specify a time period and geographic location within which the employee agrees not to compete with his current employer. Employees from senior managers to salespeople may be required to sign a covenant not to compete, and an agreement accepted at the time of hiring will typically continue to hold throughout the employment relationship and will extend beyond the end of the relationship for a contractually specified period.

The enforceability of non-competition agreements varies substantially across the states of the U.S. In high-enforceability jurisdictions courts are willing to implement agreements of long duration and wide geographical scope, even if employees entered into such contracts with no reciprocal compensation other than employment. In low-enforceability jurisdictions it can be very difficult to enforce any non-competition agreement at all. In our discussion of our empirical strategy in Section 4, we will discuss the time-series and cross-sectional variation in enforceability in the U.S. since the early 1990's. We will make use of this variation to test several theoretical hypotheses about the impact of non-competition agreements.

We examine two potential effects of these contracts. First, there is the direct effect that the manager cannot easily leave his current firm. His being bound to the firm will help to determine the manager's incentives to work hard and to choose appropriate projects. This direct effect will hold for any given manager with a fixed level of human capital. Second, there is a selection or indirect effect. The selection effect arises from the fact that high quality managers who have private information about their type will greatly value the option to leave their current firm and seek outside opportunities with other companies in the future. The implication of these career concerns (Holmstrom and Ricardo-i-Costa, 1986) is that good managers may shun firms that require non-competition agreements. The indirect effect is that managers who have signed non-competition arrangements may be reluctant to invest in their own human capital, because the firm essentially has an ownership claim on their labor output. Both the selection and indirect effects suggest that managers who have signed non-competition agreements will have less human capital than those who have not. Our data do not permit us to distinguish between the selection and indirect effects, so we group them together.

Our first prediction is that the enforcement of non-competition agreements will diminish executive mobility. Non-competition agreements should especially reduce within-industry mobility,

since the agreements restrict movement of employees to competitors. (Agreements that prevent employees from working for any other firm are never enforced in any jurisdiction, since they place too heavy a burden on departing workers.)

**Prediction 1.** *There are fewer within-industry executive transfers in high-enforceability jurisdictions.*

It is clear that there should be fewer within-industry transfers *from* firms in high-enforceability jurisdictions, but Prediction 1 also suggests that there should be fewer transfers to firms in such areas. There are two reasons for this. First, within-state transfers likely involve the lowest personal costs for executives (and hence should be relatively frequent), and firms in high-enforcement states will be unable to hire executives who have signed non-competition agreements with other firms in their state. Second, during most of our sample period there was a certain ambiguity about whether the law governing the enforcement of a non-competition agreement was the law of the state of the employee's old firm or the law of the state of his new firm. Tougher non-compete enforcement in a firm's state made it more difficult for the firm to hire out-of-state executives who had signed non-competition agreements with other companies (Pentelovitch, 2003).

Since non-competition agreements limit executives' ability to depart their current firm, they should also be associated with longer tenures.

**Prediction 2.** *Executives' job tenures are longer in high-enforceability jurisdictions.*

We now consider the implications of the direct and selection/indirect effects for executive compensation and firm investment. Many of these effects are complementary, but in some cases we distinguish between implications arising from the direct and selection/indirect mechanisms. The direct effect of non-competition agreements on executive compensation is that they shift power to firms and away from managers, and that they should therefore be associated with lower executive pay. A related idea is that managers sign non-competition agreements early in their careers in exchange for higher compensation or other perquisites. If the managers are successful and are promoted, the non-competition agreement binds; other firms would like to hire the managers but cannot. Consequently, non-competition arrangements reduce the outside options, and hence the compensation, of the highest level executives in the firm, but may have no effect on the average compensation. The selection/indirect effect also yields the same implication for top managers; executives who sign non-competition agreements will have lower human capital and hence will receive lower compensation. Both these theories yield:

**Prediction 3A.** *The compensation of top managers is lower in high-enforceability jurisdictions.*

A second possibility is that greater enforceability may simply enrich the contracting environ-

ment. Firms may be willing to pay higher wages in exchange for the security of knowing that managers cannot depart to work for a rival and potentially reveal firm secrets. Managers who sign non-competition agreements may also remain with firms longer (as suggested by Prediction 2) and thereby develop more valuable firm-specific human capital.

**Prediction 3B.** *The compensation of top managers is higher in high-enforceability jurisdictions.*

Rajan and Zingales (2001) present arguments showing that, depending on the nature of the ability of managers to expropriate firm assets, either Prediction 3A or 3B may hold.

Not only the level but also the form of compensation may vary with the enforceability of non-competition agreements. There are two direct effects. First, in low-enforcement jurisdictions, managers' outside opportunities will be quite sensitive to their performance: outside firms will be interested in hiring managers with good records. This will be much less true in high-enforcement jurisdictions, since the ability of managers to leave their current firms is quite restricted in such areas. As a consequence, the compensation of managers in low-enforcement jurisdictions must vary with their performance, while the compensation of executives in high-enforcement jurisdictions will likely contain a higher fixed (salary) component. (Fixed compensation is preferable to managers from a risk perspective and hence should be used more heavily when outside-opportunity considerations are less salient.) Second, Gibbons and Murphy (1992b) show that when implicit incentives are weak (for example, in low-enforcement areas), then explicit incentives (such as non-salary compensation) must be strong. In a related idea, Hagerty, Ofer, and Siegel (1991) and Jackson and Lazear (1991) point out that deferred compensation encourages managers to remain with their current firm since the payoff of the compensation depends on future firm performance. This is likely to be especially important in low-enforcement jurisdictions. The selection/indirect effect also suggests that the salary component will be smaller in low-enforcement states, because managers in these areas will have lower human capital and hence high-powered incentives will induce less performance, while still generating large risk exposure for managers.

**Prediction 4.** *Executive compensation will contain a larger salary component in high-enforceability jurisdictions.*

The enforcement environment can also affect the optimal use of inside (executive) directors on the firm's board. The presence of inside directors reflects the outcome of bargaining between the CEO and the board (Hermalin and Weisbach, 1988). One of the potential costs associated with bringing a manager onto his firm's board is that he may manipulate and attenuate board monitoring. A direct effect of a non-competition agreement is that it binds together the success of

the manager and his firm, and thereby reduces the agency problems that may require monitoring. This argument yields the following implication.

**Prediction 5.** *Executives are likelier to be members of the board in high-enforceability jurisdictions.*

The above discussion has highlighted some of the advantages to the firm that arise from tough enforcement of non-competition agreements. The selection/indirect effect, however, emphasizes the potential cost that managers in firms that require enforceable non-competition contracts may have low human capital.

**Prediction 6.** *Executives in firms in high-enforceability jurisdictions have less human capital.*

Predictions 1,2,5 and 6 thus highlight the advantages and disadvantages of non-competition agreements: they bind executives to the firm, thereby allowing the firm to trust its managers with greater discretion, but they may discourage good executives from joining the firm. In light of these arguments, we will now analyze the effect of covenants not to compete on the firm's investment strategy. We first consider research and development spending. The direct effect is that non-competition agreements protect the firm's intangible capital that is created by research and development investment from expropriation by employees. This suggests that higher enforcement should be associated with greater research and development spending.

**Prediction 7A.** *Firms in high-enforceability jurisdictions will engage in more research and development spending.*

Conversely, research and development is most profitably undertaken by high-quality managers. The selection/indirect effect suggests that non-competition agreements reduce managerial human capital, so firms in high-enforceability jurisdictions may do better to reduce their research and development investments.

**Prediction 7B.** *Firms in high-enforceability jurisdictions will engage in less research and development spending.*

A related question is the firm's choice of physical capital-labor ratio. Workers who have signed non-competition agreements are likely to remain with firms for longer (Prediction 2), and hence firms in high-enforcement jurisdictions should have work teams that know each other well and are efficient in production. It may be optimal for such efficient teams to be supplied with a large amount of physical capital.

**Prediction 8A.** *Firms in high-enforceability jurisdictions will have high physical capital-labor ratios.*

The selection/indirect effect suggests, however, that firms in low-enforceability jurisdictions will

attract a more skilled labor force (Prediction 6) that is adept at complex, capital-intensive tasks. If human capital and labor effort are strong substitutes, firms in low-enforcement jurisdictions will exhibit high capital-labor ratios. It may also be suggested that in low-enforcement areas firms must keep the number of employees small in order to minimize worker expropriation (Rajan and Zingales, 2001).

**Prediction 8B.** *Firms in high-enforceability jurisdictions will have low physical capital-labor ratios.*

The enforceability of non-competition arrangements may also have an impact on merger and acquisition activity. Firms that have enforceable non-competition agreements with their employees may make attractive acquisition targets, since an acquirer need not be concerned about widespread employee desertion after a merger. States differ, however, in the ease with which they permit target firm to assign employee non-competition contracts to acquirers. Acquirers will have the greatest protection in jurisdictions in which non-competition agreements are both enforceable and assignable to purchasers.

**Prediction 9A.** *Firms in high-enforceability and high-assignability jurisdictions will be acquired more frequently.*

Conversely, it may be argued that a large source of value in firms in high-enforcement areas is the particular culture and network of connections that is created by the managerial stability sustained by non-competition agreements. Acquirers often want to export their own organizational culture to newly purchased subsidiaries, and may not value the firm-specific organizational assets that are fashioned by a web of executive non-competition agreements. In fact, the acquirer may intend to replace managers at the target firm with its own executives. In this case, acquirers may be unwilling to pay a price for the target that reflects the value to the target, as a stand-alone entity, of its non-competition arrangements.

**Prediction 9B.** *Firms in high-enforceability jurisdictions will be acquired less frequently.*

The status of non-competition enforceability in its home jurisdiction may also affect a firm's interest in acquiring other companies. Firms in high-enforcement areas are likely, due to their stable management teams, to have a uniform corporate culture that can be exported to acquired firms. This may encourage acquisitions.

**Prediction 10A.** *Firms in high-enforceability jurisdictions will engage in more frequent and larger acquisitions.*

One may also argue that long-established management teams are unlikely to welcome the change and disruption that accompany an acquisition. A successful acquisition is also likely to require a

high level of skill that may not be present in firms in high-enforcement areas.

**Prediction 10B.** *Firms in high-enforceability jurisdictions will engage in less frequent and smaller acquisitions.*

# 3 Data

To test the predictions detailed in Section 2, we require data on executive jobs transfers, executive compensation, executive board participation, firm research and development spending, firm capital investment and acquisitions. Our data source for details on executives is Standard and Poor's Execucomp database. Execucomp includes compensation and board participation data on the five most highly paid executives for 2,610 large publicly traded U.S. firms. (Execucomp also follows executives who were amongst the top five and later fell below, so some firms have reported data for more than five executives in a given year.) Data are available for 1992-2004. Executive-specific identifiers allow us to track managers who move from one firm to another.

We supplement the Execucomp data with firm-level data from Compustat on research and development investment, capital expenditures and acquisition activities. We proxy for firm age by considering the firm's first appearance in Compustat. We use the location of firms' headquarters in implementing our empirical strategy (as discussed below in Section 4), but Execucomp and Compustat report only the current and not the historical headquarters location. We match the Execucomp data set to the Compact Disclosure database to determine historical headquarters locations. One hundred and thirty (5.0%) of the firms shifted locations during the sample period. We also make use of the Investor Responsibility Research Center Governance data to provide information on firm's corporate governance practices. Data on state unemployment rates and per capita personal income are provided by the Bureau of Economic Analysis. Summary statistics on these data are reported in Table 1.

# 4 Empirical Strategy

The central empirical challenge is to generate a measure of the enforceability of non-competition agreements across the states of the U.S. To this end, we construct an index of non-competition enforcement.

## 4.1 Non-competition Enforcement Index

Malsberger (2004) is the central resource describing non-competition law in the fifty U.S. states and the District of Columbia. We consider twelve questions analyzed by Malsberger for each

jurisdiction and assign a score of one to each jurisdiction for each question if the jurisdiction's enforcement of that dimension of non-competition law exceeds a given threshold. Possible scores therefore range from zero to twelve. A complete list of questions, thresholds and state scores is given in the Appendix. Summary statistics on the scores are reported in Table 1. We provide here a broad overview of the basic issues in non-competition law and provide some examples illustrating differences amongst the laws of varying states.

The first question is whether non-competition agreements are generally enforceable in any respect. The focus of our study is on the effects of non-competition agreements signed by executives who are employed by large firms, so we ignore laws that apply to non-competition arrangements concluded in the context of the sale of a business. (Those are permitted to some degree in every jurisdiction.) Almost all states allow some form of covenant not to compete between employers and employees, though California and North Dakota do not. For this question, all jurisdictions save these two are awarded a score of one.

Even though most states allow some form of non-competition agreement, there is great variation in the types of contracts permitted. For example, the nature of what a firm can claim as a legitimate protectable interest depends on the jurisdiction. In some states (e.g., New Hampshire), a firm can restrict an employee from future independent dealings with customers with whom he had direct contact, but cannot prevent him from seeking business from other customers once he leaves the firm. In other states (e.g. Georgia), a non-competition agreement can ban an employee from trading with any current clients of the firm, even if the employee has no contact with the client.

States also have different requirements for the compensation that an employee must receive in order for the non-competition contract to be valid. In some states (e.g. Wisconsin), at-will employment is sufficient consideration. In other states (e.g. Texas) some ancillary compensation must be granted to the employee at the time the agreement is made.

The geographical and time restrictions that are considered reasonable can be quite different in different jurisdictions. In some states (e.g. Missouri), an agreement will be enforced in a region even if the firm has no current business in that area. In other states (e.g. Virginia) the non-competition agreement must usually be restricted to the firm's current markets. Some states (e.g. Pennsylvania) will routinely enforce 3 year covenants, while others (e.g. Florida) presume that covenants more than 2 years in length contracted upon outside a sale-of-business context are unreasonable. State laws also vary in determining whether employees are released from their contracts if they are fired.

In our empirical work we consider the effects of both changes in the regulations of given states over time and cross-sectional differences in state laws. We note here that the enforcement of

non-competition agreements is governed by employment law, not corporate law, so the relevant jurisdiction is typically the one in which the employee works (Malsberger, 2004 and Pentelovitch, 2003). Our study analyzes top executives at large firms, who will typically work at headquarters, so it is the headquarters location, not the state of incorporation, that we consider.[2]

## 4.2 Time Series Changes in State Laws

While laws governing the enforcement of non-competition agreements are largely static, three states experienced significant shifts in the treatment of covenants not to compete during our sample period of 1992-2004.

### 4.2.1 Texas

In June 1994 in *Light v. Centel Cellular Co.* the Texas Supreme Court developed a new set of requirements that were needed for enforceability of non-competition agreements. The ruling, in this important case that redefined the legal standards in the state, stated that a covenant not to compete must be "ancillary to or part of an otherwise enforceable agreement" between the employer and the employee. In other words, the employer must offer the employee some specific consideration in exchange for the non-competition agreement, and continued at-will employment does not constitute acceptable consideration. Moreover, it must be that the covenant not to compete is designed to enforce the promises made by the employee as part of the agreement. This latter condition was a new requirement added by the court (Malsberger, 2004), and it serves to make it substantially more difficult to enforce non-competition agreements in Texas.[3] The non-competition enforcement index score for Texas is five before 1994 and three after the decision. The court also ruled that its interpretation of the law applied both retroactively to all agreements previously signed in Texas and prospectively to any future agreements.

### 4.2.2 Louisiana

The Louisiana Supreme Court radically changed the enforcement of non-competition agreements in the state with its June 2001 ruling in *SWAT 24 Shreveport Bossier, Inc. v. Bond*. The court ruled that Louisiana's statues on covenants not to compete only permitted contracts that restricted employees from setting up their own businesses in competition with a previous employer; employees

[2] As discussed below in Section 4.3 there was some ambiguity during the sample period about which state's law governed cases in which an employee left a firm with a headquarters in one state and moved to a competitor with a headquarters in a different state. This adds noise to our estimation (though no systematic bias), but the state of incorporation, in any case, is rarely of significant importance.

[3] Schueler and Solomon (2001) and Fowler (2002) discuss the effects of the *Light v. Centel* decision on Texas non-competition law.

could not be prohibited from joining a competing firm in which they held no equity interest. From the perspective of a manager of a large corporation, the *SWAT* ruling made non-competition agreements significantly less enforceable in Louisiana; the relevant labor market opportunities for such a manager would typically lie with other large competitors. The ruling applied to all previous covenants not to compete. In 2003, the state legislature altered the law to permit non-competition agreements barring employees from joining other firms in which they have no ownership interest. The non-competition enforcement index score for Louisiana is zero during the period 2002-2003 and four otherwise.

#### 4.2.3 Florida

The change in law in Florida, by contrast with the above two cases, arose from the actions of the state legislature, not a court. In late May, 1996 the state legislature repealed the previous law governing covenants not to compete and replaced it with a new law. In addition to adding clarity to the rules governing non-competition agreements, the new law strengthened employers' positions in three significant ways. First, the 1996 statue prohibits courts from considering the individual hardship that the non-competition agreement will cause the former employee. This represents a dramatic change from the previous law that balanced the interests of the employer and the former employee (Malsberger, 2004). Second, the 1996 law requires courts to modify geographic or time restrictions that are overbroad, rather than simply declaring the covenant unenforceable. Prior law allowed for such modifications, but did not require them. This change made it easier for employers to write highly restrictive covenants without fear of their being overturned in court. Third, under the new law there is a presumption of injury to the firm when a non-competition agreement is violated. This enhances the firm's powers to get an injunction preventing its former employee from working for another company (Gallo and Adler, undated). The non-competition enforcement index score for Florida is seven before the 1996 law and nine afterwards. The new law was specifically stated to apply only to contracts signed after July, 1996 - previous contracts are governed by the law in effect at the time they were signed. The purely prospective change in Florida law differs from the prospective and retroactive changes in the law in Texas and Louisiana. When considering the impact of these various legal changes on executive compensation, we will take into account in our analysis below the fact that the Texas and Louisiana rulings abrogated some past agreements, while the Florida change in statue law served only to expand the scope for future agreements, and had no effect on previous contracts.

We make use of these changes in the legal environment to generate a variable we label *Increased*

*Enforceability.* We assume that the legal changes detailed above affected compensation and firm investment starting in the year following their occurrence. The variable therefore takes the value of one for firms in Florida in 1997-2004, takes the value of negative one for firms in Texas in 1995-2004 and for firms Louisiana in 2002-2003 and is set equal to zero otherwise. To avoid any possible selection effects, we only consider the effects of the legal shifts on the large majority (95%) of firms that did not change the state of their headquarters locations throughout the entire sample period. We use a simple three-level ($\{-1, 0, +1\}$) measure of the changes for the results reported in the paper, but a measure based on differences in the non-enforcement index scores yields similar findings. The changes described above can be regarded as exogenous shocks to the legal environment. The Texas and Louisiana court decisions clearly introduced unanticipated changes into the law. The Florida 1996 law differed from the previous 1990 law in specific ways that could not have been clearly foreseen. All the changes had substantial effects on the enforceability of non-competition agreements. Since these legal changes affected firms in different states at different times, we are able to incorporate firm fixed effects into our analysis. In our analysis of the level of executive compensation, for example, we consider the effect of the change in the legal environment on the compensation paid by a given firm. Firms in the states that did not experience any legal shifts serve as controls for any times series variation in compensation levels that occurs nationalwide.

Our econometric model considers the effect of *Increased Enforceability* on the following firm characteristics: executive transfers into and out of the firm, executive job tenures, level and composition of executive compensation, executive participation on the board, firm performance, firm research and development spending, firm investment and several other variables linked to the predictions in Section 2. The equation estimated is

$$firm\,characteristic_i = F(Increased\,Enforceability, controls_i) + \epsilon_i, \tag{1}$$

where $controls_i$ is a vector of controls for firm $i$ including a firm fixed effect and $\epsilon_i$ is an error term. We most often estimate linear models, though other functional forms are considered for binary dependent variables. Given the small number of firm headquarters moves discussed in Section 3, state fixed effects are essentially subsumed by the firm fixed effects and are therefore omitted. We calculate robust standard errors clustered at the state-year level.

In some tests we analyze the characteristics of a given executive (such as growth in compensation) and estimate

$$executive\,characteristic_j = F(Increased\,Enforceability, controls_j) + \epsilon_j, \quad (2)$$

where $controls_j$ is a vector of controls for executive $j$ including an executive fixed effect and $\epsilon_j$ is an error term. Under this specification we calculate robust standard errors clustered at the firm level.

### 4.3 Cross-sectional Variation in State Laws

To supplement the time series evidence, we also consider cross-sectional variation in legal environments across states. One approach would be to simply use the non-enforcement index scores without any state fixed effects, but that analysis would presume that the differences in, for example, compensation levels between firms in different states are entirely driven by non-competition law. That is clearly not true; states vary in numerous ways, not all of which can be specified, so state fixed effects are required. Just using the non-enforcement index scores along with state fixed effects would essentially replicate the time series approach described above. Instead, we argue that the level of the non-enforcement index will have a different effect on firms that face different competitive environments. We focus on the extent of in-state competition, since it is in-state competition that is most affected by a covenant not to compete. This is true for two reasons:

1. As discussed above, enforceable non-competition agreements typically have a restricted geographical scope, and this scope is often a state or a part of a state, for example, a county, a city or a 10 or 50 mile radius around the place of business (Malsberger, 2004).

2. For most of the entire sample period there was considerable confusion about which jurisdiction provided the governing law in cases of cross-state executive job transfers. Managers from states with tough non-competition enforceability on several occasions accepted offers from California firms and asked California courts to void their non-competition agreements. California courts typically agreed, arguing that the contracts were not enforceable in California. Two cases decided in 2002 ended this practice; it was determined that the state law of the place of previous employment was the relevant law (Pentelovitch, 2003). It is clear that it was easiest and least costly to enforce non-competition agreements when the executive remained in-state.

We thus propose the interaction between the non-competition enforcement index and the level of in-state competition as a measure of effect of enforceability. For firms with considerable in-state competition, an increase in the non-competition index will substantially reduce the probability that

an executive will leave the firm and join a competitor. For firms with little in-state competition, an increase in enforceability will have little impact, for the two reasons given above. We also incorporate state fixed effects in the analysis to control for state-wide variables other than non-compete enforceability. We are thus comparing the relative effects of non-competition enforcement

We cannot use firm fixed effects in this specification because most states (and hence most firms) experience no time series variation in the non-competition index. Thus, relative to the time series variable, the cross-sectional measure sacrifices the precision of firm fixed effects, but makes use of variation in non-compete enforceability across more firms and more states.

The econometric model we use to estimate the cross-sectional effects of differences in state laws is

$$firm\,characteristic_k = F((Enforcement\,score) * (in-state\,competition), controls_k) + \epsilon_k, \quad (3)$$

where *(in-state competition)* is the fraction of total industry sales (excluding those of the firm itself) generated by in-state competitors,[4] $controls_k$ is a vector of controls for firm $k$ including state and industry fixed effects and $\epsilon_k$ is an error term. We calculate robust standard errors clustered at the firm level. The level of the non-enforcement index is almost perfectly collinear with the state fixed effects and is therefore not included in (3). (It is not completely collinear only because of the time series variation that we analyze in the method described in Section 4.2.) We interpret the coefficient on *Enforcement*(In-state competition)* as a measure of the effects of non-competition enforceability since it reflects the impact of enforceability on the firms within a given state for which it should matter most.

#### 4.3.1 Is the Level of Non-Competition Enforceability Exogenous?

It is clear that a firm can choose the location of its corporate headquarters. Our study of the effects of cross-sectional variation in non-competition enforcement is therefore subject to the following objection: perhaps firms that plan to have a corporate policy requiring non-competition contracts locate in jurisdictions that will enforce these contracts. Any results we may find linking non-competition enforceability to, for example, executive mobility may thus be driven simply by sorting in the types of firms that choose to locate in different areas.

We do not view this objection as particularly forceful for two reasons. First, the business location literature has emphasized the effects of natural resources, presence of skilled labor (Ellison

[4] Our results are robust to using a measure of competition based on the number of competitors.

and Glaeser, 1999), unionization levels, state taxes (Bartik, 1985), founder's home location, energy costs (Carlton, 1983) and environmental regulation on the siting decisions of firms. The local non-competition enforcement regime has not been proposed as a first-order determinant of firm location, nor do we think it a likely candidate. Moreover, the results in Section 5 show that non-competition enforceability has no effect on firm value or performance, so it is not plausible that it is driving firm location decisions.

Second, the sorting argument presumes that differences in non-competition law do have an effect, or else there would be no reason for firms to sort into states based on varying enforceability. That is, the sorting argument can suggest at most that any effects we find are a combination of treatment effects from differences in the laws and sorting effects. Our specification, however, is designed to negate sorting effects. Suppose, for example, that firms that have no interest in non-competition contracts locate in California (a low-enforcement state), while firms that do want non-competition agreements locate in Massachusetts (a high-enforcement state). We make use of state fixed effects in all our cross-sectional regressions that completely net out such effects.

The type of sorting for which we cannot control is sorting that takes place across the enforceability and within-state competition measures simultaneously. If there are firms that are particularly averse to executive mobility, they may locate in states that have both high enforceability and little in-state competition, since these features should reduce managerial transfers. Prediction 1 is that high enforceability reduces executive transfers. Our interaction variable can essentially be thought of as

$$(Enforcement - mean(Enforcement)) * (in-state\,competition - mean(in-state\,competition)).$$

Stability-seeking firms locating in high enforcement, low in-state competition states will generate negative values for this interaction but will presumably have low executive mobility. This would bias the results away from a finding consistent with Prediction 1 (since our empirical strategy regards a positive value for the interaction as indicative of high enforceability effects), and similar arguments apply to the other predictions. In other words, any sorting effects are likely to be quite small, and, in any case, will disfavor finding evidence in support of the predictions.

The time series tests offer clear exogenous shocks to individual firms, but the cross-sectional tests provide additional evidence using variation across all states.

# 5 Results

## 5.1 Are non-competition agreements effective?

We apply our empirical strategy to test the predictions listed in Section 2. Prediction 1 relates the frequency of within-industry executive job transfers to the non-competition enforceability environment faced by the firm. To test the prediction, we first analyze the set of job transfers. Execucomp provides information on the top executives, and unique executive identifiers allow us to establish that executives have left one firm and joined another. We are unable to observe transfers to or from firms not included in Execucomp, and we are only able to observe transfers in which the executive is in the top five at both the firm he departs and the firm he joins. Despite these limitations, we observe 1,384 such transfers over the sample period. We define industries at the five-digit NAICS code level. There are 433 industries represented in our data. We observe 344 within-industry transfers. For each firm we calculate the total number of transfers both into and out of the firm.

The first column of Table 2 reports results for the regression of the number of within-industry transfers on *Increased Enforceability*, firm fixed effects, year fixed effects and a set of controls. The controls include for each firm-year the firm's one- and three-year total returns, the log of firm equity book and market values, the log of firm sales and the log of firm assets. We also include as additional controls the log of the state unemployment rate and the log of the state personal per capita income in the year. This regression explores the effects of time series changes in state laws (specification (1)). The estimation is via ordinary least squares (OLS).[5] We find that increased enforceability of non-competition agreements significantly (t=-2.44) reduces within-industry transfers, as suggested by Prediction 1. (Robust t-statistics corrected for clustering at the state-year level are reported.) A shift to a tougher enforcement regime reduces within-industry transfers by 0.0264, relative to a mean transfer rate of 0.0314 in the sample with available firm data. (The transfer rate in the full sample is 0.0231.) While the total transfer rate is fairly low (perhaps due to general stability in the employment situations of top managers and the limited observability of transfers discussed above), the 2.64 percentage point drop (84% of the mean) due to a change in state law is both economically and statistically significant.

We next consider the impact of cross-sectional variation in the non-competition enforcement index (specification (3)). The second column of Table 2 reports results for the regression of the number of within-industry transfers on *Enforcement*(In-state competition)*, *(In-state competition)*,

[5] Although the dependent variable is discrete, we do not employ an alternate estimation method (e.g., ordered logit) because the large number of fixed effects raises the nuisance parameter consistency problem for non-linear regressions. We do make use of fixed effects (conditional) logit estimation for some binary-dependent-variable regressions, and throughout the paper we specify whenever a non-OLS estimation technique is used.

state, industry and year fixed effects, and the previous set of firm controls, along with log of firm age. ( *Log( Firm Age)* and *(In-state competition)* were omitted in the previous regression due to the presence of firm fixed effects - these variables exhibit very little time series variation for a given firm, especially in the presence of year fixed effects.) Firms that produce more than 90% of their industry's sales (1.9% of the sample) are excluded from this regression (and the analogous regressions throughout the paper) because they face so little within-industry competition that variation in the *(in-state competition)* measure will not reflect substantive differences in the same-industry employment opportunities of their executives. We find that the coefficient on *Enforcement*(In-state competition)* is negative and significant (t=-3.08), providing further support for Prediction 1. (Robust t-statistics corrected for clustering at the firm level are reported.) Tough enforcement of non-competition agreements serves to particularly reduce within-industry transfers for firms with substantial in-state competition, as hypothesized. A one-standard deviation in the interaction between the enforcement index and the level of in-state competition reduces within-industry transfers by 0.0136 (42% of the mean).

The third and fourth columns display results from regressing all transfers (within and between industries) on *Increased Enforceability* and *Enforcement*(In-state competition)*, respectively. The effects are economically smaller than for the within-industry regressions (a shift to increased enforcement reduces all transfers by 46% of the mean, and a one-standard-deviation increase in the interaction between the enforcement index and the level of in-state competition reduces all transfers by 15% of the mean), but remain statistically significant (t=-2.10 and t=-2.04, respectively). As shown in the fifth and sixth columns of Table 2, however, *Increased Enforceability* and *Enforcement*(In-state competition)* have statistically insignificant effects on the level of between-industry effects.

The results displayed in Table 2 establish that our measures of *Increased Enforceability* and *Enforcement* capture differences in the legal environment that have an important effect on within-industry executive mobility. Moreover, the results also suggest that our variables are not simply correlated with some endogenous measure of executives' general propensity to move since we show that *Increased Enforceability* and *Enforcement* reduce within-industry, but not between-industry, moves.

Prediction 2 suggests that executives should have longer job tenures in tough enforcement jurisdictions. Execucomp provides data on the start and departure dates for a subset of managers. We use this data to generate a record of completed job tenure lengths for these executives. For each firm-year we then calculate the average completed job tenure across all executives for whom data is

available. (This measure is highly correlated within firms, which we account for through clustering of the standard errors at the firm level.) In the first column of Table 3 we display the results from regressing job tenure length on *Increased Enforceability*, firm fixed effects and the previous set of controls. In support of Prediction 2, the coefficient on *Increased Enforceability* is significant (t=3.14). A switch to a higher enforceability regime increases completed job tenure lengths by 1.15 years, which is 9.0% of the mean. In the second column, we report the results from regressing job tenures on *Enforcement*(In-state competition)* and the usual controls. The coefficient is positive and significant (t=2.16). A one-standard deviation increase in *Enforcement*(In-state competition)* increases job tenures by 10.6% of the mean.

We also analyze the effect of non-competition agreement enforceability on the rate of departures from the firm. We consider than an executive has departed a firm if Execucomp records his leaving date, or if the executive appears as an employee of a different firm. In the third column of Table 3 we report results from regressing the number of departures in each firm-year on *Increased Enforceability*, firm fixed effects and the standard controls. The coefficient is statistically significant, and we find that a shift to a higher enforcement regime reduces the rate of departures by 29% of the mean. In the fourth column we show results from regressing the number of departures on *Enforcement*(In-state competition)* and the usual controls. The coefficient is statistically insignificant. We note here that the *Increased Enforceability* variable measures a change, while the *Enforcement* variable is a level. The departure measure is a change, and the job tenure variable is a level. We would thus expect that *Increased Enforceability* would directly change the rate of departures, but that it would change job tenure lengths only over time. This may explain the much larger economic impact of *Increased Enforceability* on the departure rate relative to job tenure length. The *Enforcement* variable, by contrast, should be expected to have an effect on observed job tenures (since both are levels), but its effect on the rate of departures might be more difficult to discern. This may explain the significance of *Enforcement*(In-state competition)* in the job tenure regression and its insignificance in the departures regression. Overall the results are supportive of Prediction 2. Non-competition arrangements clearly reduce executive mobility and increase stability.

### 5.2 Executive compensation

We now turn to the issue of executive compensation. Predictions 3A and 3B provide competing hypotheses about the effect of non-competition agreement enforcement on the level of compensation. We first consider the impact of a shift in legal enforcement. As discussed in Section 4.2, the change in Florida's laws differed from the changes in Texas and Louisiana. In Florida, the law change

in 1996 in favor of tougher enforcement was purely prospective and had no effect on pre-existing contracts. One theory we analyzed in the discussion of Prediction 3A suggests that executives accept a higher salary initially in exchange for agreeing to a non-competition agreement that will constrain their future pay. If this is correct, Florida executives may have negotiated higher salaries in 1997 (the year after the law change) in exchange for signing new, more restrictive covenants not to compete. In future years these covenants may have limited growth in compensation. By contrast, the abrogating of all historical contracts that occurred in Texas and Louisiana would be expected to raise salaries immediately. We thus define a new variable *Increased Enforceability'* that is equal to *Increased Enforceability*, except that it is equal to zero for Florida firms in 1997 and takes the value of one for Florida firms from 1998-2004.

The *Increased Enforceability'* variable measures a change, so we study its effect on the change in compensation. We define compensation to be the sum of the following Execucomp data items: Salary, Bonus, Other Annual, Total Value of Restricted Stock Granted, Total Value of Stock Options Granted (using Black-Scholes), Long- Term Incentive Payouts, and All Other Total. We analyze compensation at the individual executive level, and we truncate the growth in compensation at -90% and +900%. (This eliminates 1.5% of the data.) In the first column of Table 4 we report results from regressing compensation growth on *Increased Enforceability'*, executive fixed effects and the previous set of firm controls. We find a negative and significant (t=-2.42) coefficient on *Increased Enforceability'* (robust t-statistics corrected for clustering at the level of the firm are reported). For a given executive, a shift to a tougher enforcement regime reduces compensation growth by 12.8%, which is 39.1% of the mean growth rate. This economically significant decrease in the growth rate of compensation, using individual executive fixed effects, is strong evidence in support of Prediction 3A, indicating that increased enforcement of non-competition agreements reduces compensation growth. The individual fixed effect controls indicate that this finding is evidence in favor of the direct effect of non-competition enforceability on a given executive; there is no selection effect at work in this regression. Regressing log growth rates on *Increased Enforceability'* yields similar (unreported) results in the untruncated sample.

We also consider the effect of the level of *Enforcement* on the level of compensation. Since the *Enforcement*(In-state competition)* variable does not vary within a firm, we are unable to make use of firm (or executive) fixed effects. We regress the log of average compensation across the top five executives for each firm-year on the level variable *Enforcement*(In-state competition)*, *(In-state competition)*, state, industry and year fixed effects and the usual firm controls. As displayed in the second column of Table 4, we find that *Enforcement*(In-state competition)* significantly (t=-

3.64) reduces compensation (robust t-statistics corrected for clustering at the level of the firm are reported). A one-standard deviation increase in *Enforcement*(In-state competition)* reduces the log of compensation by 1.2% of the mean. (By comparison, a one-standard deviation increase in three-year total return increases the log of compensation by 0.7% of the mean.) This result provides further cross-sectional support for Prediction 3A in addition to the previous time-series evidence: top executive compensation is lower in high-enforcement jurisdictions. The level results may be driven by either the direct or the selection/indirect effects of non-competition agreements.

One well-known theory argues that executive compensation is determined by the firm's desire to create tournament-like incentives for its executives (Lazear and Rosen, 1981). It is plausible that non-competition agreements may reduce the firm's interest in organizing a compensation tournament, since these agreements reduce managers' outside opportunities and make it less expensive for the firm to promote insiders (Bognanno, 2001).[6] In that case, we would expect to see a smaller gap between the compensation of the CEO and that of his subordinates. To test this theory, we first define the compensation ratio to be the log of the ratio of the CEO's compensation to that of the average pay of the executives with the second through fifth highest compensations. In the third column of Table 4 we show results from regressing the compensation ratio on *Increased Enforceability'* and the standard controls. The coefficient on *Increased Enforceability'* is negative and significant at 10% level (t=-1.73). In the fourth column we report the results from regressing the compensation ratio on *Enforcement*(In-state competition)* and the previous controls. The coefficient on *Enforcement*(In-state competition)* is also negative and significant at the 10% level (t=-1.66). These two results provide somewhat weak evidence in favor of the hypothesis that tournament incentives are used less in firms that utilize non-competition agreements.

We have shown that compensation is lower in high-enforcement jurisdictions, and we now consider whether the form of compensation differs with non-competition enforceability. Prediction 4 indicates that salary should constitute a larger proportion of overall compensation in areas in which covenants not to compete are more strictly enforced. To test the time series implication of the prediction, we regress the difference between salary growth and overall compensation growth on *Increased Enforceability'*, executive fixed effects and the standard set of firm controls. As exhibited in the first column of Table 5, we find a positive and significant (t=2.01) coefficient on *Increased Enforceability'*: a shift to greater enforcement of non-competition agreements leads to greater growth in salary relative to other forms of compensation. This fixed effect finding provides evidence for a direct effect of non-competition enforceability. To test the cross-sectional implication

[6] Aggarwal and Samwick (2003) study the role of incentives within firms.

of Prediction 4, we regress the log of the ratio of salary to overall compensation on *Enforcement*(In-state competition)*, *(In-state competition)* and the standard controls. Column two of Table 5 shows that we find a positive and significant (t=3.99) coefficient on *Enforcement*(In-state competition)*. The cross-sectional evidence may be driven by either the direct or selection/indirect effects of non-competition contracts. The time-series and cross-sectional results provide consistent evidence in favor of Prediction 4.

To further study the question of compensation composition, we consider whether firms provide any options to their executives. For each executive-year, we regress an indicator variable for whether the executive receive any option compensation on *Increased Enforceability'*, executive fixed effects and the standard controls. Since the dependent variable is binary, the estimation is via fixed effects (conditional) logit. As displayed in column three of Table 5, we find no significant effect of *Increased Enforceability'* on options-granting. We also regress the number of top five executives receiving options on *Enforcement*(In-state competition)*, *(In-state competition)* and the previous controls. In column four of Table 5 we report that the coefficient on *Enforcement*(In-state competition)* is insignificant. Thus, although the total value of non-salary compensation (as a proportion of total compensation) is increasing in non-competition enforceability, we do not find evidence that enforceability affects whether or not firms use any options at all. In unreported results, we also find no evidence that the use of restricted stock in executive compensation varies with non-competition enforcement.

The use of reload options in executive compensation received particular attention during our sample period (Dybvig and Loewenstein, 2003). An executive who pays the exercise price for these options by selling currently held shares receives both shares and new reload options with a higher strike price but the same maturity as the original option. Dybvig and Loewenstein (2003) show that, relative to standard options, reload options are more valuable and have higher deltas, but the two options types are otherwise somewhat similar. Reload options are designed to ensure that an executive's exposure to the stock price is not diminished by his selling previously owned shares in order to pay the exercise price on his options.[7] As the discussion preceding Prediction 4 made clear, compensating managers in a manner directly tied to stock price performance should be more important in low-enforcement jurisdictions, so we might expect a greater use of reload options in such areas.

We regress a binary variable for whether an executive received any reload options in a given year

[7]Executives commonly liquidate most of the shares they receive upon exercising options (Ofek and Yermack, 2000). Chen (2004) studies the role of options in encouraging executive retention.

on *Increased Enforceability'*, executive fixed effects and the usual controls. The estimation is by fixed effects logit. As exhibited in column five of Table 5, the coefficient on *Increased Enforceability'* is negative and significant (t=-3.78). This is evidence for a direct effect of enforceability on a given executive. We also regress the number of top five executives who received any reload options in a given firm-year on *Enforcement*(In-state competition)*, *(In-state competition)* and the standard controls. The coefficient on *Enforcement*(In-state competition)* is negative and significant (t=-2.41). The cross-sectional result may be driven by either the direct or the indirect/selection effects of non-competition enforceability. Both findings are consistent with the idea explored above that maintaining a manager's exposure to his firm's stock price through the use of reload options is less important in high-enforcement jurisdictions.[8]

### 5.3 Board membership and corporate governance

In addition to their effects on the level and form of executive compensation, non-competition agreements may reduce agency costs by tying managers to firms. Prediction 5 suggests that firms in high-enforcement jurisdictions should be more likely to appoint their executives to their boards. To test the prediction, we regress the number of top five executives who serve on the board in each firm year on *Increased Enforceability*, firm fixed effects and the usual controls. As shown in column one of Table 6, we find a positive and significant (t=3.38) coefficient on *Increased Enforceability*. A shift to greater enforceability led to 0.12 more top five executives on the board, which is an increase of 6.5% relative to the mean. To analyze the cross-sectional implication of Prediction 5, we regress the number of top five executives on the board on *Enforcement*(In-state competition)*, *(In-state competition)*, state, industry and year fixed effects and the standard firm controls. We find a positive and significant (t=2.08) coefficient on *Enforcement*(In-state competition)*, as we report in the second column of Table 6. A one-standard deviation increase in *Enforcement*(In-state competition)* leads to an increase of 4.4% (relative to the mean) in the number of executives on the board. The time-series and cross-sectional evidence together provide strong support for Prediction 5: firms in high-enforcement jurisdictions are more likely to appoint executives to their boards. Prediction 5 is generated by the direct effect of enforceability on a given manager, so these findings help to demonstrate the empirical relevance of the direct effect.

While there may be substantial information benefits from having managers serve on boards,

[8] Dybvig and Loewenstein's (2003) result that reload options are worth more than standard options, combined with our finding that reload options are used more frequently in low-enforcement jurisdictions strengthens our previous result on compensation composition. We showed that non-salary compensation is larger in low-enforcement jurisdictions, using the Execucomp valuation measure that does not distinguish between reload and non-reload options. If reload options were valued properly, we would presumably find an even stronger result linking low-enforcement to high non-salary compensation.

there may also be some agency costs arising from essentially allowing executives to have a hand in evaluating themselves (Hermalin and Weisbach, 1998). To study this question, we consider whether the quality of firm governance is related to the enforceability of non-competition agreements. We proxy for the state of firm governance with the Gompers, Ishii, Metrick (2003) Governance Index. We regress the Governance Index on *Increased Enforceability*, firm fixed effects and the usual controls. The coefficient on *Increased Enforceability* is insignificant, as shown in the third column of Table 6. The high $R^2$ reflects the fact that there is relatively little within-firm variability in the Governance Index. We also regress the Governance Index on *Enforcement*(In-state competition)*, *(In-state competition)*, state, industry and year fixed effects and the standard firm controls, and find an insignificant coefficient on *Enforcement*(In-state competition)* (fourth column of Table 6). We repeat these regressions for the Gompers, Ishii, Metrick (2003) corporate governance sub-index of director/officer protection (labelled the Protection index), since this category directly captures privileges accorded to top managers and board members. As shown in columns five and six of Table 6 both the time series and cross-sectional tests show no statistically significant relationship between non-competition enforceability and the Protection index. We conclude that managers in high-enforcement jurisdictions are more likely to join their firms' boards, but we find no evidence that this leads to poorer corporate governance.

### 5.4 Managerial quality

The results thus far presented have emphasized the benefits of non-competition enforceability to firms: lower turnover, lower compensation and higher managerial board participation. We now consider the potential selection/indirect cost of requiring managers to sign a covenant not to compete. Prediction 6 states that executives in tough enforcement states will have lower human capital either because the option to accept offers from a competitor is particularly valuable to managers who know they are skilled and so these managers will shun firms requiring non-competition agreements (selection effect) or because managers will work less to build human capital if they are bound to their firm (indirect effect). The Execucomp data set does not provide detailed educational or qualification information about individual executives. To measure executive quality, we consider the set of executives who switch firms. We argue that the compensation received by an executive after he leaves his current firm is an indication of his quality.[9]

The time series implication of Prediction 6 is that better managers will join firms in jurisdictions that experience decreased non-competition enforceability. Testing that implication requires data

[9]Table 4 shows that an executive's compensation at his current firm may reflect either his quality or his having signed a non-competition agreement, but his wage at a new firm should mainly be determined by his quality.

on managers who join a firm after a regulatory shift and then depart for a new firm, and that information is only available for fewer than fifteen managers. Consequently, we are only able to test the cross sectional implication of Prediction 6 that managers that depart firms in high-enforcement jurisdictions will receive relatively low salaries at their new companies, due to their comparatively modest human capital.

There are 1,312 transferring executives with compensation data available at both their previous and new firms. For each executive we regress the log of the compensation they receive at their new firm on *Enforcement*(In-state competition)*, *(In-state competition)*, the log of the compensation they received at their previous firm, their compensation rank at their previous firm (all those below the top five are assigned the sixth rank), state and industry dummies for the previous firm (including state and industry dummies for the new firm has no qualitative effect on the results) and the full set of firm controls. As shown in the first column of Table 7, the coefficient on *Enforcement*(In-state competition)* is negative and significant (t=-2.08). Relative to their previous compensation, executives who transfer from high enforcement jurisdictions receive relatively lower pay increases. This is striking for two reasons. First, as show in Table 4, executives in high enforcement states receive generally low pay in their current firms, so it might have been expected that their compensation would dramatically increase after a transfer. Second, the costs (e.g. legal expenses) of moving to a new company are higher for executives who have signed non-competition agreements, so they might be expected to switch firms only for very large compensation inducements. Despite both these points, executives transferring from high enforcement jurisdictions receive smaller compensation increases upon transferring. This is consistent with Prediction 6 that these are lower quality managers who are less valued by the market for executives.

Executive quality may be measured both by compensation and by promotion to high office. Restricting attention to the set of managers who were not CEOs, we analyze which of these managers were appointed to the CEO position in their new firms.[10] These managers have obviously been rated highly by the market. We regress a binary variable for whether the manager is hired to be the CEO at his new firm on *Enforcement*(In-state competition)*, *(In-state competition)* and the previously described set of controls.[11] We report in the second column of Table 7 that the coefficient on *Enforcement*(In-state competition)* is negative and significant (t=-2.39). Managers from firms in high-enforcement areas are less likely to be promoted to CEO positions at outside

[10]Within-firm promotion to CEO may be determined by multiple factors (Huson, Parrino and Starks, 2001), including the relatively low required wages of internal candidates with non-competition agreements, but promotion to CEO of an external firm should largely reflect managerial quality.

[11]We estimate the regression by OLS, not fixed effects logit, because there are two sets of fixed effects, state and industry. These can both be incorporated into the OLS regression without loss of consistency.

firms. This is further evidence in support of Prediction 6.

We also analyze the market assessment of CEOs in high-enforcement firms. For the set of transferring managers who were CEOs at their former firms, we regress the log of sales at their new firms on *Enforcement*(In-state competition)*, *(In-state competition)* and the previous set of controls (excluding previous rank, which is almost always one for this sample). The controls include the log of sales at the former CEOs' old firms. We find a negative and significant (t=-3.48) coefficient on *Enforcement*(In-state competition)*, as displayed in the third column of 7. CEOs from high-enforcement-jurisdiction firms are hired by relatively small firms. Since managing a large firm is a reward granted to only the most successful CEOs, this is evidence that the CEOs in tough enforcement areas are of lesser quality. In the fourth column we exhibit the results from repeating this regression for the subsample of managers who were CEOs at both their old and new firms. This is a small sample of 78 managers, but we nonetheless find a negative and significant (t=-7.47) coefficient on *Enforcement*(In-state competition)*. Altogether, the results in Table 7 provide consistent evidence in favor of Prediction 6 that managerial human capital is lower in high enforceability jurisdictions. These findings strongly support the importance of the selection/indirect effect of non-competition contracts.

### 5.5 Firm performance and value

We have shown that non-competition enforceability yields benefits to firms in the form of executive stability and lower compensation bills but impedes the recruitment (or development) of high quality managers. We now consider the net impact of non-competition on firm performance. We regress the log of the ratio of the firm's equity market value to its equity book value on *Increased Enforceability*, firm and year fixed effects, the log of the book value of debt, the log of sales, the log of assets, the log of the state unemployment rate and the log of the state personal income. The results, exhibited in the first column of Table 8, show that the coefficient on *Increased Enforceability* is insignificant. To study cross-sectional effects, we regress the log of the market to book ratio on *Enforcement*(In-state competition)*, *(In-state competition)*, state, industry and year fixed effects, the log of the firm age and the previous set of firm controls. As shown in the second column of Table 8, the coefficient on *Enforcement*(In-state competition)* is insignificant. We repeat these regressions, replacing the dependent variable with the return on equity (results reported in columns three and four of Table 8) and the return on assets (results not reported) and find insignificant results throughout. The clear conclusion from this evidence is that non-competition enforcement has no significant effect on

firm value or profitability.[12]

This finding is, perhaps, not wholly surprising and can be rationalized in the following way. The managerial stability and lower compensation benefits to firms generated by covenants not to compete may be roughly offset by the disadvantages of having managers with less human capital. Suppose that the enhanced managerial quality benefits of not requiring a non-competition contract are decreasing in the number of other firms not requiring such contracts. In a plausible equilibrium, the number of firms in high-enforcement jurisdictions voluntarily not requiring non-competition agreements would be such that there is no net benefit to these contracts. Firms in high-enforcement jurisdictions would then have the option of either requiring or not requiring non-competition agreements, but either choice would yield the same firm value. Firms in low-enforcement areas could not require such contracts but would have also have the same value.

While the two firm types will have similar values, they will have very different optimal strategies and governance policies. As we demonstrated empirically, non-competition enforcement has strong implications for executive mobility, optimal executive compensation, managerial board participation and executive quality. In the following analysis, we will examine the effects of non-competition agreements on firm investment.

### 5.6 Research and development spending and firm investment

Predictions 7A and 7B embody competing hypotheses about the effects of non-competition agreements on research and development spending. To test these hypotheses, we regress the log of one plus research and development spending (research and development spending is zero for many firms) on *Increased Enforceability*, firm fixed effects and the standard firm controls from previous regressions. As reported in the first column of Table 9, we find a negative and significant (t=-2.81) coefficient on *Increased Enforceability*. A shift to tighter enforcement of non-competition agreements reduces the log of one plus research and development investment for a given firm by 4.8% of the mean.

We test the cross-sectional implications of Predictions 7A and 7B by regressing the log of one plus research and development spending (research and development spending is zero for many firms) on *Enforcement*(In-state competition)*, *(In-state competition)*, state and industry fixed effects and the usual set of firm controls. We show in the second column of Table 9 that the coefficient on *Enforcement*(In-state competition)* is negative and significant (t=-2.17). A one-standard-deviation increase in *Enforcement*(In-state competition)* reduces the log of one plus research and development

[12]In unreported results, we also find no significant effect on debt to equity ratios.

spending by 3.8% of the mean. The time series and cross-sectional results both provide support for Prediction 7B. The organization capital literature has emphasized that research and development creates intangible value that is vulnerable to being removed from the firm by departing employees. It might be expected that high enforceability of non-competition agreements would therefore facilitate research and development investment. This is the direct effect of non-competition agreements underlying Prediction 7A. We find, however, that the selection/indirect effect is empirically dominant. Non-compete agreements discourage better employees from joining the firm and dissuade them from making investments in their own human capital. This places firms that require covenants not to compete at a comparative disadvantage in engaging in research and development, so they pursue less of it.

This result provides a new perspective on Saxenian's (1996) discussion of the differences between the computer industries in California and Massachusetts. Saxenian describes the greater communication amongst Silicon Valley firms relative to those along Massachusetts's Route 128 and argues that this information-sharing advantage led to the success of the California firms. Gilson (1999) and Rajan and Zingales (2001) argue that differences in non-compete enforcement between the California and Massachusetts can explain organizational and cultural differences between firms in the two states. Our empirical analysis indicates that firms in low non-competition enforcement jurisdictions are best suited to make investments in research and development. This suggests that the success of Silicon Valley may in part be linked to California's public policy of not enforcing covenants not to compete. Our empirical findings, however, are not driven simply by a state-to-state comparison; there are, after all, many differences between states that are unrelated to non-competition law. Instead we analyze both time series variation in the laws of given states and cross-sectional variation in non-competition enforceability interacted with in-state competition, while controlling for state fixed effects. This finding is of interest because of the positive externalities commonly attributed to R&D (e.g. Hall, 2002).

Predictions 8A and 8B relate non-competition enforceability to firms' physical capital-labor ratios. To test the predictions, we regress the log of one plus the ratio of capital expenditures to the number of employees on *Increased Enforceability*, firm fixed effects and the usual controls. The results, displayed in the third column of Table 9, show that increased non-competition enforceability significantly (t=-4.33) reduces capex per employee. A shift to a regime of greater enforceability leads to a decrease in the log of capex per employee by 5.7% of the mean. We test the cross-sectional implications of Predictions 8A and 8B by regressing the log of one plus the ratio of capital expenditures to the number of employees on *Enforcement*(In-state competition)*, *(In-state*

*competition)*, state and industry fixed effects and the standard set of firm controls. As shown in the fourth column of Table 9, the coefficient on *Enforcement*(In-state competition)* is negative and significant (t=-3.18). A one-standard-deviation increase in *Enforcement*(In-state competition)* reduces the log of the capex per employee by 3.9% of the mean. The time series and cross-sectional evidence both provide strong support for Prediction 8B. Firms in high non-compete enforcement jurisdictions have substantially lower physical capital to labor ratios. This is further evidence that is consistent with the selection/indirect effect. The low reliance on labor by firms in low enforceability areas may also reflect their desire to minimize their dependence on potentially unstable employees.

### 5.7 Merger and acquisition activity

We now consider the effects of non-competition law on firms' merger and acquisition strategies. Predictions 9A and 9B link non-competition enforceability to the likelihood that a firm will be acquired. In particular, Prediction 9A indicates that firms in jurisdictions with high enforceability and high assignability of non-competition contracts to acquirers will be more likely to be acquired. To test this prediction, we require a measure of assignability, since we will analyze the effect of the interaction between enforceability and assignability. Using Malsberger (2004), we define a new variable *Assignment* that is equal to one for all firms with headquarters in a state if the state permits firms to assign non-competition agreements to successors without the consent of employees and that is equal to zero otherwise. If the state enforces contracts that explicitly permit future assignment but does not allow for assignment otherwise, we score the *Assignment* variable as one. (This is in keeping with our practice of measuring enforceability of contracts, rather than comparing state rules that apply in the absence of a contract.) Most, but not all, states allow for assignment (Carr, 1999). The legal shifts in Texas and Louisiana discussed in Section 4.2 did not affect the issue of assignment, but the 1996 Florida statue clarified the treatment of assignment by enforcing it if and only original contract permits it. Previous Florida law was somewhat confused on the question of assignability. In considering time series changes, therefore, we will make use of our previous variable *Increased Enforceability*, since the significant changes in all three states were mainly on the dimension on enforceability.

We test Predictions 9A and 9B by regressing an indicator for whether a firm was acquired in a given year on *Increased Enforceability*, firm fixed effects, year fixed effects and the following firm controls, all measured as of the previous year: one- and three-year total returns, the log of firm equity book and market values, the log of firm sales and the log of firm assets. We also include the log of the state unemployment rate and the log of the state personal per capita in-

come in the previous year. The estimation is via fixed effects logit. As is shown in column one of Table 10, the coefficient on *Increased Enforceability* is insignificant. To test the cross-sectional implications of Predictions 9A and 9B, we regress an indicator for whether a firm was acquired in a given year on *Enforcement*Assignment*(In-state competition)*, *(In-state competition)*, state and industry fixed effects and the usual set of firm controls, with the firm variables all measured as of the previous year. As discussed earlier, the estimation is via OLS due to the two sets of fixed effects. The results are displayed in the second column of Table 10. The coefficient on *Enforcement*Assignment*(In-state competition)* is insignificant. We also repeated this regression, replacing *Enforcement*Assignment*(In-state competition)* with *Enforcement*(In-state competition)*. The coefficient on the latter variable was insignificant in this specification. (These results are unreported.) There is no significant evidence in favor of either Prediction 9A or Prediction 9B.

Predictions 10A and 10B connect the enforceability of a firm's jurisdiction to the frequency and magnitude of its acquisitions.[13] We analyze these predictions first by regressing an indicator variable for whether a firm made any acquisitions at all in a given year on *Increased Enforceability*, firm fixed effects and the usual controls. (Firm controls are for the given year, not the previous year, as in the two preceding regressions.) The estimation is via fixed effects logit, and the results are displayed in the third column of Table 10. We find a positive and significant (t=1.77) coefficient on *Increased Enforceability*. In our cross-sectional test, we regress the indicator for any acquisitions on *Enforcement*(In-state competition)*, *(In-state competition)* and the standard controls. This estimation is via OLS. The regression yields an insignificant coefficient on *Enforcement*(In-state competition)*, as reported in the fourth column. The results are somewhat mixed, but there is no strong evidence suggesting the firms in high enforcement areas are more likely to engage in an acquisition in a given year.

We now consider the size of acquisitions undertaken. We restrict the data to those firm-years in which there is some acquisition activity, and we regress the log of the funds used for acquisition on *Increased Enforceability*, firm fixed effects and the previous controls. As displayed in the fifth column of Table 10, the coefficient on *Increased Enforceability* is positive and significant (t=2.44). In the cross-sectional regression, we find a positive and significant (t=2.87) coefficient on *Enforcement*(In-state competition)* (sixth column). These results provide support for Prediction 10A: while firms in high enforcement areas may not make acquisitions in more years, they do spend more on acquisitions

[13]Kumar, Rajan and Zingales (2001) study the country-level relationship between the efficiency of legal systems and average firm size.

in years in which they are active. The Compustat data do not permit us to distinguish whether high-enforcement firms make a greater number or larger acquisitions in their active years, but the overall scale of their activities in those years is greater.

### 5.8 Direct versus Selection/Indirect Effects

We found support for both the direct and selection/indirect effects of non-competition agreements. The executive compensation findings provide clear evidence for the direct effect (especially the fixed effect results) but are also consistent with the selection/indirect effect. The board participation results support the direct effect, while the managerial quality findings strongly substantiate the selection/indirect effect. The research and development spending and firm investment results are consistent with the selection/indirect effect but are not consistent with the direct effect. Clearly both effects are empirically relevant, but when they generate conflicting predictions, we have found that the selection/indirect effect dominates. This suggests that the consequences of non-competition agreements for managerial human capital can be substantial.

## 6 Conclusion

Our study of the time series and cross-sectional variation in non-competition enforceability across the U.S. states demonstrates the importance of these legal regulations for executive mobility, executive compensation and firm investment. We show that increased enforceability leads to fewer executive within-industry transfers, lower and more salary-based compensation, greater executive board participation, reduced R&D spending and lower capital expenditures per employee. We also find that firms in high enforcement jurisdictions have less skilled managers.

A new stream of literature on the theory of the firm has emphasized the important role and somewhat unstable character of human capital assets. Our results on executive firm transfers and job tenure show that non-competition agreements can serve as an effective means for securing employee resources within the boundary of the firm. Our findings on the market assessment of transferring executives and research and development investment indicate, however, that the human capital of the firm's managers is changed by the presence of covenants not to compete. We find that the enforceability of non-competition agreements has no effect on firm value. Our work thus suggests a direction for the analysis of the socially-optimal level of enforcement. In the absence of firm-level value implications, the optimal level of enforceability should probably promote those activities, including research and development, that create positive externalities.

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# Appendix

## Questions and Thresholds

The following twelve questions from Malsberger (2004) are used to evaluate the level of non-competition agreement enforceability in each state. Each state is granted one point for each question concerning which its laws lie above the threshold.

**Question 1.** Is there a state statue of general application that governs the enforceability of covenants not to compete?

**Threshold 1.** States that enforce non-competition agreements outside a sale-of-business context receive a score of one.

**Question 2.** What is an employer's protectable interest and how is it defined?

**Threshold 2.** States in which the employer can prevent the employee from future independent dealings with all the firm's customers, not merely with the customers with whom the employee had direct contact, receive a score of one.

**Question 3.** What must the plaintiff be able to show to prove the existence of an enforceable covenant not to compete?

**Threshold 3.** Laws that place greater weight on the interests of the firm relative to those of the former employee are above the threshold. For example, a law that requires that the contract be reasonably protective of the firm's business interests and only meet the condition of not being unreasonably injurious to the employee's interests would receive a score of one.

**Question 4.** Does the signing of a covenant not to compete at the inception of the employment relationship provide sufficient consideration to support the covenant?

**Threshold 4.** States for which the answer to Question 4 is clearly "Yes" are above the threshold.

**Question 5.** Will a change in the terms and conditions of employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?

**Threshold 5.** States for which the answer to Question 5 is clearly "Yes" are above the threshold.

**Question 6.** Will continued employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?

**Threshold 6.** States for which the answer to Question 6 is clearly "Yes" are above the threshold.

**Question 7.** What factors will the court consider in determining whether time and geographic restrictions in the covenant are reasonable?

**Threshold 7.** Jurisdictions in which courts are instructed not to consider economic or other hardships faced by the employee are above the threshold.

**Question 8.** Who has the burden of proving the reasonableness or unreasonableness of the covenant not to compete?

**Threshold 8.** States in which the burden of proof is clearly placed on the employee are above the threshold.

**Question 9.** What type of time or geographic restrictions has the court found to be reasonable? Unreasonable?

**Threshold 9.** Jurisdictions in which three-year statewide restrictions have been upheld receive a score of one.

**Question 10.** If the restrictions in the covenant not to compete are unenforceable because they are overbroad, are the courts permitted to modify the covenant to make the restrictions more narrow and to make the covenants enforceable?

**Threshold 10.** States for which the answer to Question 10 is clearly "Yes" are above the threshold.

**Question 11.** If the employer terminates the employment relationship, is the covenant enforceable?
**Threshold 11.** States for which the answer to Question 11 is clearly "Yes" are above the threshold.
**Question 12.** What damages may an employer recover and from whom for breach of a covenant not to compete?
**Threshold 12.** If, in addition to lost profits, there is a potential for punitive damages against the former employee, the state receives a score of one. States that explicitly exclude consideration of the reasonableness of the contract from the calculation of damages are also above the threshold.

Non-competition Enforceability Index

| State | Score | State | Score |
|---|---|---|---|
| Alabama | 5 | Missouri | 7 |
| Alaska | 3 | Montana | 2 |
| Arizona | 3 | Nebraska | 4 |
| Arkansas | 5 | Nevada | 5 |
| California | 0 | New Hampshire | 2 |
| Colorado | 2 | New Jersey | 4 |
| Connecticut | 3 | New Mexico | 2 |
| Delaware | 6 | New York | 3 |
| District of Columbia | 7 | North Carolina | 4 |
| Florida 1992-1996 | 7 | North Dakota | 0 |
| Florida 1997-2004 | 9 | Ohio | 5 |
| Georgia | 5 | Oklahoma | 1 |
| Hawaii | 3 | Oregon | 6 |
| Idaho | 6 | Pennsylvania | 6 |
| Illinois | 5 | Rhode Island | 3 |
| Indiana | 5 | South Carolina | 5 |
| Iowa | 6 | South Dakota | 5 |
| Kansas | 6 | Tennessee | 7 |
| Kentucky | 6 | Texas 1992-1994 | 5 |
| Louisiana 1992-2001, 2004 | 4 | Texas 1995-2004 | 3 |
| Louisiana 2002-2003 | 0 | Utah | 6 |
| Maine | 4 | Vermont | 5 |
| Maryland | 5 | Virginia | 3 |
| Massachusetts | 6 | Washington | 5 |
| Michigan | 5 | West Virginia | 2 |
| Minnesota | 5 | Wisconsin | 3 |
| Mississippi | 4 | Wyoming | 4 |

Table 1: Summary Statistics

| **Variable** | Mean | Median | Standard Deviation |
|---|---|---|---|
| Increased Enforceability | 0.07 | 0 | 0.25 |
| Enforceability | 0.32 | 0.33 | 0.18 |
| In-state Competition | 0.10 | 0.01 | 0.18 |
| 1 year total return | 123.74 | 12.72 | 8,884.51 |
| 3 year total return | 13.04 | 11.29 | 33.63 |
| Firm Age | 21.23 | 16.00 | 15.53 |
| Book Value | 1,538.43 | 369.21 | 4,650.71 |
| Sales | 3,312.71 | 760.75 | 9,850.77 |
| State Unemployment Rate | 5.56 | 5.40 | 1.50 |
| State Personal Income per Capita | 27,371.04 | 26,862.00 | 5,327.60 |
| Within-industry transfers | 0.02 | 0 | 0.20 |
| All transfers | 0.10 | 0 | 0.39 |
| Executive Job Tenure | 14.69 | 9.00 | 12.82 |
| Compensation Level | 2,173,953.72 | 932,828.00 | 6,606,673.16 |
| Compensation Growth | 0.33 | 0.10 | 1.01 |
| Salary/Compensation | 0.38 | 0.34 | 0.2344 |
| Options Granted? | 0.73 | 1.00 | 0.45 |
| Reload Options Granted? | 0.02 | 0 | 0.13 |
| Top 5 Executives on Board | 1.81 | 2.00 | 1.03 |
| Research and Development Spending | 123.40 | 14.68 | 502.22 |
| Capex per Employee | 325,226.73 | 13,131.31 | 5,176,368.54 |
| Was Acquired? | 0.02 | 0 | 0.14 |
| Made Acquisition? | 0.30 | 0 | 0.46 |
| Size of Acquisitions | 82.64 | 0 | 508.58 |

Book Value, Market Value, Sales, Assets, Research and Development Spending and Size of Acquisitions are expressed in millions of dollars. Compensation growth is truncated at -90% and +900%.

Table 2: Executive Mobility

| **Dependent Variable** | Within-ind. transfers | Within-ind. transfers | All transfers | All transfers | Between-ind. transfers | Between-ind. transfers |
|---|---|---|---|---|---|---|
| *# Obs.* | *20,965* | *20,474* | *20,965* | *20,474* | *20,965* | *20,474* |
| Increased Enforceability | -0.0264** | | -0.0544** | | -0.0280 | |
| | (-2.44) | | (-2.10) | | (-1.39) | |
| Enforce*(In-state Comp.) | | -0.2200** | | -0.3003** | | -0.0803 |
| | | (-3.08) | | (-2.04) | | (-0.63) |
| In-state Competition | | 0.1013** | | 0.1039** | | 0.0026 |
| | | (3.99) | | (2.06) | | (0.06) |
| 1 year total return | 0.0000 | 0.0000** | 0.0000 | 0.0000 | 0.0000 | 0.0000 |
| | (0.52) | (2.24) | (-1.32) | (1.29) | (-1.41) | (0.71) |
| 3 year total return | -0.0001 | 0.0000 | -0.0003** | -0.0002** | -0.0002** | -0.0002** |
| | (-1.23) | (-0.96) | (-2.74) | (-2.23) | (-2.60) | (-2.04) |
| Log (Firm Age) | | -0.0010 | | -0.0007 | | 0.0002 |
| | | (-0.28) | | (-0.13) | | (0.05) |
| Log (Book Value) | 0.0007 | 0.0022 | -0.0207* | -0.0234** | -0.0214** | -0.0257** |
| | (0.16) | (0.70) | (-1.80) | (-2.53) | (-2.11) | (-3.03) |
| Log (Market Value) | 0.0105** | 0.0041 | 0.0286** | 0.0110* | 0.0181** | 0.0069 |
| | (3.06) | (1.62) | (3.33) | (1.84) | (2.41) | (1.32) |
| Log (Sales) | 0.0042 | 0.0108** | -0.0051 | 0.0217** | -0.0093 | 0.0108** |
| | (0.79) | (3.61) | (-0.42) | (3.40) | (-0.93) | (1.99) |
| Log (Assets) | -0.0163** | -0.0068* | 0.0115 | 0.0311** | 0.0277** | 0.0379** |
| | (-2.16) | (-1.74) | (0.72) | (3.50) | (2.15) | (4.78) |
| Log (State Unemployment) | 0.0016 | -0.0140 | -0.0282 | -0.0229 | -0.0298 | -0.0089 |
| | (0.08) | (-0.82) | (-0.89) | (-0.76) | (-1.20) | (-0.35) |
| Log (State Personal Income) | 0.0471 | -0.0319 | 0.0655 | 0.2211 | 0.0185 | 0.2530* |
| | (0.61) | (-0.29) | (0.51) | (1.21) | (0.19) | (1.79) |
| Estimation Method | *OLS* | *OLS* | *OLS* | *OLS* | *OLS* | *OLS* |
| Firm Fixed Effects? | Yes | No | Yes | No | Yes | No |
| State Fixed Effects? | No | Yes | No | Yes | No | Yes |
| Industry Fixed Effects? | No | Yes | No | Yes | No | Yes |
| Clustering | State-Year | Firm | State-Year | Firm | State-Year | Firm |
| $R^2$ | 0.19 | 0.04 | 0.22 | 0.09 | 0.22 | 0.09 |

The regressions are estimated via ordinary least squares (OLS) as described, with $t$-statistics reported in parentheses using robust "sandwich" standard errors.

*,** Indicates significance at the 10% and 5% levels, respectively.

Table 3: Executive Tenure

| **Dependent Variable** | Tenure | Tenure | Departures | Departures |
|---|---|---|---|---|
| *# Obs.* | *5,765* | *5,628* | *20,965* | *20,474* |
| Increased Enforceability | 1.1518** | | -0.0804** | |
| | (3.14) | | (-2.26) | |
| Enforce*(In-state Comp.) | | 28.8326** | | -0.3899 |
| | | (2.16) | | (-1.43) |
| In-state Competition | | -5.7817 | | 0.1720* |
| | | (-1.39) | | (1.80) |
| 1 year total return | 0.0000 | 0.0000 | 0.0000 | 0.0000 |
| | (0.80) | (0.07) | (-0.68) | (0.36) |
| 3 year total return | -0.0258** | -0.0220** | -0.0012** | -0.0017** |
| | (-5.55) | (-3.59) | (-5.17) | (-4.66) |
| Log (Firm Age) | | 3.8960** | | 0.0151* |
| | | (6.57) | | (1.70) |
| Log (Book Value) | 1.5278** | 1.7951** | -0.0015 | -0.0090 |
| | (4.97) | (3.08) | (-0.08) | (-0.69) |
| Log (Market Value) | 1.3284** | 1.4268** | -0.0535** | -0.0489** |
| | (5.17) | (3.39) | (-4.03) | (-4.10) |
| Log (Sales) | 0.3079 | 0.0289 | -0.0247 | 0.0095 |
| | (1.14) | (0.05) | (-1.41) | (0.90) |
| Log (Assets) | -0.0110 | -1.0732 | 0.0581** | 0.0759** |
| | (-0.03) | (-1.31) | (2.55) | (5.51) |
| Log (State Unemployment) | 0.6330 | 1.1743 | -0.1256** | -0.0916* |
| | (1.04) | (0.59) | (-2.83) | (-1.87) |
| Log (State Personal Income) | -3.8089** | 11.2703 | 0.5187** | 0.4501 |
| | (-2.69) | (1.01) | (2.92) | (1.61) |
| Estimation Method | *OLS* | *OLS* | *OLS* | *OLS* |
| Firm Fixed Effects? | Yes | No | Yes | No |
| State Fixed Effects? | No | Yes | No | Yes |
| Industry Fixed Effects? | No | Yes | No | Yes |
| Clustering | State-Year | Firm | State-Year | Firm |
| $R^2$ | 0.52 | 0.56 | 0.24 | 0.09 |

The regressions are estimated via ordinary least squares (OLS) as described, with $t$-statistics reported in parentheses using robust "sandwich" standard errors.

*,** Indicates significance at the 10% and 5% levels, respectively.

Table 4: Executive Compensation

| **Dependent Variable** | Compensation growth | Log of Compensation level | Compensation ratio | Compensation ratio |
|---|---|---|---|---|
| *# Obs.* | *74,068* | *16,337* | *16,743* | *16,337* |
| Increased Enforceability' | -0.1282** | | -0.0495* | |
| | (-2.42) | | (-1.73) | |
| Enforce*(In-state Comp.) | | -1.3745** | | -0.4648* |
| | | (-3.64) | | (-1.66) |
| In-state Competition | | 0.3954** | | 0.1289 |
| | | (2.90) | | (1.29) |
| 1 year total return | 0.0000 | 0.0000** | 0.0000** | 0.0000 |
| | (1.52) | (2.17) | (-8.39) | (-0.62) |
| 3 year total return | 0.0030** | 0.0018** | 0.0006** | 0.0006** |
| | (6.37) | (7.62) | (3.01) | (3.26) |
| Log (Firm Age) | | -0.0488** | | -0.0017 |
| | | (-3.24) | | (-0.16) |
| Log (Book Value) | -0.0482 | -0.0636** | -0.0004 | -0.0338** |
| | (-1.54) | (-3.68) | (-0.02) | (-2.51) |
| Log (Market Value) | 0.1283** | 0.2265** | 0.0238* | 0.0248** |
| | (5.13) | (15.28) | (1.86) | (2.41) |
| Log (Sales) | 0.0298 | 0.0426** | -0.0395** | -0.0250** |
| | (0.96) | (2.14) | (-2.03) | (-1.97) |
| Log (Assets) | -0.0629 | 0.2434** | 0.0232 | 0.0597** |
| | (-1.22) | (9.35) | (0.92) | (3.40) |
| Log (State Unemployment) | 0.1287* | -0.0172 | 0.0013 | 0.0271 |
| | (1.89) | (-0.31) | (0.03) | (0.61) |
| Log (State Personal Income) | -0.0983 | 0.3254 | 0.0462 | 0.1914 |
| | (-0.33) | (0.99) | (0.26) | (0.68) |
| Unit of Observation | Exec | Firm | Firm | Firm |
| Estimation Method | *OLS* | *OLS* | *OLS* | *OLS* |
| Exec/Firm Fixed Effects? | Exec | No | Firm | No |
| State Fixed Effects? | No | Yes | No | Yes |
| Industry Fixed Effects? | No | Yes | No | Yes |
| Clustering | Firm | Firm | State-Year | Firm |
| $R^2$ | 0.28 | 0.68 | 0.42 | 0.11 |

The regressions are estimated via ordinary least squares (OLS) as described, with $t$-statistics reported in parentheses using robust "sandwich" standard errors.

*,** Indicates significance at the 10% and 5% levels, respectively.

Table 5: Form of Executive Compensation

| **Dependent Variable** | Salary growth - Comp. growth | Log of $\frac{Salary}{Compensation}$ | Options granted? | Options granted? | Reload options? | Reload options? |
|---|---|---|---|---|---|---|
| *# Obs.* | *73,899* | *16,337* | *74,997* | *16,337* | *74,997* | *16,337* |
| Increased Enforceability' | 0.1087** | | 0.1531 | | -2.1948** | |
| | (2.01) | | (1.34) | | (-3.78) | |
| Enforce*(In-state Comp.) | | 1.4298** | | -1.4920 | | -0.7784** |
| | | (3.99) | | (-1.36) | | (-2.41) |
| In-state Competition | | -0.4670** | | 0.6005* | | 0.1831* |
| | | (-3.64) | | (1.70) | | (1.92) |
| 1 year total return | 0.0000 | 0.0000** | 0.0000 | 0.0000 | -0.0008 | 0.0000 |
| | (-1.51) | (-2.43) | (1.08) | (0.70) | (-0.61) | (-1.28) |
| 3 year total return | -0.0032** | -0.0021** | 0.0003 | -0.0012* | 0.0122** | -0.0001 |
| | (-6.76) | (-8.34) | (0.41) | (-1.88) | (3.05) | (-0.50) |
| Log (Firm Age) | | 0.0928** | | -0.0483 | | 0.0018 |
| | | (6.61) | | (-1.21) | | (0.10) |
| Log (Book Value) | 0.0449 | 0.0713** | 0.3349** | -0.0372 | 0.0226 | -0.0014 |
| | (1.40) | (4.03) | (6.04) | (-0.78) | (0.10) | (-0.08) |
| Log (Market Value) | -0.0964** | -0.2139** | 0.1977** | 0.1836** | 1.1227** | 0.0442** |
| | (-3.81) | (-14.22) | (4.64) | (4.93) | (4.91) | (3.43) |
| Log (Sales) | -0.0125 | 0.0575** | 0.2278** | 0.0242 | 0.1358 | -0.0099 |
| | (-0.39) | (3.16) | (3.49) | (0.55) | (0.39) | (-0.63) |
| Log (Assets) | 0.0750 | -0.1518** | -0.2679** | 0.0352 | -0.4136 | 0.0117 |
| | (1.42) | (-5.32) | (-3.11) | (0.62) | (-1.01) | (0.49) |
| Log (State Unemployment) | -0.0845 | 0.0529 | 0.4402** | 0.3172* | 0.9119 | -0.0382 |
| | (-1.22) | (0.98) | (2.48) | (1.70) | (1.46) | (-0.69) |
| Log (State Personal Income) | -0.0004 | -0.2426 | -2.1383** | -0.9975 | 0.3784 | -0.2351 |
| | (0.00) | (-0.76) | (-2.80) | (-0.90) | (0.13) | (-0.70) |
| Unit of Observation | Exec | Firm | Exec | Firm | Exec | Firm |
| Estimation Method | *OLS* | *OLS* | *Logit* | *OLS* | *Logit* | *OLS* |
| Exec/Firm Fixed Effects? | Exec | No | Exec | No | Exec | No |
| State Fixed Effects? | No | Yes | No | Yes | No | Yes |
| Industry Fixed Effects? | No | Yes | No | Yes | No | Yes |
| Clustering | Firm | Firm | No | Firm | No | Firm |
| $R^2$ | 0.28 | 0.49 | 0.16 | 0.18 | 0.23 | 0.15 |

The regressions are estimated via ordinary least squares (OLS) or fixed effect (conditional) logistic regression (Logit), as described, with OLS $t$-statistics reported in parentheses using robust "sandwich" standard errors. Reported $R^2$ for Logit specifications is McFadden's pseudo $R^2$.

*,** Indicates significance at the 10% and 5% levels, respectively.

Table 6: Board Membership and Corporate Governance

| **Dependent Variable** | Executives on Board | Executives on Board | Governance Index | Governance Index | Protection Index | Protection Index |
|---|---|---|---|---|---|---|
| *# Obs.* | *19,511* | *19,037* | *6,527* | *6,357* | *6,527* | *6,357* |
| Increased Enforceability | 0.1212** | | -0.1650 | | 0.0127 | |
| | (3.38) | | (-1.31) | | (0.34) | |
| Enforce*(In-state Comp.) | | 1.3531** | | 3.2060 | | -0.3499 |
| | | (2.08) | | (1.23) | | (-0.31) |
| In-state Competition | | -0.6383** | | -1.3701 | | -0.1040 |
| | | (-3.17) | | (-1.55) | | (-0.27) |
| 1 year total return | 0.0000** | 0.0000 | 0.0002 | -0.0004 | 0.0001 | 0.0002 |
| | (2.47) | (-0.29) | (0.93) | (-0.83) | (0.62) | (0.84) |
| 3 year total return | -0.0007** | -0.0008** | -0.0003 | 0.0012 | 0.0005 | 0.0020** |
| | (-3.39) | (-2.37) | (-0.39) | (0.79) | (1.33) | (2.93) |
| Log (Firm Age) | | 0.0086 | | 0.9788** | | 0.7888** |
| | | (0.34) | | (9.46) | | (18.88) |
| Log (Book Value) | 0.0476** | 0.1505** | 0.0343 | 0.0337 | -0.0172 | -0.0246 |
| | (2.47) | (5.36) | (0.71) | (0.35) | (-0.61) | (-0.53) |
| Log (Market Value) | 0.0641** | 0.0607** | -0.0624* | -0.1352 | -0.0684** | -0.1303** |
| | (4.38) | (2.80) | (-1.83) | (-1.59) | (-3.27) | (-3.59) |
| Log (Sales) | 0.0488** | -0.0297 | 0.1106 | 0.3508** | 0.0592* | 0.1436** |
| | (2.32) | (-1.11) | (1.49) | (3.17) | (1.75) | (3.27) |
| Log (Assets) | 0.0231 | -0.0858** | 0.0642 | -0.1331 | 0.0136 | 0.0537 |
| | (0.78) | (-2.16) | (0.70) | (-1.00) | (0.30) | (0.97) |
| Log (State Unemployment) | 0.0651 | -0.0011 | 0.1072 | 0.3861 | 0.0000 | 0.2525** |
| | (1.09) | (-0.01) | (0.75) | (1.42) | (0.00) | (2.06) |
| Log (State Personal Income) | 0.2674 | 0.3040 | -0.8323 | -2.4007 | 0.0219 | -0.8419 |
| | (1.11) | (0.54) | (-1.01) | (-1.50) | (0.05) | (-1.15) |
| Estimation Method | *OLS* | *OLS* | *OLS* | *OLS* | *OLS* | *OLS* |
| Firm Fixed Effects? | Yes | No | Yes | No | Yes | No |
| State Fixed Effects? | No | Yes | No | Yes | No | Yes |
| Industry Fixed Effects? | No | Yes | No | Yes | No | Yes |
| Clustering | State-Year | Firm | State-Year | Firm | State-Year | Firm |
| $R^2$ | 0.62 | 0.22 | 0.93 | 0.36 | 0.90 | 0.41 |

The regressions are estimated via ordinary least squares (OLS) as described, with $t$-statistics reported in parentheses using robust "sandwich" standard errors.

*,** Indicates significance at the 10% and 5% levels, respectively.

Table 7: Market Assessment of Transferring Executives

| **Dependent Variable** | Log (compensation) at new firm | Hired as CEO? | Log (Sales) of new firm | Log (Sales) of new firm |
|---|---|---|---|---|
| *# Obs.* | *1,312* | *1,095* | *189* | *78* |
| Enforce*(In-state Comp.) | -2.8581** | -1.3925** | -19.3306** | -152.0772** |
| | (-2.08) | (-2.39) | (-3.48) | (-7.47) |
| In-state Competition | 0.8951** | 0.3998** | 2.7486* | 20.9314** |
| | (2.11) | (1.99) | (1.93) | (2.58) |
| Log (compensation) at previous firm | 0.5012** | 0.0889** | 0.4143 | -0.5027 |
| | (10.67) | (4.14) | (1.40) | (-0.74) |
| Previous rank | -0.0263 | -0.0125 | | |
| | (-1.15) | (-1.16) | | |
| 1 year total return | 0.0021** | 0.0004* | 0.0058** | -0.0050 |
| | (3.46) | (1.87) | (5.69) | (-0.27) |
| 3 year total return | -0.0016 | -0.0010 | -0.0054 | -0.0182** |
| | (-0.86) | (-1.42) | (-0.72) | (-2.11) |
| Log (Firm Age) | -0.0622 | -0.0109 | 0.1394 | -0.4805 |
| | (-1.28) | (-0.47) | (0.64) | (-1.43) |
| Log (Sales) | 0.2384** | 0.0543* | 0.6386** | -1.9076** |
| | (3.90) | (1.77) | (2.15) | (-3.56) |
| Log (Book Value) | 0.0317 | -0.0070 | 0.5347 | 2.7393** |
| | (0.52) | (-0.21) | (1.18) | (2.85) |
| Log (Market Value) | 0.0964* | 0.0621** | -0.6169* | -0.0383 |
| | (1.65) | (2.19) | (-1.65) | (-0.12) |
| Log (Assets) | -0.2095** | -0.0777** | -0.2854 | 0.6148 |
| | (-2.69) | (-1.99) | (-0.59) | (0.64) |
| Log (State Unemployment) | -0.0878 | 0.0057 | -0.1012 | -0.9376 |
| | (-0.25) | (0.05) | (-0.14) | (-0.69) |
| Log (State Personal Income) | -0.0761 | -0.2492 | 2.0194 | -1.4780 |
| | (-0.13) | (-1.42) | (1.11) | (-0.58) |
| Estimation Method | *OLS* | *OLS* | *OLS* | *OLS* |
| Sample | All | Non-CEOs at old firm | CEOs at old firm | CEOs at old and new firms |
| State Fixed Effects? | Yes | Yes | Yes | Yes |
| Industry Fixed Effects? | Yes | Yes | Yes | Yes |
| $R^2$ | 0.46 | 0.25 | 0.76 | 0.98 |

The regressions are estimated via ordinary least squares (OLS) as described, with $t$-statistics reported in parentheses using robust "sandwich" standard errors.

*,** Indicates significance at the 10% and 5% levels, respectively.

Table 8: Firm Performance

| **Dependent Variable** | Log of Market to Book Ratio | Log of Market to Book Ratio | Return on Equity | Return on Equity |
|---|---|---|---|---|
| *# Obs.* | *20,875* | *20,384* | *17,746* | *17,313* |
| Increased Enforceability | -0.0417 | | -4.2798 | |
| | (-1.01) | | (-0.57) | |
| Enforce*(In-state Comp.) | | -0.0771 | | 2.6021 |
| | | (-0.18) | | (0.04) |
| In-state Competition | | 0.1282 | | -62.3172 |
| | | (0.89) | | (-1.32) |
| Log (Firm Age) | | -0.0919** | | -288.9220 |
| | | (-5.34) | | (-1.34) |
| Log (Debt) | 0.0001 | -0.0322** | 260.1298 | 13.7342 |
| | (0.02) | (-4.47) | (1.20) | (1.46) |
| Log (Sales) | 0.2686** | 0.1847** | -289.8920 | 2.9834 |
| | (7.52) | (8.24) | (-1.28) | (0.26) |
| Log (Assets) | -0.3381** | -0.0843** | -18.0083 | 159.1344 |
| | (-9.47) | (-3.50) | (-1.03) | (1.21) |
| Log (State Unemployment) | -0.1334** | -0.1462** | 24.6577 | |
| | (-2.01) | (-2.19) | (0.55) | |
| Log (State Personal Income) | 0.4343* | 0.8900** | | |
| | (1.69) | (2.31) | | |
| Estimation Method | *OLS* | *OLS* | *OLS* | *OLS* |
| Firm Fixed Effects? | Yes | No | Yes | No |
| State Fixed Effects? | No | Yes | No | Yes |
| Industry Fixed Effects? | No | Yes | No | Yes |
| Clustering | State-Year | Firm | State-Year | Firm |
| $R^2$ | 0.64 | 0.31 | 0.52 | 0.06 |

The regressions are estimated via ordinary least squares (OLS) as described, with $t$-statistics reported in parentheses using robust "sandwich" standard errors.

*,** Indicates significance at the 10% and 5% levels, respectively.

Table 9: Research and Development and Capital Expenditures

| **Dependent Variable** | Log of R&D Spending | Log of R&D Spending | Log of Capex per Employee | Log of Capex per Employee |
|---|---|---|---|---|
| *# Obs.* | *10,608* | *10,351* | *18,939* | *18,456* |
| Increased Enforceability | -0.1369** | | -0.1383** | |
| | (-2.81) | | (-4.33) | |
| Enforce*(In-state Comp.) | | -1.8772** | | -1.4907** |
| | | (-2.17) | | (-3.18) |
| In-state Competition | | 0.4495 | | 0.4584** |
| | | (1.58) | | (2.74) |
| 1 year total return | 0.0000 | 0.0000** | 0.0000* | 0.0000 |
| | (-1.14) | (-2.24) | (-1.66) | (-1.58) |
| 3 year total return | -0.0016** | -0.0036** | 0.0014** | 0.0006** |
| | (-5.72) | (-5.78) | (7.52) | (2.48) |
| Log (Firm Age) | | 0.0865** | | -0.1341** |
| | | (2.49) | | (-6.67) |
| Log (Book Value) | -0.0380** | -0.0356 | 0.0764** | 0.0863** |
| | (-2.76) | (-1.04) | (5.34) | (3.62) |
| Log (Market Value) | 0.0790** | 0.2269** | 0.0958** | 0.1871** |
| | (5.62) | (7.38) | (7.38) | (11.76) |
| Log (Sales) | 0.0748** | -0.0567 | -0.0377 | -0.1700** |
| | (3.68) | (-1.45) | (-1.26) | (-5.54) |
| Log (Assets) | 0.4390** | 0.6122** | 0.0463** | 0.0114 |
| | (16.69) | (10.79) | (1.97) | (0.29) |
| Log (State Unemployment) | -0.1830** | -0.0222 | -0.0411 | -0.0405 |
| | (-4.02) | (-0.23) | (-0.86) | (-0.61) |
| Log (State Personal Income) | -0.0220 | 0.2684 | 0.2903* | 0.6910* |
| | (-0.12) | (0.43) | (1.67) | (1.80) |
| Estimation Method | *OLS* | *OLS* | *OLS* | *OLS* |
| Firm Fixed Effects? | Yes | No | Yes | No |
| State Fixed Effects? | No | Yes | No | Yes |
| Industry Fixed Effects? | No | Yes | No | Yes |
| Clustering | State-Year | Firm | State-Year | Firm |
| $R^2$ | 0.98 | 0.88 | 0.87 | 0.71 |

The regressions are estimated via ordinary least squares (OLS) or fixed effect (conditional) logistic regression (Logit), as described, with OLS $t$-statistics reported in parentheses using robust "sandwich" standard errors. Reported $R^2$ for Logit specifications is McFadden's pseudo $R^2$.

*,** Indicates significance at the 10% and 5% levels, respectively.

Table 10: Acquisitions

| **Dependent Variable** | Was acquired? | Was acquired? | Made acquisitions? | Made acquisitions? | Log of acq. size | Log of acq. size |
|---|---|---|---|---|---|---|
| *# Obs.* | *20,965* | *20,474* | *18,085* | *17,548* | *8,011* | *7,678* |
| Increased Enforceability | -2.8371 | | 0.3039* | | 0.3317** | |
| | (-0.89) | | (1.77) | | (2.44) | |
| Enforce*Assign*(In-state Comp.) | | 0.0637 | | | | |
| | | (1.49) | | | | |
| Enforce*(In-state Comp.) | | | | 0.2640 | | 2.9861** |
| | | | | (1.11) | | (2.87) |
| In-state Competition | | -0.0159 | | -0.0784 | | -1.0566** |
| | | (-1.12) | | (-0.95) | | (-2.89) |
| 1 year total return | -0.0003 | 0.0000 | 0.0000* | 0.0000** | 0.0000** | 0.0000** |
| | (-0.60) | (-1.25) | (-1.79) | (-3.65) | (-2.04) | (-3.15) |
| 3 year total return | -0.0419** | 0.0000 | 0.0047** | 0.0008** | 0.0029** | 0.0028** |
| | (-3.82) | (-0.75) | (4.52) | (4.67) | (3.32) | (2.64) |
| Log (Firm Age) | | -0.0009 | | -0.0507** | | -0.0984** |
| | | (-0.43) | | (-4.99) | | (-2.44) |
| Log (Book Value) | -0.2138 | 0.0001 | 0.1352* | 0.0381** | -0.2289** | -0.0128 |
| | (-0.28) | (0.03) | (1.72) | (3.07) | (-2.47) | (-0.19) |
| Log (Market Value) | 0.9725 | -0.0039* | 0.0461 | -0.0229** | 0.2227** | 0.1469** |
| | (0.94) | (-1.95) | (0.73) | (-2.56) | (4.03) | (3.23) |
| Log (Sales) | 1.6636 | 0.0041* | -0.3140** | 0.0415** | -0.7155** | -0.2025** |
| | (1.01) | (1.83) | (-3.21) | (3.54) | (-5.29) | (-3.50) |
| Log (Assets) | -2.1513 | -0.0023 | 1.4727** | 0.0266 | 1.8660** | 0.8994** |
| | (-0.98) | (-0.78) | (11.50) | (1.48) | (12.06) | (12.14) |
| Log (State Unemployment) | 12.7126** | 0.0127 | -0.0722 | -0.0766* | 0.0876 | 0.1374 |
| | (2.67) | (1.12) | (-0.28) | (-1.71) | (0.36) | (0.60) |
| Log (State Personal Income) | 159.1397** | 0.0334 | 2.8482** | -0.0783 | -0.2606 | -0.3534 |
| | (11.14) | (0.50) | (2.76) | (-0.30) | (-0.30) | (-0.25) |
| Estimation Method | *Logit* | *OLS* | *Logit* | *OLS* | *OLS* | *OLS* |
| Firm Fixed Effects? | Yes | No | Yes | No | Yes | No |
| State Fixed Effects? | No | Yes | No | Yes | No | Yes |
| Industry Fixed Effects? | No | Yes | No | Yes | No | Yes |
| Clustering | No | Firm | No | Firm | State-Year | Firm |
| $R^2$ | 0.90 | 0.05 | 0.28 | 0.26 | 0.60 | 0.45 |

The regressions are estimated via ordinary least squares (OLS) as described, with $t$-statistics reported in parentheses using robust "sandwich" standard errors.

*,** Indicates significance at the 10% and 5% levels, respectively.