Three Essays on Covenants Not to Compete

by

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DEDICATION

To my family.

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TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

CHAPTER I

Training the Enemy? Firm-Sponsored Training and the Enforcement of Covenants Not to Compete

1.1 Introduction

The poaching of employees presents a challenge for firms who wish to improve the skills of their workforce. Firms that fear a worker is likely to join a competitor may decide to provide that worker with less training, especially if it involves the transfer of valuable information such as client lists or trade secrets. Firms have found a contractual solution to this problem in the form of covenants not to compete (non-competes), which prevent the worker from joining or starting a competing firm for a fixed amount of time post separation. Non-competes are believed to be ubiquitous today, often standard in employment contracts for both CEOs and minimum wage workers, and represent the most litigated portion of employment contracts (Stone 2002).[1] Yet state courts vary significantly in the circumstances under which they will enforce them. For example, some states have a per se prohibition on enforcing non-competes, while other states enforce them even if the worker is fired.

This paper theoretically and empirically investigates the traditional argument that firms will invest more in their workers if their non-competes are more likely to be enforced. The

[1] See Section 2 for more details.

assumption underlying the presumed positive relationship between enforcement and training is that training is not contractible. When training is chosen in equilibrium as a result of firms competing for workers by offering contracts including both wages and training, then an increase in non-compete enforcement can reduce the amount of firm-sponsored training. In this scenario, non-compete enforcement intensity affects the amount of training chosen by the worker because it affects the likelihood he will be able to move to a competitor firm in the future. If the worker expects to move to a competitor in which his training is more valuable, then higher non-compete enforcement reduces the likelihood of that future movement, reducing the value of the training and causing the worker to select a contract with less training and more money upfront. Without knowing which of these two processes generates *observed* firm-sponsored training, the relationship between non-compete enforcement intensity and training is theoretically ambiguous.

The empirical relationship between non-compete enforcement and observed firm-sponsored training has never before been examined because of the difficulties involved in accurately quantifying the various dimensions of enforcement. I create an improved measure of enforcement which weighs six dimensions of enforcement recently quantified by Bishara (2011) by using confirmatory factor analysis. With this new index, I employ a difference-in-differences identification strategy which exploits the fact that only occupations present in litigation (high litigation) are subject to state enforcement schemes. In order to map occupations to high litigation and low litigation groups, I use the occupation distribution reported in two surveys of litigated non-compete cases (LaVan 2000, Whitmore 1990).[2] The estimates represent the causal, intent-to-treat effect of state non-compete enforcement, since the data does not contain information on which workers actually signed non-competes.

I find that a one standard deviation increase in a state's enforcement level increases the

[2]The high litigation group refers only to occupations which are present in litigation, regardless of whether the non-compete was ultimately enforced.

probability that the average high litigation occupation receives firm-sponsored training by 3% relative to low litigation occupations.[3] This estimate suggests that if California were to adopt Florida's laws, then high litigation occupations would receive a 16% increase in the likelihood of receiving firm-sponsored training. The relative, marginal effect rises monotonically between 3% and 8% in each of the first 20 years of tenure, and is between 2 and 7% for workers aged 22 to 42. Because training later in tenure is less likely to be contracted upon, the fact that the largest effects of enforcement on training appear for workers with 10-20 years of tenure suggests that the relevant model of training in that stage of tenure is the "not contractible" model. The "not-contractible" provides a clear role for non-compete enforcement because it improves training outcomes unambiguously by reducing the tendency of firms to underinvest in training.

Disaggregating the effect by occupation shows that relative to low litigation occupations, higher non-compete enforcement increases firm-sponsored training for primarily high skill and high earnings occupations such as managers, computer and mathematical occupations, and health practitioners, though personal care and services occupations are also strongly impacted by enforcement. I also find that the training effects coincide with an enforcement impact on the hiring margin: for some occupations, firms in lower enforcing states tend to hire more experienced workers, presumably because they are unwilling to bear their training costs.

Breaking the non-compete enforcement index into its individual components reveals that courts looking to improve training outcomes in their states should consider reducing the burden of proof on the plaintiff, or introducing policies which enforce non-competes only when workers are provided compensation beyond continued employment in exchange for signing. Additionally, policies which exploit the heterogeneity of the training impact, such

[3]The mean probability of receiving firm-sponsored training in the last year is 0.23 for high litigation occupations and 0.13 for low litigation occupations.

as Colorado's enforcement only for upper level management, are well-suited to extract the training benefits without adversely affecting occupations which receive little or no relative training benefits from increased enforcement.

This paper contributes to the training literature by adding non-compete enforcement to the labor market frictions which lead to firm-sponsored training and the nascent empirical literature on the welfare effects of non-competes. There is a growing reluctance towards the enforcement of these agreements (Hyde 2003, Lobel 2013) because of the negative impacts on worker mobility (Marx et al. 2009, Garmaise 2011, Lavetti et al. 2011) and on new venture creation (Samila and Sorenson 2011), but few studies have empirically examined to what extent firms and workers actually benefit from the protection offered by enforcement. Lavetti et al. (2011) find that physicians who sign non-competes tend to earn 11% more because they are allocated more clients, while Marx and Younge (2013) find that Tobin's q increased by 9.75% after non-competes became enforceable in Michigan. My results contribute to this line of inquiry by estimating an important parameter necessary to understand the overall welfare effects: at least for some high skill occupations firms are indeed responding to the increased protection of their confidential information by providing more training to their employees.

The rest of the paper is organized as follows: Section 2 describes non-competes and how enforcement is quantified and Section 3 reviews the relevant training literature. Section 4 extends the classic two-period training model to include non-compete enforcement and a poaching stage. Section 5 introduces the data and the identification strategy. Section 6 discusses the results and robustness checks, and Section 7 concludes.

1.2 Non-Competes

1.2.1 The Incidence of Non-Competes

Legal scholars claim that non-competes are ubiquitous, but there is little evidence to justify this claim for a broad array of occupations (Stone 2002). Previous studies find that about 80% of CEOs sign non-competes (Bishara et al. 2012, Garmaise 2011), 45% of physicians (Lavetti et al. 2011), 40% of engineers (Marx 2011), and 70% of entrepreneurs with venture capital contracts (Kaplan and Stromberg 2002). Galle and Koen (2000) survey practicing human resource professionals and find that of the 123 returned surveys (12.3% response rate), 55% of firms used non-competes. The authors did not investigate which occupations within the firm were asked to sign non-competes. While the incidence of non-competes in other, low-skill occupations is generally unknown, Stone (2002) reports that non-compete cases have been litigated against manicurists, carpet installers, liquor deliverymen, bartenders, cosmetologists, pest exterminators, garbage collectors, janitors, night-watchmen, undertakers, and security guards. Together this evidence shows that non-competes are an important, potentially standard, part of employment contracts today.

1.2.2 Non-Competes in Practice

Employees tend to sign covenants not to compete on the first day of their new job, or soon after (Marx 2011). These agreements typically stipulate that upon separation from the employer the employee cannot work for a competitor, or start a competing business, for a certain amount of time and in a specified geographic region. Common time restrictions are one to three years (Bishara et al. 2012), and geographic restrictions vary by industry. In highly localized markets, such as the market for hairdressers, the geographic region specified in the contract may be the county, or a number of miles from the places of business. In national markets, the contract may restrict the worker from working anywhere in the country. Upon violating the terms of the contract, a number of steps must be taken by the prior employer in order for the worker to be prevented from actually working for the competitor. The prior employer must first learn of the violation, then it must choose to file suit in court. When the case reaches court, the prior employer usually seeks a preliminary injunction, which will prevent the employee from working for the competitor until the judge determines whether or not he will enforce the employee's non-compete. Non-competes are considered common law and are decided by judges based on state statutes or case law precedents.[4] In 2012 there were 742 reported, litigated non-compete cases (Beck 2013). This number is an underestimate of the vastness of the impact of non-competes, however, because most cases settle out of court, and many workers may take career detours to explicitly avoid potential litigation (Marx 2011).

1.2.3 Quantifying Non-Compete Enforcement

While some states, such as California and North Dakota, refuse to enforce non-competes, most states will enforce them by implementing their own version of the 'reasonableness doctrine,'[5] which balances the protection necessary for the firm with the injury to the worker and society. Among enforcing states there is unanimous agreement that a necessary condition for the enforcement of a non-compete is that the worker possesses some kind of valuable information, called 'protectable interests,' in which the firm has made a significant investment it seeks to protect, such as trade secrets, client lists, and other confidential information which gains value from not being publicly known. Some states, such as Florida and Kentucky,

[4] Interjurisdictional issues regarding non-compete enforcement can be quite complex. See Glynn (2008) for a discussion on choices of law and forum and conflict of law. See also *Advanced Bionics Corp.* v. *Medtronic, Inc.* 59 P.3d 231, 238 (California 2002) for a complicated case.

[5] See Appendix 1.8.1 for a brief review of the legal literature on non-compete enforcement. See Blake (1960) for an in-depth review of the history of non-compete enforcement.

include extraordinary general skills training in this list of protectable interests, but traditionally it has been omitted. Regardless of whether general training is itself a protectable interest, however, the training level a firm chooses for its employees is closely related to the traditional protectable interests: Once an employee under an enforceable non-compete is exposed to the firm's secret formula, client lists, advertising strategies, or other confidential information, the employee is bonded to the firm by the non-compete and the firm has the same increased incentives to invest in the worker. Those further investments in training may include learning more trade secrets and confidential information, but it is the first exposure to confidential information that counts.[6]

Even after courts identify whether the worker possesses a trade secret or has access to client lists, significant variation remains in how states perceive reasonableness or respond to the unreasonableness of various other dimensions of the case. For example, some states will only enforce a worker's non-compete if the worker voluntarily quits, while others will enforce it even if the worker is fired. State courts also vary in the manner in which they handle unreasonably overbroad covenants. Most states will rewrite overbroad non-competes to be more reasonable and subsequently enforce them. Other states, notably Wisconsin, will throw out the entire contract if it is overbroad. States also have different enforcement protocols for whether the non-compete was signed after the employment relationship began or after a promotion. In Oregon, for example, firms have to notify prospective employees that they will be asked to sign a non-compete two weeks before employment commences. Colorado is particularly unique in that it will only enforce non-competes for workers in

[6]There exists a debate in the legal literature about whether general training should be a protectable interest. The arguments hinge on whether or not the worker is able to stay at the firm long enough to pay back the training costs borne by the firm. If the worker leaves too soon, the firm cannot capture enough of the return to training to cover the cost (Lester 2001). On the other hand, if the worker leaves long after he has repaid his training cost, it seems unfair to restrict his post-employment options by enforcing his non-compete (Long 2005). As a result of this debate, many legal scholars advocate the use of training recoupment contracts such that if the worker leaves too soon he must pay back damages to the firm (Von Bergen and Mawer 2007).

upper management. Massachusetts is currently considering a law in which it would consider durations under 6 months reasonable, but if the worker earns over \$250,000 the court might allow longer durations.[7]

Malsberger tracks these and other dimensions of enforcement in his volume *Covenants Not to Compete: A State-by-State Survey.* Bishara (2011) reviews Malsberger's texts and assigns each state a score between 0 to 10 on seven dimensions of non-compete enforcement for 2009 and 1991.[8] He aggregates the individual dimensions into a single index using his own subjective weights. I improve upon Bishara's weighting scheme by using confirmatory factor analysis (CFA) on his seven scores to generate weights for each dimension. The benefits of incorporating each dimension into a single index as opposed to considering the impact of each component individually are twofold: (1) Since the standard errors of my estimates will be clustered at the state level, worries about micronumerosity[9] increase as the number of state-level regressors increases[10] and (2) if each dimension of enforcement is considered a measurement error ridden proxy for latent non-compete enforcement intensity, then combining the measures into a single index reduces attenuation bias.[11] Due to the highly correlated nature of the individual dimensions of enforcement, however, all weighting schemes which give non-negative weights to each dimension result in highly correlated aggregate indices. Confirmatory factor analysis as a reweighting tool is therefore a modest improvement.

Factor analysis postulates that each particular dimension of enforcement depends linearly upon latent enforcement intensity. Defining x_{is} as enforcement dimension i for state s and

[7] See generally Malsberger (1996) and later editions.

[8] A complete explanation of Bishara's (2011) scoring method is available in Appendix 1.8.3.

[9] See Goldberger (1991).

[10] I run a specification with each dimension entered linearly in Section 6.

[11] Lubotsky and Wittenberg (2006) show that including the individual measures in the baseline regression specification and then using the coefficients on the individual dimensions as weights in the aggregation into a single index is the best way to reduce measurement error. Their method generates different weights with different dependent variables, which is unappealing in this context. Regardless, their method of aggregation will be utilized as a robustness check.

$Enfc_s$ as latent enforcement intensity, the model is defined by the set of equations

$$x_{is} = \lambda_i Enfc_s + \epsilon_{is} \quad \text{for } i = 1, 2...6,$$

where ϵ_{is} is measurement error. It is assumed that $E[\epsilon_{is}] = 0$, $E[\epsilon_{is}^2 = \sigma_i]$, $E[\epsilon_{is}\epsilon_{js}] = 0$ for all $i \neq j$, $E[\epsilon_{is}\epsilon_{ik}] = 0$ for all $s \neq k$. Under the assumption that $\lambda_1 = 1$, the correlation matrix identifies the other λ_i terms because $corr(x_i, x_j) = \lambda_i\lambda_j$. The latent enforcement scores are generated by taking the parameter estimates and minimizing the sum of squared deviations of the latent enforcement factor from its true value.[12] The enforcement index is normalized to have a mean of zero and a standard deviation of one in a sample where each state is given equal weight. Table 2.15 reports the mean, standard deviation, weight of each dimension of enforcement for 1991 and 2009 from Bishara (2011) and the resulting weights from the factor analysis.

Table 1.1: Factor Analysis Index

	1991			2009			
Question	Mean	SD	FA Weight	Mean	SD	FA Weight	Bishara Weight
Statute of Enforceability	4.90	1.53	0.07	4.96	1.79	0.09	0.10
Protectable Interest	5.80	2.03	0.07	0.07	1.93	0.21	0.10
Plaintiff's Burden of Proof	5.36	2.06	0.06	5.59	1.93	0.13	0.10
Consideration At Inception	8.45	2.35	0.22	8.73	2.39	0.07	0.05
Consideration Post Inception	7.04	2.78	0.09	7.15	2.86	0.05	0.05
Overbroad Contracts	5.71	3.07	0.04	5.83	2.91	0.03	0.05
Quit v. Fire	6.23	2.32	0.07	6.45	2.37	0.07	0.10

Factor analysis yields a relatively consistent picture of the dimensions which characterize a state's intensity of enforcement. Indeed the correlation between the 1991 and 2009 scores is 0.94 and the correlations with the initial Bishara index are greater than or equal to 0.93. In 2009, the most important factors are whether a state has a statute of enforceability, what

[12]See Kolenikov (2009) for CFA details, Harman (1976) for further details on exploratory factor analysis. See Black and Smith (2006) for an example of using factor analysis to generate an index of college quality.

constitutes a protectable interest, and the extent of the burden of proof on the plaintiff. In 1991, the dimension which receives most of the weight is whether or not non-competes are enforceable if the worker only receives continued employment in exchange for signing.

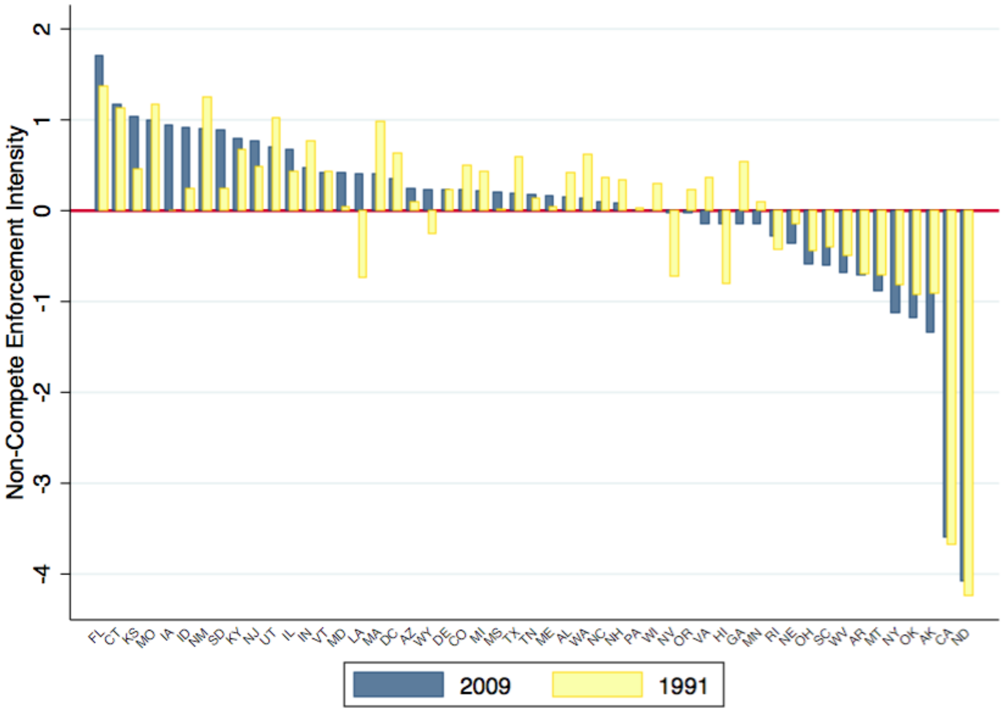
Using the 2009 weights above, I present the non-compete enforcement score for each state in Figure 1.2.1. As expected, California and North Dakota have the lowest scores. The highest scores belong to Florida and Connecticut. Overall, the variation across states is large both in levels and relative to the within-state variation over time.[13] Enforcement intensity is not correlated with a state's political leanings (Lavetti et al. 2011) and does not appear to be clustered geographically.[14]

While non-compete enforcement is relatively consistent across time, the fact that the training data comes from 1996, 2001, 2004, and 2008 raises concerns that state laws may have adjusted between the ends of the time horizon. Indeed, the only change occurred in Louisiana, which had an initial reversal in mid-2001 and then reverted back to pre-2001 enforcement levels in 2003. This reversal period is unlikely to affect my estimates because (1) the affected number of workers is very small (only 104 workers in the final sample of 70,374), and (2) the survey asks about training during the past year, while workers were surveyed only two months into the reversal. To account for any variation in changes over time, I assign data from 1996 the 1991 enforcement scores, while the rest of the years receive the 2009 enforcement score. Additionally, the results I present will use the 2009 weighting scheme above. The results are robust to using the 2009 scores, the 1991 scores, the weights from the other year's factor analysis results, the initial Bishara index, and an index constructed using the

[13]There are three reasons why there might be differences between the 1991 and 2009 scores: (1) New cases or statutes caused changes in state laws; (2) The factor analysis weights for 1991 and 2009 are different, causing the weighted index to differ between the two years even for the same scores; (3) Many states had not established firm policies in 1991 with regards to some of the dimensions and therefore have missing information. These missing values are imputed based on the state's average non-missing score. If by 2009 the court had determined an outcome, it may differ from the imputed value. I run numerous robustness checks for different sets of years and weights to verify that these differences do not drive my results.

[14]See the map in Appendix 1.8.4.

Figure 1.2.1: Factor Analysis Enforcement Index for 2009 and 1991



Lubotsky-Wittenberg method.

1.3 Training Literature

Becker's classic theory of general human capital argues that as the sole beneficiaries of general human capital, workers should bear the cost of its acquisition. Contrary to this theory, many papers find that firms indeed pay for what appears to be general training (see Bishop 1991 for a survey) and workers do not take commensurate wage cuts (Barron et al. 1999 and others) to pay for it.[15] Acemoglu and Pischke (1999) show that wage compression, when wages rise less than productivity with training, incentivizes firms to invest in general on-the-job training. They demonstrate that many plausible market failures including general

[15] Endogeneity concerns remain, however, since it is difficult to control for the fact that unobservably higher skilled workers sort into higher wage jobs that might require more training.

and specific complementarities in production, minimum wage laws, adverse selection, and search frictions generate wage compression and thus encourage firms to provide training. Recent work on why firms pay for training have focused on specific market failures which lead to monopsony power for the firm, such as technological complementarities (Acemoglu and Pischke 1998), the "thinness" of labor markets (Muehlemann et al. 2012), asymmetric information (Autor 2001, Stevens 1994), search frictions (Asa and Moen 2004), and moving costs (Katz and Ziderman 1990, Benson 2013).[16]

One key feature of the Becker model is that when training is general and the labor market is perfectly competitive the resulting training level is efficient. Without strong evidence suggesting workers pay for their on-the-job training, economists and policymakers have been concerned with the potential underprovision of employee training. To identify whether or not there is a market failure in training, the traditional approach has been to compare training levels across countries (Acemoglu and Pischke 1999 review this literature). For instance, Harhoff and Kane (1997) provide evidence that the institutional structure of the German labor market makes it less likely that workers trained within the firm will leave to work for other employers. The mobility of US workers is one of the primary reasons firms are likely to provide less than the efficient level of training (Bishop 1991).

This paper considers whether the enforcement of covenants not to compete, which inhibits employee movement to competitor firms, increases the firm's willingness to provide training. The relationship between firm-sponsored training and non-compete enforcement was first noted by Rubin and Shedd (1981). They argue that while non-competes have no role in perfectly competitive labor markets where training is either perfectly general or specific, an alternative scenario arises, when the worker is credit constrained and cannot pay for his training, which is likely to be the case when part of the firm-sponsored training involves

[16] For a nice summary on monopsony in the labor market, see Manning (2003).

sharing sensitive, confidential information. In this situation, the firm would want the worker to sign an enforceable non-compete agreement to prevent the worker from appropriating the value of the training, for which he did not pay, elsewhere. If the firm can prevent the worker from leaving, then it has the proper incentives to invest in the training and the information in the first place. Even without credit constraints, however, if workers are unable to pay for informal or otherwise unanticipated training, then the enforcement of non-competes provides the proper incentives for the firm to invest.

Hyde (2003) is the only legal scholar I am aware of who presents alternatives to this perspective. Hyde argues that if the firm wishes to limit employee turnover, it can utilize mechanisms other than the non-compete contract, such as delayed compensation, steep wage profiles, and vesting requirements for retirement packages. If the firm is primarily worried about the transmission of trade secrets and confidential information for which competitors would pay dearly, however, these other contractual mechanisms may be less useful.

In addition to the contractibility of training, which I examine in detail in section 4, I propose two other potential reasons why non-compete enforcement may not impact training choices. First, employees who sign non-competes are subject to the *in terrorem* effect, which refers to the idea that a worker who has signed a non-compete might obey it because he believes it to be enforceable, or because he feels ethically bound by it, despite the actual enforceability of the contract. The magnitude of this effect is largely unknown, though Marx et al. (2011) provides the first evidence that 30% of engineers who signed a non-compete and later quit took career detour and wage cuts to avoid potential litigation. If employees believe their non-compete to be enforceable, or abide by it for whatever reason, then whether or not the courts will actually enforce it is inconsequential. As a result, firms in high enforcing and low enforcing states are likely to invest similarly in their employee human capital.

Second, even if firms use non-competes in their employment contracts, they must actually choose to enforce them in order to deter workers from quitting for competitors in equilibrium. Choosing to enforce these agreements, however can be costly. Hyde (2003) provides anecdotal evidence that some firms have reputations for enforcing non-competes against departing employees and that experienced, potential employees are less willing to work for them. Thus there may be an equilibrium in which firms choose not to enforce their workers's non-competes because of the potential for greater recruiting costs.

The theory developed in Section 4 is most similar to Posner, Triantis, and Triantis (2004), who also present a model of non-compete enforcement that explores the tension between human capital investment and employee mobility.[17] Their focus on the contracting behavior of the worker and firm leads them to consider three remedies if the contract is breached: specific performance (forcing the worker to stay at the firm), liquidated damages (the worker pays the firm if he leaves), and injunctive relief (preventing the worker from joining the other firm). They consider both when the contract is renegotiable and not, finding that when the contract is renegotiable, the firm and worker can sign a contract that will induce both ex post and ex ante efficiency. When contracts are not renegotiable, however, non-compete enforcement represents a hybrid between specific enforcement for movements within its scope and zero liquidated damages for movement outside its scope. Their suggestion to courts is that non-competes appropriate in scope should be enforced, but in cases where renegotiation is possible courts should be worried about the tendency to try to extract rents from new entrants. While their model clarifies the relationship between an injunction required by a non-compete and alternative breach remedies, their model makes two assumptions which omit important scenarios: (1) They assume that the worker is most productive in his initial firm, which precludes the possibility that mobility is welfare enhancing; (2) they do not allow for the contractibility of training, but instead assume that firms make incentive compatible, unilateral investment decisions, which generates the commonly assumed result that

[17] See Leuven 2005 for a survey of classic private sector training models.

higher non-compete enforcement increases firm-sponsored investment. The model presented in Section 4 considers the role of these additional issues.

1.4 A Theory of Non-Compete Enforcement

The training model presented in this section is a partial equilibrium, simplified search model that abstracts away from the many legal complications that arise in specific contexts. It is a search model in the sense that the worker only meets a subset of firms in the first period, but in a later period meets a firm at which he is differentially productive; in this sense, mobility can be welfare enhancing or reducing. The model is not intended to provide a complete welfare analysis of non-compete enforcement. The benefit of these abstractions, however, is the clear intuition developed. The central takeaways from the model are: (1) if training is part of the employment contract (contractible) then competition internalizes the training externality and increases in non-compete enforcement may or may not increase firm-sponsored training levels, (2) if training is not contractible then higher non-compete enforcement increases the firm's willingness to provide training, (3) training is higher in the contractible case even when non-compete enforcement is optimally chosen, (4) workers who carry with them valuable information that could damage a previous employer have a greater likelihood of making an inefficient quit,[18] and (5) actual enforcement policy is irrelevant if workers believe those contracts to be enforceable or feel ethically bound to abide by them.

1.4.1 Model Setup

The goals of the model are to formalize the tension between human capital investment and worker mobility, to understand the assumptions underlying the positive relationships between

[18] By inefficient quit I am referring to the case in which a worker moves to a firm in which he is less productive but receives a higher wage.

non-compete enforcement and firm-sponsored training, and to characterize the optimal non-compete enforcement levels given two training generating processes. The model generally follows the full-competition and constrained regimes laid out in the Acemoglu and Pischke (1999) training models. The baseline assumptions are: (1) In any training that occurs, the worker absorbs some trade secrets or other confidential information that would be defined as a protectable interest of the firm, thereby making non-compete enforcement applicable, (2) all training is considered general training, and (3) the worker does not at any point renegotiate with his employer.[19]

The model consists of three stages, a hiring stage, a training stage, and a poaching stage. I consider two cases in the hiring stage in which a risk neutral worker looks for employment. In the first case, denoted the "contractible" case, training is assumed to be contractible and identical firms compete to hire the worker by offering wage and training contracts, denoted $\{W, T\}$, where W refers to the worker's wage in the training stage, and T corresponds to the amount of training the worker will receive in the training stage. In the second case, denoted the "not contractible" case, firms compete on training period wages but cannot or do not compete on either training or post-training wages.[20] In the not contractible case, training is unilaterally chosen by the firm to maximize profits. Whether or not training is contractible, it is assumed that the worker signs and bargains over a non-compete at the start of the employment relationship.[21] After joining the firm but before the worker enters the poaching

[19]One rationale for assumption (3) is that transaction costs are high. Another, as Moscarini (2008) suggests, is that by committing to a no-renegotiating stance, the firm perpetuates a coordination failure among employees: If employees coordinated and all were able to procure alternative job offers, the firm would have no choice but to renegotiate employment contracts to maintain the business. By choosing to not renogotiate contracts, the firm discourages such coordination among its employees. There is some evidence that upper management workers renegotiate their contracts, see Lublin (2013).

[20]Allowing competition on post-training wages yields equivalent results to allowing competition on training because there is a one to one mapping between training and post-training wages. In both cases, competition for the worker bids up wages until total expected profit from hiring the worker is zero.

[21]Firms of course have the option of not using non-competes. In the contractible case, the option to work for a firm without a non-compete creates a discontinuity in the contract space, in which only workers with an extreme preference for mobility choose to work for a firm without a non-compete. Workers who choose

stage, the worker bargains over his post-training wage, $w(T)$, based on his expected outside option from quitting, $\mathbf{E}[v(T)]$, with bargaining weight β.

The hiring stage is divided by the contractibility of training for four reasons: (1) As Capelli (1999) notes, workers want their jobs to provide them with future "employability," including relevant experience or training. This 'new employment relationship' is likely to result in training becoming either an explicit or implicit (enforceable by the reputation of the employer) part of the employment contract. (2) As noted by Barron et al. (1999), most training is informal, and this type of training is by its very nature not contractible. (3) The not contractible case reflects three important situations. First, training might not be contractible because contracting over every conceivable contingency is infeasible. Second, it may be that all employers are ex-ante unwilling to commit to providing (at least some) training for the worker because of uncertainty about the worker's ability or the match quality. Third, the worker may already be employed by a firm and his contractible training, the level of which any other firm would have been willing to supply, has already been provided, but the firm, because of a positive demand shock, decides to provide additional training to the worker. (4) Lastly, distinguishing the effects of non-compete enforcement when training is and is not contractible may provide courts with clear policy alternatives. Oregon has passed, and Massachusetts is currently considering, a law which require firms to notify workers that they will ask them to sign a non-compete at least 2 weeks in advance of the commencement of the employment relationship. To the extent that these kinds of laws encourage myopic workers to negotiate over training that they would not have otherwise negotiated over, they will encourage the contracting of training.

In the training period, the firm trains the worker as specified in the training contract (in the

this option receive a lower wage and less training, since non-competes generate second period rents but competition forces firms to pass along the rents to the worker.

contractible case) or chooses the level optimally (in the not contractible case).[22] The worker's production function is given by $y(T)$, which is assumed to be increasing and concave,[23] while the cost of training, $c(T)$, is increasing and convex.[24]

In the poaching stage, the now trained worker meets another firm, or equivalently, mulls starting his own business. He observes a wage offer equal to his productivity at the new competitor firm, defined as $ay(T)$, where a is a random variable with cumulative distribution function $G(a)$ on $[0, \bar{a}]$, where $\bar{a}$ represents the upper limit of the support of a. If the worker decides to stay, then he earns $w(T)$ and produces $y(T)$ at the initial firm. If the worker decides to quit, then the worker's non-compete is enforced with probability λ. The worker's expected wage from quitting is $(1-\lambda)ay(T)$, where the worker is assumed to earn nothing if his non-compete is enforced.[25]

1.4.2 Solving the Model

I solve the model via backwards induction, starting with the worker's quit decision. The worker quits if his expected pay at the competitor firm exceeds his bargained wage at the incumbent firm:

$$(1-\lambda)ay(T) > w(T) \tag{1.4.1}$$

[22] While both models assume a single training period in the beginning of the worker's tenure, in the data training occurs throughout the life of the worker. This consideration can be incorporated into the theory by repurposing the not contractible case. Suppose the firm "wakes up" to find it has a worker with training level T_0 and wage w_0. The firm then unilaterally makes an incentive compatible decision to upgrade the worker's training to T_1. After the worker receives the additional training, the poaching phase commences. This scenario is identical to the not contractible case with $w_0 = 0$ and $T_0 = 0$.

[23] Formally, $y(T)$ satisfies $y(0) = 0$, $y'(T) > 0$, $y'(0) = \infty$, and $y''(T) < 0$.

[24] Formally, $c'(T) > 0$ if $T > 0$, $c'(0) = 0$, and $c''(T) > 0$.

[25] There are two ways to think about non-compete enforcement in this context: (1) as mentioned above, λ is the probability that the worker's non-compete is enforced if he quits. (2) Alternatively, one can think of λ as the percentage of time in the poaching period that the worker will be prevented from working for the competitor firm. Since the goal is not to provide a complete welfare evaluation, but instead to understand the relationship between enforcement, training choices, and labor market competition, the exact interpretation of λ is left unspecified.

The worker's post-training wages, $w(T)$, are determined in a full information Nash bargain after the worker is hired but before he meets a firm in the poaching stage. The only uncertainty is over the type of firm the worker will meet in the poaching stage. The worker's expected outside option from quitting at the time the wage is bargained is $\mathbf{E}[v(T)] = (1-\lambda)\mathbf{E}[a]y(T)$. His bargained wage solves $w(T) = \mathbf{E}[v(T)] + \beta(y(T) - \mathbf{E}[v(T)])$, which simplifies to:

$$w(T) = y(T)\left[\beta + (1-\beta)(1-\lambda)\mathbf{E}[a]\right] \tag{1.4.2}$$

Condition (1.4.2) shows that non-compete enforcement causes wage compression, which Acemoglu and Pischke (1999) identify as the key to incentivizing the firm to pay for general training. Formally, differentiating $\frac{w(T)}{y(T)}$ with respect to λ yields:

$$\frac{\partial \frac{w(T)}{y(T)}}{\partial \lambda} = -(1-\beta)\mathbf{E}[a] < 0, \;\; \forall\, \beta \in \; [0,1)$$

Increases in λ result in the worker receiving a smaller fraction of his output because the firm does not have to fully compensate him for his outside options.[26]

Substituting the wage from (1.4.2) back into the quit equation from (1.4.1) gives the quit decision as a function of exogenous variables:

$$a > \hat{a}(\lambda) \equiv \frac{\beta}{1-\lambda} + (1-\beta)\mathbf{E}[a] \tag{1.4.3}$$

Given the threshold value of $\hat{a}(\lambda)$, the probability of a quit can be neatly summarized by:

$$P(a > \hat{a}(\lambda)) = 1 - G(\hat{a}(\lambda)) \tag{1.4.4}$$

[26] A prerequisite condition for the initial firm employing the worker in the poaching period is that it must make weakly positive profits by employing the worker, $w(T) \leq y(T)$. This results in a limit on how big $\mathbf{E}[a]$ can be: $\mathbf{E}[a] \leq \frac{1}{1-\lambda}$.

Note that increases in enforcement increase the threshold quitting productivity and thus make the worker less likely to quit.

Case 1: Contractible Training

In this case, firms compete for the worker by offering wage contracts of the form $\{W, T\}$ and the initial firm requires the worker to sign a non-compete. Assuming that the worker is equally valuable to all firms in this stage, competition ensures that firms earn zero expected profits. The set of $\{W, T\}$ such that the firm earns zero profits is given by $W = G(\hat{a})(y(T) - w(T)) - c(T)$. Given the zero expected profits condition, the worker chooses the utility maximizing $\{W, T\}$ contract. Formally, the problem the risk neutral worker faces is:

$$\begin{aligned} \max_{T,W} \quad U(W,T) &= W + G(\hat{a})w(T) + (1 - G(\hat{a}))(1 - \lambda)\mathbf{E}[a|a > \hat{a}]y(T) \\ s.t. \quad W &= G(\hat{a})(y(T) - w(T)) - c(T) \end{aligned}$$

Substituting for W from the firm's zero profit constraint into the worker's maximization problem gives:

$$\max_{T} U(T) = G(\hat{a})y(T) + (1 - G(\hat{a}))(1 - \lambda)\mathbf{E}[a|a > \hat{a}]y(T) - c(T) \qquad (1.4.5)$$

The zero profit condition and resulting indifference between zero expected profit wage-training contracts turns the worker's optimal contract choice problem into a problem of joint surplus maximization.[27] If the worker stays, $y(T)$ is produced and if the worker leaves then expected production is $(1 - \lambda)\mathbf{E}[a|a > \hat{a}]y(T)$. Simplifying the objective function by

[27] The positive training externality is internalized to the extent that the worker earns his full marginal product at the competitor.

incorporating the fact that:

$$\mathbf{E}[a|a>\hat{a}] = \frac{\int_{\hat{a}}^{\bar{a}} ag(a)da}{1-G(\hat{a})}$$

and taking the derivative with respect to T from (1.4.5) yields the first order condition for the optimal training level $T_c^*(\lambda)$ of the contract selected by the worker:

$$y'(T_c^*(\lambda))\left(G(\hat{a}) + (1-\lambda)\int_{\hat{a}}^{\bar{a}} ag(a)da\right) = c'(T_c^*(\lambda)) \tag{1.4.6}$$

Whether increases in non-compete enforcement induce more training is unclear. Totally differentiating (1.4.6) with respect to non-compete enforcement and using Leibniz' rule gives:

$$\frac{\partial T_c^*(\lambda)}{\partial \lambda} = \frac{y'(T_c^*(\lambda))\left(g(\hat{a})\frac{\beta}{(1-\lambda)^2}(1-(1-\lambda)\hat{a}) - \int_{\hat{a}}^{\bar{a}} ag(a)da\right)}{c''(T_c^*(\lambda)) - y''(T_c^*(\lambda))\left(G(\hat{a}) + (1-\lambda)\int_{\hat{a}}^{\bar{a}} ag(a)da\right)} \tag{1.4.7}$$

The denominator is clearly positive by the concavity and convexity of the production and cost functions, but the numerator reflects the indeterminate nature of the relationship. Increasing non-compete enforcement increases the probability that the worker stays at the current firm, which increases training when the likelihood of an inefficient quit is high enough. When the likelihood of an efficient quit is high, however, then increasing non-compete enforcement prevents the worker from moving, which reduces the benefit from investing in training. Intuitively, this situation arises when a worker knows he might be more productive at another firm, and would have chosen a contract with more training if he knew he could eventually move to the more productive firm, but due to the potential enforcement of his non-compete he instead chooses a contract with less training and a wage increase.[28]

[28] The assumption that the worker is only trained once may appear limiting here. But note that if the poaching firm was also allowed to train the worker then increasing non-compete enforcement may delay and possibly prevent the move to the more productive firm in the first place.

Sharing the Cost of Training

The first period payment W reflects the profit the firm would have gained in the second period if competition had not forced the firm to pay it to the worker. This second period monopsony power is derived from two sources: (1) The assumption of stochastic, productive heterogeneity in the competitor firm in the poaching period, which remains regardless of the non-compete enforcement level, and (2) non-compete enforcement which reduces the outside option, compresses the wage structure, and reduces the probability of a quit. This wage is given by the zero profit constraint evaluated at the chosen training level:

$$W_c^*(\lambda) = G(\hat{a}(\lambda))y(T_c^*(\lambda))(1-\beta)\Bigg(1-(1-\lambda)\mathbf{E}[a]\Bigg) - c(T_c^*(\lambda)) \tag{1.4.8}$$

In the case where the worker will certainly leave in the poaching period, $G(\hat{a}) = 0$, the worker is left to pay entirely for his training, $W_c^* = -c(T_c^*(\lambda))$. The worker also pays for all the training if he starts in the average firm, $\mathbf{E}[a] = 1$, and non-competes are not enforced, $\lambda = 0$. If there is perfect non-compete enforcement, $\lambda = 1$, then the worker is paid $W_c^* = y(T_c^*(1))(1-\beta) - c(T_c^*(1))$.

From (1.4.8), there are three effects of increased non-compete enforcement on the firm's willingness to pay for training, $c(T_c^*(\lambda)) + W_c^*(\lambda)$: (1) The increase in the probability the worker will stay with the firm, $G(\hat{a}(\lambda))$, (2) the increase in profits from paying the worker less, $(1-(1-\lambda)\mathbf{E}[a])$,(3) the change in profitability from the amount of training the worker receives, $y(T_c^*(\lambda))$. Whether increases in non-compete enforcement result in increases in the amount of training paid for by the firm depends upon on the size and magnitude of the third effect. If increases in enforcement increase training, then the firm is indeed willing to pay more for that training. If increases in non-compete enforcement reduce the optimal training level, then the firm will pay less for the training if the third effect dominates the other two.

In order to see a simple example in which more enforcement reduces the willingness of the firm to pay for training, imagine that there exist only two types of firms, $a \in \{1, 2\}$, where half of the firms have productivity $a = 1$. Assuming that $\beta = 0.5$ and that $\lambda = 0.5$, then $\mathbf{E}[a] = 1.5$, $\hat{a} = 1.75$, and $G(\hat{a}) = 0.5$. In this case, it is straightforward to show via equation (1.4.7) that an increase in non-compete enforcement reduces training. Intuitively, because a marginal increase in λ will not reduce the probability of a quit and will also not reduce the wage the firm has to pay because the distribution of potential poaching firms is unchanged, then the only impact of the increased non-compete enforcement is to reduce the amount of training chosen, which reduces the firm's contribution to training.

Suppose that either the worker is credit constrained or the firm is bound by minimum wage laws such that the starting wage must be above some lower bound, $W \geq \underline{W}$. If the worker cannot pay for his portion of the training, then do increases in non-compete enforcement increase the firm's willingness to pay for training? The answer is yes, as long as the increase in non-compete enforcement does not result in training falling by so much that the credit constraint unbinds. Intuitively, as long as the worker's first period wages are fixed at $\underline{W}$, then increases in non-compete enforcement increase the firm's second period monopsony power, leading it to pay more for training.

Case 2: Training Not Contractible

In the case where firms do not, or cannot, compete over the training the worker will receive, they are left to compete on first period wages.[29] Because an untrained worker's marginal product is assumed to be zero, y(0)=0, competition forces wages to the level of the worker's

[29] If firms could compete over post-training wages in addition to pre-training wages, then competition among employers would reduce the total expected profit from hiring the worker to zero, which is identical to the contractible case. Restricting wage competition to only the training stage results in zero profits only in the training period.

marginal product yielding a starting wage of $W = 0$.[30] With regards to paying for training, I assume that in this case the firm must bear all the training costs. This assumption is justified in two ways: (1) Because training is not contractible, the firm, not the worker, must choose the incentive compatible amount of training it wishes to supply, and (2) while the worker can offer to pay for training by taking a wage cut, as long as worker and firm contributions to training are perfect substitutes then in the Nash equilibrium only one party will pay for all the training (see Acemoglu and Pischke 1999).

Under these assumptions, the employer's problem is given by:

$$\max_T \mathbf{E}[\pi(T)] = G(\hat{a})(y(T) - w(T)) - c(T)$$

Plugging in for the value of $w(T)$ from (1.4.2) and solving for the optimal training level, $T^*_{nc}(\lambda)$, gives:

$$G(\hat{a})y'(T^*_{nc}(\lambda))(1-\beta)(1-(1-\lambda)\mathbf{E}[a]) = c'(T^*_{nc}(\lambda)) \tag{1.4.9}$$

Using the implicit function theorem, the partial derivative of the optimal training choice with respect to non-compete enforcement is given by:

$$\frac{\partial T^*_{nc}(\lambda)}{\partial \lambda} = \frac{g(\hat{a})\frac{\beta}{(1-\lambda)^2}y'(T^*_{nc}(\lambda))(1-\beta)(1-(1-\lambda)\mathbf{E}[a]) + G(\hat{a})y'(T^*_{nc}(\lambda))(1-\beta)\mathbf{E}[a]}{c''(T^*_{nc}(\lambda)) - G(\hat{a})y''(T^*_{nc}(\lambda))(1-\beta)(1-(1-\lambda)\mathbf{E}[a])} > 0$$

In this case, non-compete enforcement has an unambiguously positive effect on the amount of training undertaken for two reasons: (1) It reduces the chance the worker quits, and (2) it reduces the wage the firm must pay the worker because his outside options are limited.

Comparing the training outcomes from the two cases leads to the following proposition.

Proposition 1. *For a given enforcement level, λ, optimal training levels are higher when*

[30] Assuming $y(0) > 0$ does not substantively change any analysis. If this were the case, competition simply bids up his wage to $W = y(0)$ and does not affect any subsequent training decisions.

training is contractible at the hiring stage, $T_c^*(\lambda) > T_{nc}^*(\lambda)$.

The proof is in Appendix section 1.8.2, but the intuition is clear: In the contractible case, training is chosen to maximize total surplus whereas in the not contractible case training is chosen to maximize firm profits, which are less than total surplus because they exclude worker benefits and benefits to alternative employers.

1.4.3 Timing of the Non-Compete

Marx (2011) finds that most engineers who sign non-competes do not know about them at the time of the offer. Indeed, the typical story is that a worker accepts an offer without knowing about the non-compete in advance, then signs the non-compete on the first day while working through a pile of paper work. Incorporating these facts into the contractible model, and assuming that the worker does not anticipate the non-compete, it is straightforward to show that non-compete enforcement can only have a non-negative impact on training. In this scenario, the worker bargains for training level $T_c^*(0)$ and starting wage $W_c^*(0)$. In the second period the firm would make an incentive compatible training choice. If the the training chosen by the contract is such that $T_{nc}^*(\lambda) > T_c^*(0)$, then the firm will provide more training. Alternatively, assuming that the firm cannot 'untrain' the employee, if $T_{nc}^*(\lambda) \leq T_c^*(0)$ then the firm will leave the training level at $T_c^*(0)$. This blend of the contractible and not-contractible training indeed may be representative of the training received by the typical worker.

1.4.4 Efficiency

For brevity, I summarize here the main efficiency results and refer the interested reader to the thorough treatment of efficiency in the appendix. The primary questions of interest in this section are: (1) Given how firms will train their workers and workers will make quit decisions, what is the optimal level of non-compete enforcement? And (2), given optimal enforcement levels, how do training decisions compare to each other and the efficient outcome?

To establish the efficient outcomes, consider a social planner who chooses the non-compete enforcement level, the training, decision, and the quit decision, all subject to the information and timing constraints of the model. It is straightforward to show that such a social planner would choose never to enforce non-competes, would train by maximizing expected social surplus, and make the worker quit whenever he meets a more productive firm.

Consider next the choice of non-compete policy faced by state legislatures. In this theoretical setup, there are two potential benefits to enforcing non-competes: (1) Preventing inefficient quits, which occur when workers quit to join firms in which they are less productive and (2) reducing the tendency to underinvest in training due to the external benefits which accrue to future employers of the worker. The cost of non-compete enforcement is that it might prevent workers from moving to firms in which they are more productive. Optimal non-compete enforcement levels balance these costs and benefits. When training is not contractible in the hiring stage, enforcing non-competes both incentivizes the firm to train the worker more and reduces the chance the worker will quit for less productive jobs. When training is contractible, on the other hand, the positive training externality is fully internalized when the worker receives his full marginal product at his outside option, and therefore the only benefit of increasing non-compete enforcement is to prevent workers from quitting for less productive firms. This leads to a lower optimal level of non-compete enforcement relative to the not contractible case.

As long as optimal non-compete enforcement is non-zero, then both the training and mobility decisions will be inefficient. Evaluated at their respective optimal enforcement levels, training outcomes from the contractible case are weakly greater than when training is not contractible.

1.4.5 Confidential Information

A common rationale for enforcing non-compete agreements is that if workers have valuable information in the form of client lists or trade secrets, which the firm has presumably tried hard to procure and keep secret, then a departing worker could do harm to his previous employer by stealing its business and in so doing reduce its incentive for investment. Failing to enforce non-competes in this situation might be considered anti-competitive.

Adapting the model to address these concerns yields some interesting considerations. The rationale from the previous model is a good guide to thinking intuitively about how this addition to the model will work. If the worker who quits brings over confidential information which can be exploited by the new firm, in addition to his marginal product, then the worker is more likely to make an inefficient quit because the new firm values not only his marginal product but also his information. To the extent that this is a zero sum game,[31] so that the added production from the worker's knowledge at one firm results in a commensurate loss of production at his prior firm, there is no social benefit to the worker quitting to join a firm where his marginal product is lower. Therefore, the resulting optimal enforcement levels should be higher in order to deter the increased propensity for inefficient quitting.

To make this explicit, suppose that when the worker quits and his non-compete is not enforced, the worker's marginal product at the other firm is still $ay(T)$, but in addition the new competitor firm values the worker's knowledge of his prior firm at $k(T)$. Assume that this is a zero sum game, so that if the worker quits and his non-compete is not enforced then the initial firm loses $k(T)$.[32] The primary assumption on $k(T)$ is that increases in training

[31] This is likely to be the case when clients and client lists are the information being transported. It is unclear to what extent other trade secrets and confidential information would justify this zero-sum assumption.

[32] The assumption of a zero sum game is made for convenience. A more general specification would be that the production of the worker at the competitor firm is given by $f(a, y(T), k(T))$. One might imagine that a competitor with a higher a may be able to use the worker's knowledge better. In this case, the government would want the worker to move to the place where both he and his information are most highly valued. This

result in the worker knowing more and more about the firm, thereby making him more valuable. One might think then that $k(T)$ and λ should be related. Consider the simplest case, however, when λ is independent of $k(T)$, implying that knowing one trade secret yields the same probability of non-compete enforcement as knowing ten trade secrets.[33]

Consider the worker's quit decision and bargained second period wages. The worker quits if his expected pay at the poaching firm exceeds his bargained wage at the old firm. Since at the competitor firm the worker is assumed to earn his marginal product plus whatever else he brings in, the worker's wage if he quits successfully would be $ay(T) + k(T)$. The worker would quit when $(1 - \lambda)[ay(T) + k(T)] > w(T)$.

Next consider the worker's wages, $w(T)$, if he were to stay at the initial firm. The worker has the option to quit in the poaching period, but in the second period when he is bargaining his wage, he does not know what kind of firm he will meet. His expected outside option is $\mathbf{E}[v(T)] = (1-\lambda)(\mathbf{E}[a]y(T)+k(T))$. Given that $k(T) = zy(T)$, and that each firm, regardless of a, can use the new information equally well, his bargained wage is:

$$w(T) = y(T)(\beta + (1 - \beta)(1 - \lambda)[\mathbf{E}[a] + z])$$

Plugging the wage back into the quit equation gives the quit decision as a function of exogenous variables:

$$a > \frac{\beta}{1 - \lambda} + (1 - \beta)\mathbf{E}[a] - \beta z$$

Thus relative to the initial case, increases in z increase the second period wage, but not

necessarily complicates the identification of the optimal non-compete enforcement level, but the intuition remains valid.

[33] One objection to this setup is that it ignores the incentive issues for the firm to create valuable information in the first place. While this might be true, the firm still posses valuable information such as client lists or trade secrets including advertising strategies or other specific business information not known to its competitors. At least some of this information is transferred to the worker throughout is training at the firm.

enough to keep the quit probability constant. As shown above, the probability of quitting still increases with z.

Consider how allowing a worker to provide valuable information about his initial firm to a competitor firm will affect the efficient choices and the optimal non-compete level and training choices made in the two cases distinguished above. In terms of constrained efficiency, as long as the game is zero sum, in the sense that the knowledge part of the worker's production at the new firm does not add anything to the total surplus, there is no additional gain to enforcing non-competes or training workers differently, and therefore none of the constrained efficient choices would be changed.

In the case where competition at the hiring stage turns the profit maximization problem into a problem of total surplus maximization, the fact that the amount of business taken from the initial firm is equal to the amount taken by the competitor firm, $k(T)$, means that the only effect of non-compete enforcement is to decrease the chance the worker will make an inefficient quit decision, since competition over contractible training internalizes the positive training and information externalities. Therefore, the resulting optimal non-compete enforcement level will increase with z.

In the case where training is not contractible, there are two negative effects on the firm because the worker has valuable information: (1) If the worker successfully quits, the firm loses $k(T)$ in addition to his actual marginal product, and (2) the worker is more likely to quit to appropriate the value of his knowledge. The result of the first effect is that the firm will choose a lower training level, independent of the non-compete enforcement level (as long as $\lambda < 1$). The second effect also reduces the benefit to training because the worker is more likely to quit. In this scenario, the government should choose an optimal enforcement level that is much higher, and increasing in z, so that firms have better incentives to invest in human capital and workers are not encouraged to make inefficient quit decisions.

The optimality calculations from the adjusted model follow the same methods as in the appendix, and are therefore omitted. They lead to the following proposition.

Proposition 2. *If confidential information is zero-sum, then (a) optimal non-compete enforcement increases when training is and is not contractible, but increases more when training is not contractible, and (b) the chosen training level will be higher in the contractible case.*

1.4.6 The *in terrorem* Effect

The model outlined above provides a simple setup in which to examine the *in terrorem* effect. The *in terrorem* distinguishes between the worker's perceived enforcement, λ_p, and actual enforcement, λ_a. The perceived enforcement level affects the worker's quit decision, while the actual enforcement probability affects the actual probability the worker is allowed to switch firms. Implicit in the worker's perceived enforcement level is the worker's willingness to abide by the contract because he feels ethically obligated. Given that workers likely know little about non-compete enforcement but that firms are likely to be keen to remind them of their non-compete after they decide to quit, this consideration may be especially important. With these definitions, the worker's quit decision can be rewritten as

$$a > \hat{a}_p(\lambda_p) \equiv \frac{1}{1-\lambda_p} + (1-\beta)\mathbf{E}[a]$$

The worker's contract choice problem when training is contractible is given by:

$$\max_T \quad G(\hat{a}_p)y(T) + (1 - G(\hat{a}_p))(1-\lambda_a)\mathbf{E}[a|a > \hat{a}_p]y(T) - c(T)$$

The *in terrorem* effect suggests that if workers believe their non-compete to be enforceable, or feel ethically bound by it, then $\lambda_p = 1$. Substituting yields:

$$\max_{T} \quad y(T) - c(T)$$

As a result, the impact of actual non-compete enforcement on training choices is eliminated. The same can be shown in the not contractible case. As a result of the *in terrorem* effect, actual enforcement is irrelevant for training choices in either case. Intuitively, if workers either think their non-compete is enforceable or feel bound by it, regardless of the state in which they sign it, they will choose to obey it. As a result, firms have no differential training incentives.

Proposition 3. *If workers believe their non-competes to be enforceable or abide by them for any reason, $\lambda_p = 1$, then they never quit, $G(\hat{a})=1$, and firm-sponsored training levels are unrelated to actual non-compete enforcement, $\frac{\partial T_c^*}{\partial \lambda_a} = 0$ and $\frac{\partial T_{nc}^*}{\partial \lambda_a} = 0$.*[34]

1.4.7 Theoretical Prescriptions for Courts and State Legislatures

While this model does not present a full welfare analysis of non-competes, the takeaways relevant to courts are: (1) When training is contractible, increased enforcement of non-compete agreements does not necessarily increase firm-sponsored training. (2) When training is not contractible, increased enforcement increases firm-sponsored training. (3) If there is a legitimate worry that a worker is simply transporting clients or potential trade secrets from one firm to the other, then the likelihood that the quit is inefficient is greatly enhanced and enforcement should be higher. (4) If workers believe their non-competes to be enforceable or adhere to them for some other reason, then actual enforcement policies are irrelevant. (5)

[34]The proof of this proposition is omitted.

In light of the fact that more training occurs when it is contractible, courts may be able to improve training outcomes by inducing workers and firms to bargain over the terms of the contract. To the extent that early notification of their non-compete would encourage workers to bargain for training that they would not have otherwise requested, laws such as Oregon's which enforce only non-competes for workers who are given two weeks notice may encourage the contractibility of training.

1.5 Empirical Analysis

The theoretical model shows that non-compete enforcement should only necessarily be positively related to training levels if training is not contractible, or if training is contractible but the probability of an inefficient quit is high. Additionally, the model shows how perceptions of non-compete enforcement may undermine that positive relationship. Without knowing the extent to which training is contractible in the data, the relationship between non-compete enforcement and training as an empirical question.

1.5.1 Training Data

The training data comes from the topical module from Wave 2 of the Survey of Income and Program Participation (SIPP) panels from 1996, 2001, 2004, and 2008. The SIPP is a longitudinal survey that interviews respondents once every four months for three to four years. Because non-compete enforcement varies almost entirely in the cross-section, I pool all of the cross-sections together and include year fixed effects in the estimation. The SIPP tracks up to two occupations for each individual and in order to assure that I analyze the occupation in which the training actually occurred, I restrict the sample to workers who hold only one job. I also drop workers younger than 22 and older than 55, as well as workers with jobs in the non-profit sector, government, community service, education, military, and protective services. There remain 70,374 individuals in the sample. Occupation codes are updated to 2007 two-digit Standard Occupational Classification (SOC) codes and industry codes are updated to 2007 two-digit NAICS codes.

Due to the ambiguity in defining training, I choose as the dependent variable the most blunt instrument: an indicator equal to one if the worker answers yes to the question "During the past year, has [the respondent] received any of kind of training intended to improve skill in one's current or most recent job?" and also reports that his firm has paid for the training. It is unclear whether a worker who answers affirmatively to both of these questions is referring to informal or formal training, and the SIPP does not make this distinction. Only about 20% of the individuals in the sample report receiving firm-sponsored training in the last year, which suggests that this variable reflects formal training, since workers early in their tenures appear to receive relatively more informal training (Barron et al. 1999). If indeed the dependent variable captures only formal training, and formal training tends to be contractible while informal training is less often contracted upon, then the model suggests that any effects I find may understate the actual effect of non-compete enforcement on *total* training.

In order to exploit the cross-sectional state level heterogeneity in non-compete enforcement, I compare training outcomes between occupations likely to see non-compete litigation (high litigation) and occupations unlikely to see such litigation (low litigation) using surveys of litigated, non-compete cases (LaVan 2000, Whitmore 1990). LaVan's (2000) study of 104 randomly selected cases finds the following occupation distribution: 25% managerial, 31% sales, 37% professional, 1% entertainer. Whitmore (1990) studies 105 cases from the 1960s to the 1980s and finds that the occupation distribution is 9% skilled labor, 51% sales, 14% middle management, 7% business executive, 2% engineers, 1% entertainers, 9% physician, and 5% other professional. It is unclear if this is a random sample. I include service workers as high litigation because 44% of cases in LaVan's study involved either retail or service companies and it is unclear if services were considered separately from traditional sales occupations. The mapping of two digit Standard Occupational Classification (SOC) system codes is presented in Table 1.2. Inclusion into low litigation occupations was defined as having less than or equal to 1% of litigated cases or being in a legal field, since non-competes are traditionally banned for lawyers (Stroud 2002).

Selection into low litigation can be determined by four possibilities: (1) Workers in these occupations do not actually sign non-competes, thereby exempting them from potential litigation, (2) firms decide not to attempt to enforce non-competes for these occupations, presumably because the expected costs of enforcement outweigh the expected benefits, (3) the outcome of enforcement is certain, and therefore firms and workers do not bother litigating, and (4) the worker and firm settle outside of court. Examining the two-digit SOC occupations in the low litigation group shows that with the exception of lawyers, most of the occupations tend to be low skill and low earnings occupations. This evidence suggests that selection into low litigation is primarily determined by either not signing non-competes or firms choosing not to enforce because the occupation is a low value occupation. Summary statistics for key variables are presented in Table 2.14, and state, industry, and occupation distributions by high and low litigation status are shown in Figures 1.5.1 to 1.5.3.

Table 1.2: Mapping SOC Codes to High/Low Litigation Occupations

Low Litigation	High Litigation
Legal	Management
Arts, Entertainment, Recreation	Business, Financial
Food Prep, Serving	Computer, Mathematical
Grounds Maintenance	Engineering, Architecture
Office Support	Life, Physical, Social Sciences
Farming, Fishing, Hunting	Healthcare Practitioners, Technical
Construction, Extraction	Personal Care, Services
Transportation, Materials Moving	Installation, Repair
	Production
	Sales

Note: Education, Community Service, Protective Service, and Military occupations have been dropped from the sample, along with all non-profit and government workers. Service workers, such as installation and repair and personal care, are included as high litigation because LaVan (2000) and Whitmore (1990) do not distinguish between selling a product and performing a service.

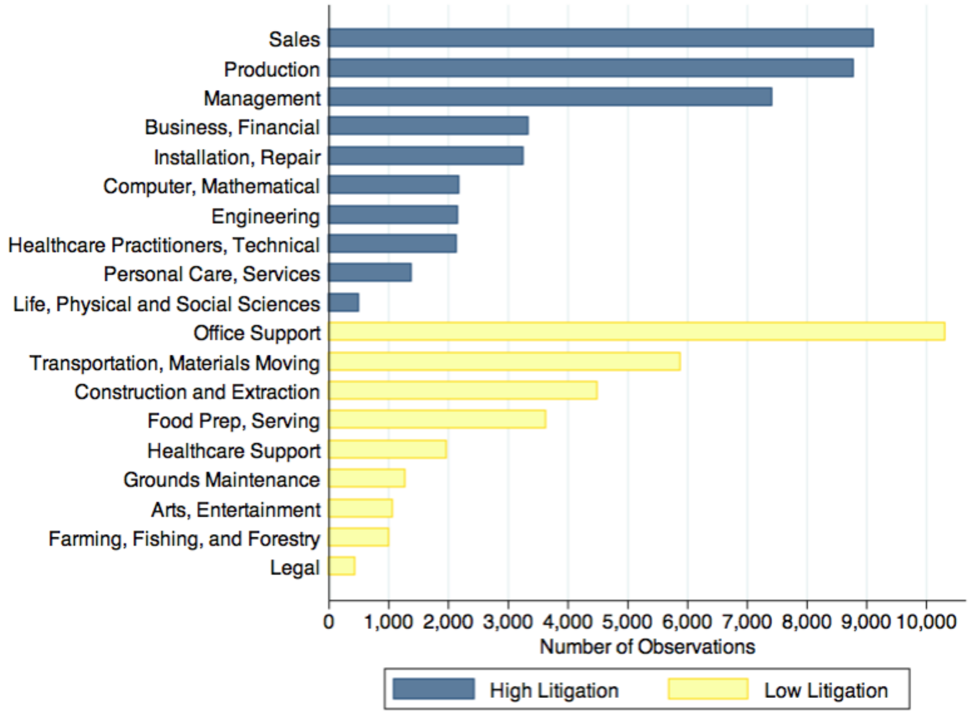
Table 1.3: Summary Statistics

	Low Litigation		High Litigiation		T-Test	
Variable	Mean	SD	Mean	SD	Difference	P-value
Firm-sponsored Training	0.13	0.34	0.23	0.42	-0.09	0.00
Initial Potential Experience	31.76	9.10	31.62	8.90	0.14	0.04
Tenure	5.96	6.71	7.36	7.37	-1.41	0.00
Monthly Earnings	2,655	2,555	4,344	4,407	-1,688	0.00
Bachelors	0.10	0.30	0.23	0.42	-0.13	0.00
Grad School	0.02	0.14	0.08	0.27	-0.06	0.00
Metro	0.79	0.41	0.81	0.39	-0.02	0.00
Male	0.51	0.50	0.58	0.49	-0.08	0.00
White	0.66	0.48	0.75	0.43	-0.09	0.00
Establishment Size 25-99	0.24	0.43	0.22	0.42	0.02	0.00
Establishment Size 100+	0.35	0.48	0.45	0.50	-0.10	0.00
Firm Size 25-99	0.15	0.36	0.12	0.33	0.02	0.00
Firm Size 100+	0.58	0.49	0.70	0.46	-0.12	0.00
Hours Per Week	38.96	10.18	41.92	9.61	-2.96	0.00
Union	0.12	0.32	0.08	0.28	0.03	0.00
Observations	30,094		40,280			

Note: The T-Test is two-tailed and the corresponding p-value is the probability of getting an estimate this large if the population difference equals zero.

Workers in high litigation occupations are very different from those in low litigation occupations. For example, in this sample they report receiving seven percentage points more

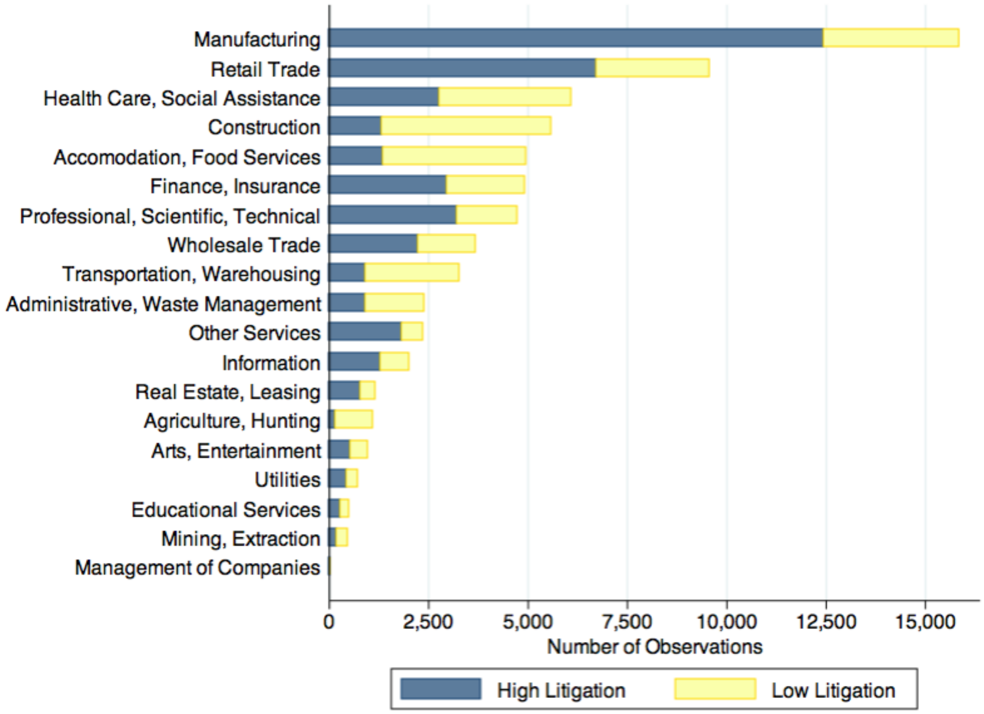
Figure 1.5.1: Occupation Distribution by Litigation Type



training than low litigation occupations. They are also more educated, earn more money each month, tend to be in bigger firms, and are less unionized. It is especially important to recognize that the within-state distribution of litigation types is balanced because the empirical strategy I employ relies on within state differences in training between high and low litigation occupations. Indeed in the sample as whole, 43% of workers are in low litigation occupations. Overall, high litigation occupations look a lot like high skill occupations and low litigation occupations look a lot like low skill occupations. Presumably this distinction arises because high skill occupations are more valuable to the firm and firms might only be willing to sue high value workers to prevent them from moving to a competitor.

To get a sense of the unconditional relationship between non-compete enforcement and the within-state difference between firm-sponsored training received by high and low litigation occupation, Figure 1.5.4 plots the average probability of receiving training in high and low

Figure 1.5.2: Industry Distribution by Litigation Type

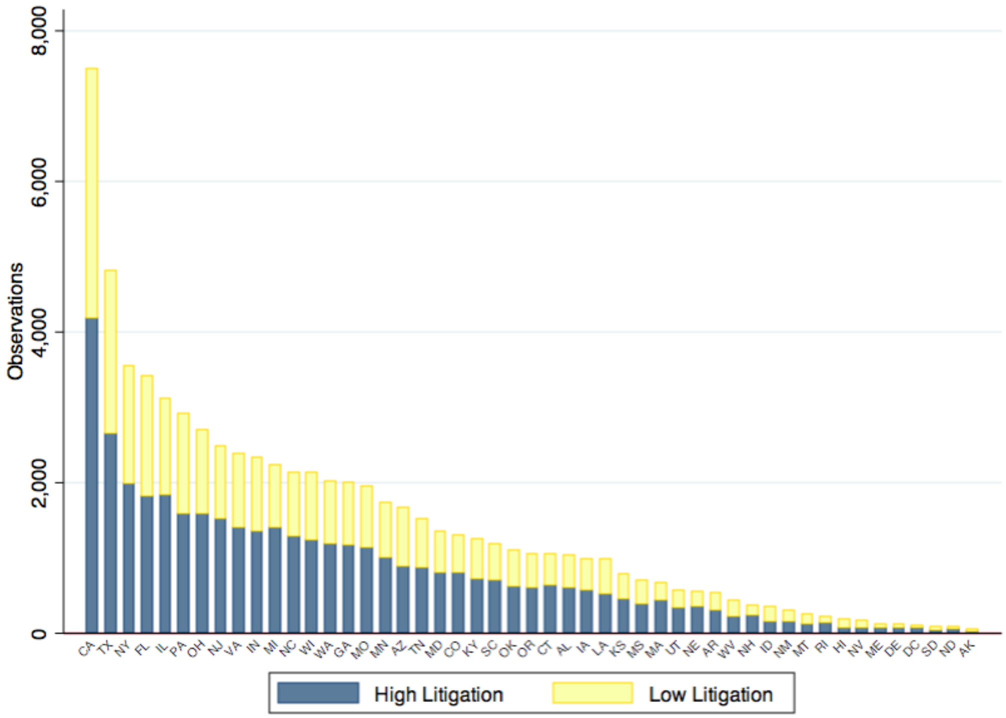


litigation occupations within each state against non-compete enforcement intensity. The unconditional difference-in-differences estimate is the difference between the slopes. Importantly, note that the difference in the slopes is driven not by the ends of the enforcement distribution, but instead by states which have an enforcement score greater than zero.

1.5.2 Identification

Due to the fact that non-compete enforcement varies primarily in the cross section, I employ a difference-in-differences (DID) strategy with state fixed effects to identify the relative impact of non-compete enforcement between occupations which appear frequently in non-compete litigation (high litigation) and those which appear infrequently or not at all (low litigation). Importantly, low litigation occupations are not necessarily unaffected by enforcement because

Figure 1.5.3: State Distribution by Litigation Type



these workers may also sign non-competes, but the likelihood of litigation is much lower for this group. Indeed, there is evidence that there is a positive relationship between training and non-compete enforcement for workers in low litigation occupations, which implies that the difference-in-difference estimates are a lower bound on the overall effect.

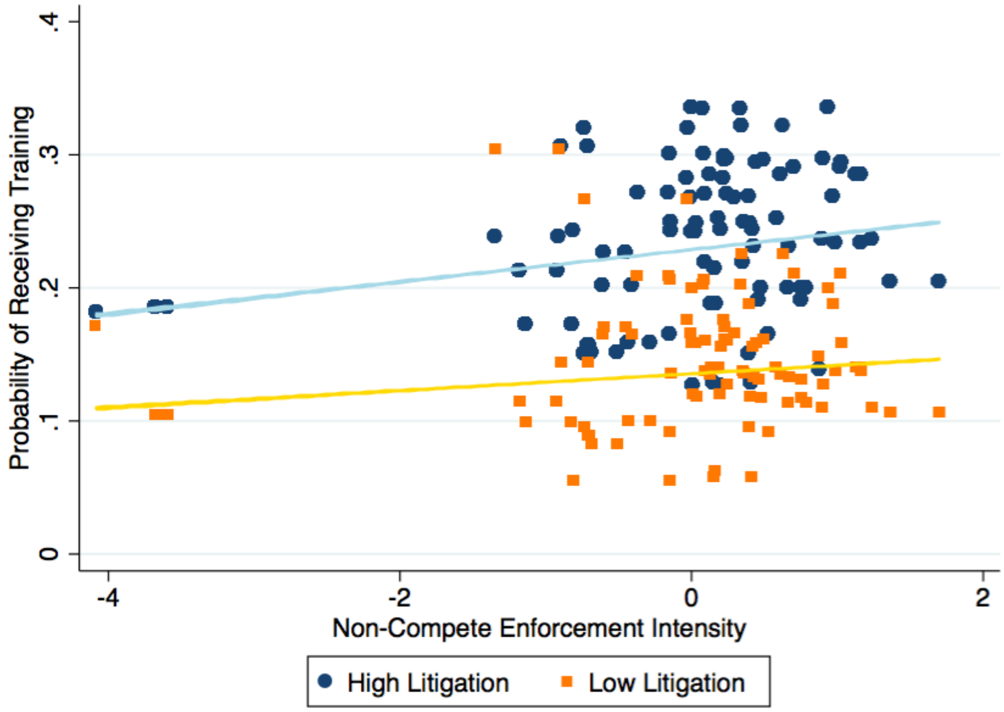
With this strategy, I estimate variants of the following two equations:

$$T_{ijost} = \beta_0 + \beta_1 Enfc_{st} * HL_o + \beta_2 Enfc_{st} + \gamma X_{ijst} + \Omega_o + \theta_s + \phi_t + \epsilon_{ijost} \quad (1.5.1)$$

$$T_{ijost} = b_0 + \sum_{k=1}^{10} \alpha_k Enfc_{st} * Occ_{k,HL} + b_2 Enfc_{st} + \gamma X_{ijst} + \Omega_o + \theta_s + \phi_t + \nu_{ijost} \quad (1.5.2)$$

In equations (1.5.1) and (1.5.2), T_{ijost} refers to an indicator for worker i at firm j in occupation o and state s having received firm-sponsored training in year $t-1$. State fixed effects are represented by θ_s, Ω_o are 2-digit SOC occupation dummies, ϕ_t are year fixed effects, X_{ijst}

Figure 1.5.4: Within-State Training versus Non-Compete Enforcement



is a set of individual, firm level, and interacted state-level controls, HL_0 is a dummy for high litigation occupations, and $Enforce_{st}$ is the non-compete enforcement level of state s at time t. The variable $Occ_{k,HL}$ is an indicator variable for occupation k conditional on being a high litigation occupation. The errors are clustered at the state level to account for state-level correlations in the disturbances. The coefficients of interest are β_1 in equation (1.5.1) and the ten α_k terms in equation (1.5.2). They capture the causal, intention-to-treat effect of non-compete enforcement on high litigation occupations relative to low litigation occupations.

The set of controls, X_{ijst} consist of potential experience, potential experience squared, tenure, tenure squared, hours worked, and indicators for working in a metro area, bachelors degree, graduate degree, male, white, establishment and firm size 25-99, establishment and firm size 100+, whether the worker is unionized, NAICS 2 digit industries, year, and state. State corporate tax rates (Seegert 2013), indicators for exceptions to at-will employment (Autor et al. 2006), and whether the state is a right-to-work state are all interacted with the high litigation or occupation specific main effects.

The state fixed effects account for other time invariant state characteristics which might cause omitted variable bias. Due to the inclusion of state fixed effects, the identifying assumption is that there are no unobserved variables which differentially affect within-state firm-sponsored training choices for high litigation groups relative to low litigation groups that are also correlated with non-compete enforcement. In notation, the identifying assumption for equation (1.5.1) is

$$\mathbf{E}[Enfc_{st} \cdot HL_o \cdot \epsilon_{ijost} | X_{ijst}, \Omega_o, \theta_s, \phi_t] = 0 \tag{1.5.3}$$

The equivalent assumption for equation (1.5.2) follows a similar form. In the robustness section below, I show that the training effects are not driven by reverse causality, high training firms sorting to high enforcing states, and skill-related training being more likely in high enforcing states.

Intent to Treat vs. Treatment on the Treated

Non-compete enforcement only matters for workers who sign non-compete agreements. Unfortunately, whether a worker has signed a non-compete is not contained in the data. Therefore, the way to interpret a coefficient like β_1 from equation (1.5.1) is as an intent-to-treat effect. The state with a high intensity of enforcement is offering a treatment, but firms can choose to opt out of treatment by not using non-competes. While identifying the treatment on the treated effect is certainly a parameter of interest, the intent-to-treat effect is the relevant parameter for state judiciaries to consider since they choose the intensity of enforcement but cannot force firms to use non-competes.

1.6 Results

1.6.1 Baseline Results

The results from equation (1.5.1) are reported in column (4) of Table 1.4.[35] The intent to treat effect in the full sample is 0.007. This implies that if a state were to increase its non-compete enforcement intensity by 1 standard deviation, high occupation workers on average would receive 0.7 percentage point increase in the probability of receiving firm-sponsored training in the given year. This corresponds to 3% of the mean of training for high litigation workers. Columns (1), (2) and (3) show the standard difference-in-differences results with and without controls, and without controls but with state fixed effects. Given that the differential effect for high litigation without state fixed effects is very similar to the specification with state fixed effects, it appears that unobserved state level factors are not driving the impact on the low litigation group.[36]

The first column of Table 1.6 shows the occupation-specific ITT estimates from equation (1.5.2). The occupation specific impact on of enforcement on training ranges from -0.01 to 0.17 percentage points, which correspond to between 3% and 7.5% of the mean level of workers reporting receiving training in that occupation. Management, business, financial, computer and mathematical occupations, engineers, healthcare practitioners and technical healthcare workers (not support), and personal care and service occupations are significantly

[35]Note that because the enforcement index is a generated regressor, there is error associated with the generation process which is not captured in the estimation procedure.

[36]Tenure, potential experience, and firm size may be considered bad controls since greater non-compete enforcement is likely to lengthen tenures, reduce the experience necessary to be hired, and increase the size of the firm since workers are not quitting and firms are incentivized to invest more in R & D. Omitting these variables from the regression does not substantially change the estimate or the standard error.

Table 1.4: Baseline Training Results

	DID		DID State FE	
	(1)	(2)	(3)	(4)
High Litigation*Enforcement	0.007***	0.006**	0.006***	0.007**
	(0.002)	(0.003)	(0.002)	(0.003)
Enforcement	0.007**	0.003	0.069	0.007
	(0.003)	(0.002)	(0.044)	(0.013)
Observations	70,374	70,374	70,374	70,374
R-squared	0.041	0.097	0.049	0.101
State FE	No	No	Yes	Yes
Controls	No	Yes	No	Yes

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses, clustered at the state level. The dependent variable is an indicator equal to one if the worker received firm-sponsored training in the last year. The omitted group is low litigation occupations. The set of controls consist of potential experience, potential experience squared, tenure, tenure squared, hours worked, and indicators for working in a metro area, bachelors degree, graduate school degree, male, white, establishment and firm size 25-99, establishment and firm size 100+, whether the worker is unionized, NAICS 2 digit industries, year, and state. State corporate tax rates, indicators for exceptions to at-will employment, and whether the state is a right-to-work state are all interacted with the high litigation or occupation specific main effects.

affected by non-compete enforcement relative to low-litigation occupations.[37] Breaking down the effect by gender shows that women are more affected than men, but the difference is not statistically significant (results not shown here). Figures 1.8.2 to 1.8.7 in section 1.8.4 in the appendix plot the occupation specific estimates against occupation level averages of schooling, monthly earnings, tenure, training, and industry concentration. They show that the impact of non-compete enforcement on training tends to be located in occupations that have higher earnings and higher schooling levels.

[37] Alternative specifications using logit and probit models find substantively similar results.

1.6.2 The Type of Training

Since training could consist of many activities, I use the SIPP's follow up questions regarding the contents of the training to see which type of training is driving the results. Training is categorized into the following non-mutually exclusive categories: basic skills, new skills, upgrade existing skills, and introduce company policies. Summary statistics of these outcomes by high and low litigation status are given in Table 1.5. Two-thirds of the firm-sponsored training is upgrading skills, about half is teaching new skills, and one-third is teaching basic skills and introducing company policies, though there is substantial overlap.

Table 1.5: Summary Statistics of Firm-Sponsored Training Content

	Low Litigation		High Litigiation	
Variable	Mean	SD	Mean	SD
Firm-Sponsored Training	0.13	0.42	0.23	0.42
Basic Skills	0.05	0.21	0.06	0.24
New Skills	0.06	0.25	0.10	0.30
Upgrade Skills	0.09	0.29	0.17	0.37
Company Policies	0.04	0.18	0.05	0.23
Observations	30,094		40,280	

To examine which type of training non-compete enforcement affects, I run the same regressions using indicators for type of training received as the dependent variables. The results in Table 1.6 show that the relative enforcement effect is driven by skill upgrading. Breaking the effect down by occupation shows that only business and financial occupations are trained more in basic skills in higher enforcing states. Most of the firm-sponsored training effects are driven by training designed to upgrade worker skills. Additionally, while the overall impact of enforcement on firm-sponsored training for business and financial occupations is small and statistically insignificant, the 1 percentage point impact on basic skills training is relatively large. With regards to the probability of learning new skills, non-compete enforcement appears to positively affect only business, financial, and computer and mathematical occupations.

Table 1.6: Firm-Sponsored Training Content

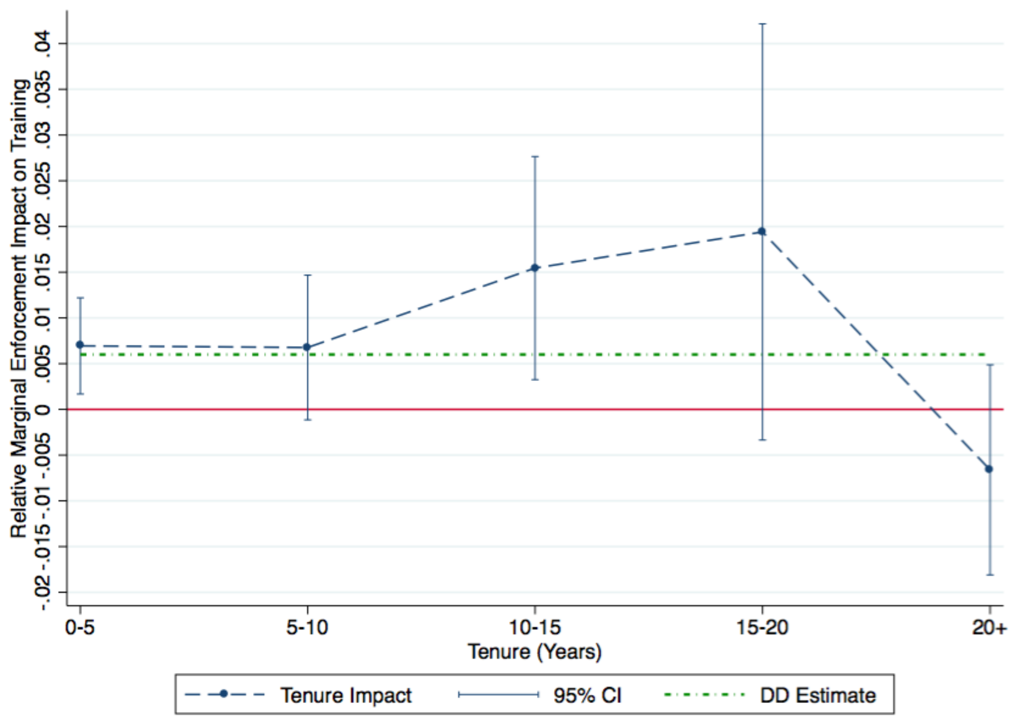
Intent-to-Treat Effect	Training	Basic	New	Upgrade	Policies
High Litigation	0.007**	0.002	0.002	0.005**	0.001
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Management	0.007*	0.003	0.002	0.007**	0.001
	(0.004)	(0.002)	(0.002)	(0.003)	(0.002)
Business, Financial	0.012**	0.011***	0.006*	0.008*	0.004*
	(0.005)	(0.002)	(0.003)	(0.005)	(0.002)
Computer, Mathematical	0.010**	0.003	0.007*	0.014***	0.003
	(0.005)	(0.002)	(0.004)	(0.005)	(0.002)
Engineering	0.017**	-0.001	0.003	0.015***	0.001
	(0.006)	(0.003)	(0.004)	(0.004)	(0.003)
Life, Physical, Social Sciences	-0.010	-0.001	0.003	-0.003	0.004
	(0.010)	(0.004)	(0.006)	(0.009)	(0.006)
Healthcare Practitioners, Technical	0.015**	-0.004	0.002	0.008*	0.000
	(0.006)	(0.005)	(0.006)	(0.005)	(0.003)
Personal Care, Services	0.011***	0.004	0.006	0.009**	0.004
	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)
Sales	0.004	0.002	0.001	0.003	0.002
	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Installation, Repair	0.008	-0.000	0.004	0.002	-0.004
	(0.006)	(0.002)	(0.004)	(0.006)	(0.003)
Production	0.001	-0.002	-0.001	0.000	-0.003
	(0.003)	(0.001)	(0.002)	(0.003)	(0.002)
Observations	70,374	70,374	70,374	70,374	70,374

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses, clustered at the state level. The omitted group is low litigation occupations. All dependent variables are indicator variables for the type of firm-sponsored training received. Basic refers to training for basic skills. New refers to training to learn new skills. Upgrade refers to training that improves existing skills. Policies refers to training that introduces company policies. The set of controls are the same as the baseline specification discussed on page 182.

1.6.3 Enforcement Impact Across Tenure and Age

Recall that under the contractible model the effect of enforcement on training was ambiguous and depended upon the likelihood of an efficient quit, while the effect in the not-contractible model was unambiguously positive. Under the assumption that contractible training is more likely to occur early in a worker's tenure, examining the impact of non-compete enforcement on training across tenure can distinguish between training that is contractible and not-contractible. To do this, I run the baseline specification for different bins of tenure levels. The results, displayed in Figure 1.6.1,[38] show that the impact of non-compete enforcement on training rises between years 0-5 and years 15-20, before falling off. The rising impact across tenure provides evidence that indeed the operative model of training later in tenure is the not-contractible model. If this is the case, then there is a role for non-compete enforcement in reducing the resulting inefficiency in investment.

Figure 1.6.1: Marginal Effect of Non-Compete Enforcement Across Tenure



The impact across tenure may also be a result of the fact that as employee's stay longer with the firm, they collect more and more valuable company trade secrets, client relationships, and confidential information. As such, while the employee has demonstrated a commitment to the firm by staying, competitor firms would pay willingly for the employee's talent, knowledge, and client access, which would reduce the firm's incentive to train at all points in tenure, all

[38]The corresponding numbers are shown in Table 1.12 in the appendix.

else equal. Thus the employee's non-compete becomes an important feature of the employee-firm relationship and gives the firm confidence in the employee's loyalty. In this way, the enforceability of an employee's non-compete may increase his likelihood of promotion. Since promotions typically come with enhanced responsibility, the firm may provide additional training corresponding to the promotion.

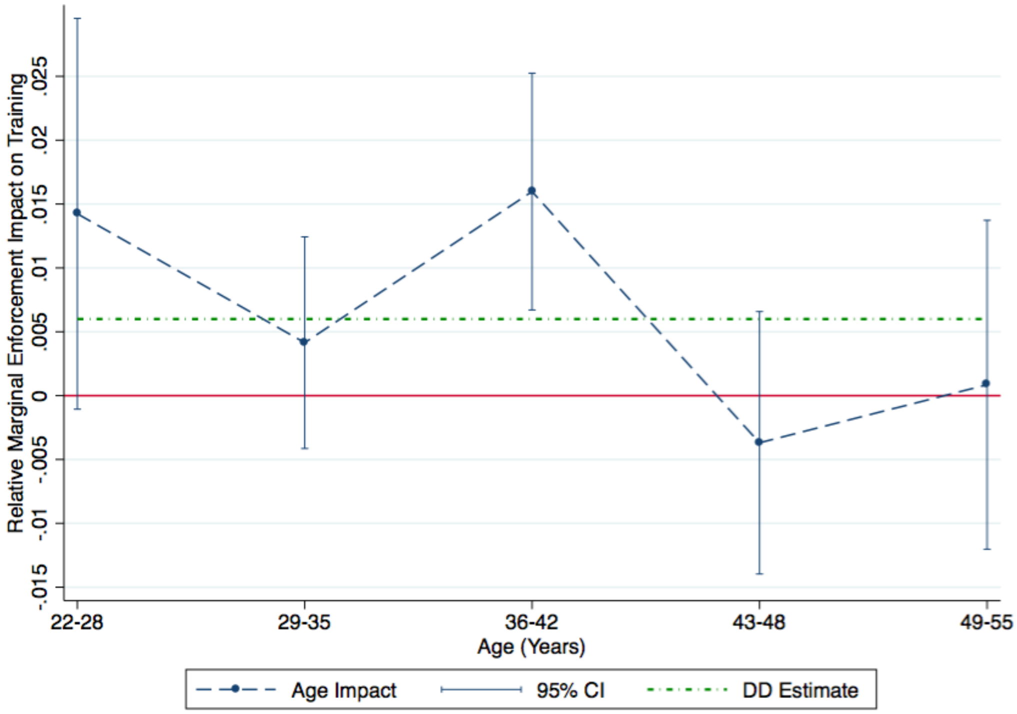
Figure 1.6.2 shows the impact of non-compete enforcement by age of the worker. The results show that the impact is positive for workers aged 22 to 42, but indistinguishable from zero for workers older than 42. The strongest impact is for workers aged 36-42, for whom a one standard deviation increase in non-compete enforcement increases the probability of receiving training by 1.6 percentage points, an increase of about 7%. These results correspond to the tenure results, showing the workers in the middle of their career benefit most from non-compete enforcement, and are in line with the logic that the firm wants to invest more in younger works so that they can extract the benefit of that training over the long tenure of the worker.

1.6.4 Other Enforcement Predictions

Given the training effects documented above, I test for three alternative predictions. First, it could be that training is not the primary margin on which non-compete enforcement affects the firm. Instead, firms in lower enforcing states may be choosing to hire more experienced workers in order to avoid having to pay training costs, while firms in higher enforcing states may be more likely to hire less experienced workers. In order to explain the observed training effects there must be a negative relationship between non-compete enforcement and the starting experience of the worker. Instead of actual starting experience, I use starting potential experience with the same estimation strategy from (1.5.1) and (1.5.2).[39]

[39] The other control variables in the estimation are the main effects for high litigation occupations, hours worked per week, indicators for working in a metro area, establishment and firm size 25-99, establishment

Figure 1.6.2: Marginal Effect of Non-Compete Enforcement Across Age



Second, the model shows that higher non-compete enforcement leads to lower quit probabilities, which in turn lead to longer tenures. The lower probability of a quit encourages the firm to invest in their worker's training. Ideally, I would estimate job durations, but given the pooled cross-sectional nature of the data I estimate the effects of enforcement on tenure using the same difference-in-differences strategy.[40]

Third, the model shows that higher non-compete enforcement leads to wage compression, which encourages more training. Directly estimating whether non-compete enforcement

and firm size 100+, NAICS 2 digit industries, year and state. State corporate tax rates, indicators for exceptions to at-will employment, and whether the state is a right-to-work state are all interacted with the main effects for high litigation occupations.

[40] Other controls are main effects for high litigation occupations, starting potential experience, starting potential experience squared, hours worker, and indicators for working in a metro area, bachelors degree, graduate degree, male, white, establishment and firm size 25-99, establishment and firm size 100+, whether the worker is unionized, NAICS 2 digit industries, year, and state. State corporate tax rates, indicators for exceptions to at-will employment, and whether the state is a right-to-work state are all interacted with the main effects for the high litigation occupations.

affects wage compression requires knowledge of pre-training wages and productivity and post-training wages and productivity. Without productivity data, I cannot estimate the effect of non-compete enforcement on wage compression because non-compete enforcement affects the extent of labor market competitiveness and thus limits any assumptions on the relationship between wages and productivity. Despite these limitations, however, I can still test for evidence of wage effects. Recall equation (1.4.2). If non-compete enforcement increases training, then there are two contrasting effects: (1) workers in higher enforcing states may have lower wages because they are not fully compensated for their outside options, and (2) they also receive the wage boost from the extra training they receive.[41] If non-compete enforcement causes lower wages, then the effect of non-compete enforcement through wage compression dominates. I examine this by regressing log hourly wages on non-compete enforcement using the same identification strategy as above.[42]

The results of these regressions are shown in the second through fourth columns of Table 1.7. The results for high litigation occupations as a whole are generally in the expected direction but only the starting potential experience margin reaches canonical levels of statistical significance. The occupation specific effects show that for computer and mathematical occupations, engineers, personal care and service occupations, and production occupations, firms tend to hire younger workers in higher enforcing states. The occupation specific effects on tenure are mixed in their direction, but none of them reach statistical significance. The effects of non-compete enforcement on wages show that computer and mathematical occupa-

[41] They also might receive additional compensation for signing an enforceable non-compete if they were able to bargain over it. This effect would presumably be in the first years of tenure if the worker signed the agreement at the beginning of the employment relationship. Since most workers don't have the opportunity to bargain over their non-compete (Marx 2011), this effect is expected to be small. Regressions, not shown here, confirm this is true.

[42] Other controls include main effects for high litigation occupations, potential experience, potential experience squared, tenure, tenure squared, hours worked per week, and indicators for working in a metro area, bachelors degree, graduate school degree, male, white, establishment and firm size 25-99, establishment and firm size 100+, whether the worker is unionized, NAICS 2 digit industries, year, and state. State corporate tax rates, indicators for exceptions to at-will employment, and whether the state is a right-to-work state are all interacted with the main effects for high litigation occupations.

tions and engineers earn statistically significantly less in higher enforcing states, suggesting that the wage compression effect dominates the training effect. The lack of a significant negative sign for some of these occupations is not surprising given the contrasting impacts of non-compete enforcement on wages. Overall, these results suggest that the training effect can in part be explained by the effect on the hiring margin.

Table 1.7: Results and Potential Explanations

Intent-to-Treat Effect	Training	Initial Exp	Tenure	Log Wage
High Litigation	0.007**	-0.116*	-0.009	-0.004
	(0.003)	(0.066)	(0.050)	(0.004)
Management	0.007*	-0.057	-0.006	-0.009
	(0.004)	(0.118)	(0.063)	(0.005)
Business, Financial	0.012**	-0.097	0.030	-0.011*
	(0.005)	(0.111)	(0.072)	(0.006)
Computer, Mathematical	0.010**	-0.513***	-0.099	-0.024***
	(0.005)	(0.096)	(0.066)	(0.007)
Engineering	0.017**	-0.230*	0.114	-0.020***
	(0.006)	(0.126)	(0.075)	(0.007)
Life, Physical, Social Sciences	-0.010	0.028	-0.069	-0.003
	(0.010)	(0.181)	(0.143)	(0.015)
Healthcare Practitioners, Technical	0.015**	-0.139	0.062	0.004
	(0.006)	(0.186)	(0.087)	(0.006)
Personal Care, Services	0.011***	-0.551**	-0.150	0.007
	(0.004)	(0.242)	(0.113)	(0.008)
Sales	0.004	0.114	-0.036	-0.012
	(0.003)	(0.074)	(0.064)	(0.007)
Installation, Repair	0.008	-0.160*	0.001	0.004
	(0.006)	(0.086)	(0.065)	(0.009)
Production	0.001	-0.207**	0.005	0.018
	(0.003)	(0.081)	(0.066)	(0.012)
Observations	70,374	70,374	70,374	66,528

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses, clustered at the state level. The omitted group is low litigation occupations. For other control variables, see the footnote 39 on page 46 (for initial experience), 42 on page 48 (for log wages), and 40 on page 47 (for tenure). Tenure is in years and log earnings are log monthly earnings.

1.6.5 Empirical Recommendation to Courts and State Legislatures

The enforcement index generated from factor analysis is useful because it provides relatively objective weights for the seven underlying dimensions of non-compete enforcement intensity, but it is less useful to courts that want to know in which way they should increase or decrease enforcement. In order to provide direct policy relevance, in this section I break up the enforcement index into its separate components and see which components of the index cause the increase in firm-sponsored training. The results appear in Table 1.8.

The results from including only one dimension of enforcement in columns (1) - (7) show that the extent of the plaintiff's burden of proof is positively and (statistically) significantly related to firm-sponsored training. The easier it is for the firm to prove their case, the more likely they are to actually provide training to their worker.

The individual dimensions are positively correlated, however, and when considered individually are biased upwards because of omitted variable bias. Including each of the variables linearly, the results in column (8) show two interesting points. First, the impact of easing the plaintiff's burden of proof increases training, as it did in the univariate specification. The other notable point is that conditional on other dimensions of enforcement, enforcing non-competes that provide less consideration post inception reduces training. While it appears to contradict the previous findings of the paper, this finding shows that firms in states which require some kind of compensation in order for a non-compete signed after inception to be enforced actually provide workers with more training in exchange for signing the contract. This effect is in fact aligned well with the contractible model since the firm is makes either training, promotions, or some other benefits a part of the contract. Importantly, including all of the enforcement variables into the specification is subject to the problem of micronumerosity, which arises because clustering at the state level reduces the effective sample size to 48.

Table 1.8: Policy Options

Enforcement Dimension	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Statute	0.003							-0.001
	(0.002)							(0.002)
Protectable Interest		0.003						0.000
		(0.002)						(0.002)
Plaintiff's Burden			0.005***					0.006**
			(0.002)					(0.002)
Consideration At Inception				0.001				-0.000
				(0.001)				(0.002)
Consideration Post Inception					0.000			-0.002**
					(0.001)			(0.001)
Overbroad Contracts						0.002		0.001
						(0.001)		(0.001)
Quit v Fire							0.002	0.001
							(0.001)	(0.002)
Observations	70,374	70,374	70,374	70,374	70,374	70,374	70,374	70,374
R-squared	0.102	0.102	0.102	0.102	0.102	0.102	0.102	0.102

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses, clustered at the state level. The dependent variable is an indicator equal to one if the worker received firm-sponsored training in the last year. Reported coefficients are interaction effects with high litigation occupations from difference-in-differences estimation with state fixed effects. The set of controls are the same as the baseline specification discussed on page 182.

1.6.6 Robustness

Threats to Identification

There are at least three threats to identification: (1) Reverse causality, (2) unobserved confounding treatments may make skill-related training more likely in higher enforcing states, and (3) firms in higher enforcing states may be systematically different from firms in lower enforcing states in unobserved ways which make firms in higher enforcing states more likely to provide training to high litigation workers. With regards to reverse causality, most states have not changed their policies over time, suggesting that states are not changing enforcement protocol in response to training outcomes. Indeed, California's ban on non-competes began when it adopted the laws written by David Dudley Field in 1872 (Gilson 1999). Some states within my time frame have included extraordinary training as a protectable interest of the firm. To assure that states are not responding to training, I run a robustness check using only the 1991 enforcement weights and scores, which occur before any of the training in my data. These and other variations of the enforcement index are shown in Table 1.10. They show that the results are robust to all variations in the enforcement index.

Additionally, because the high litigation versus low litigation comparison is in some sense a high skill versus low skill comparison, there may be some omitted variable which makes training for high skill workers more likely in higher enforcing states. Such an omitted variable would bias the coefficient on non-compete enforcement upward. To address this concern, I use the same difference-in-differences specification to compare high litigation occupations from the not-for-profit sector[43] which are presumably less likely to be impacted by non-compete enforcement, to for-profit low litigation occupations (the same control group). These results

[43] The not-for-profit sector here is all sectors that are not private sector for profit firms. It includes the government, non-profits, and the self-employed. While there is no empirical evidence suggesting that non-competes are less important for these sectors, I believe these organizations are either less likely to either have competitors, use non-competes, or attempt to enforce them. I also include all lawyers in the high litigation group here because they are a high skill group which are unaffected by non-competes.

are presented in column (1) of Table 1.9. Additionally, because lawyers are a high skill occupation which are unaffected by non-competes per the American Bar Association's Model Rule 5.6 (Stroud 2002), I perform the same difference-in-difference estimation for lawyers versus for-profit low litigation workers. The results of this test are shown in column (2) of Table 1.9. The intent to treat estimates from columns (1) and (2) are all small and insignificant, providing evidence that a higher likelihood of skill-related training in higher enforcing states is not driving these results.

The last concern relates to the fact that firms may sort into high and low enforcing states based on some unobserved characteristic which is correlated with the training differential between high litigation and low litigation workers. For example, if high training firms are more likely to locate in high enforcing states and low training firms are equally likely to locate anywhere, then a random sample of workers from all states is more likely to sample workers from high training firms in high enforcing states. This type of sorting biases upward the intent-to-treat estimate. In order to address the extent to which this type of sorting is occurring, I divide the sample based on the tradability of the good sold by the worker's firm. Some firms, such as hairdressers or other personal service firms, sell highly non-tradable goods because their client base and markets are local in nature. These types of firms have no choice but to operate where their client base is located. Others, such as manufacturing and consulting firms, can sell their product from any state and therefore can move towards higher enforcing states. I rely on Jensen and Kletzer (2005) to divide industries into tradable and non-tradable categories.[44] The correlation between enforcement and a tradable dummy is -0.013, which provides some evidence that this type of sorting is not happening. In columns (3) and (4) of Table 1.9 I re-run the baseline specification for tradable and non-tradable industries. The results show that while the impact is stronger in tradable industries, the difference is not statistically different from the impact in non-tradable industries.

[44] Appendix 1.8.4 shows the tradable versus non-tradable breakdown by industry.

Table 1.9: Skill-Related Training and Tradability Robustness Checks

	Not-Profit (1)	Law (2)	Tradable (3)	Non-tradable (4)
Enforcement*Not-Profit High Lit.	-0.002			
	(0.005)			
Enforcement*Law		0.008		
		(0.020)		
Law		0.017		
		(0.102)		
Enforcement*High Lit.			0.007*	0.005
			(0.004)	(0.003)
Observations	38,264	30,307	37,226	33,148
R-squared	0.112	0.092	0.103	0.088

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses, clustered at the state level. The dependent variable is an indicator equal to 1 if the worker received firm-sponsored training in the past year. High Lit. is a dummy equal to one if the worker is a high litigation worker. Columns (1) and (2) run the difference-in-differences specification comparing not-for-profit high litigation workers to for-profit low litigation workers. Columns (3) and (4) compare lawyers to other for-profit low litigation workers. Column (5) divides industries into tradable and non-tradable based on Jensen and Kletzer (2005) and looks for heterogenous treatment effects using a triple difference. The set of controls are the same as the baseline specification, discussed on page 182.

Variations in the Enforcement Index and Excluding Extreme States

I next test whether the results are robust to variations of the factor analysis enforcement index, using alternative enforcement indices, discretizing the enforcement index, and excluding states at the ends of the distribution. Unfortunately, limitations of the data preclude using variation in enforcement over time within a state as an additional robustness check. The Michigan reversal of 1985 studied in Marx et al. (2009) occurs well before the start of the data, and the major changes identified by Garmaise (2011) occur in 1996 in Florida and 1994 in Texas, which do not allow for a 'pre' treatment period. The temporary Louisiana reversal from June 2001 to 2003 results in 104 treated individuals in my sample, which is too small for reliable inference. Other major changes in Oregon and New York in 2008 occur at the end of the data's time frame resulting in no post-period for a difference-in-difference estimation. Given the lack of an adequate quasi-natural experiment, the only longitudinal variation I can use is the potentially endogenous variation that comes from the Bishara index, which captures the enforcement landscape in 1991 and 2009. The challenge with this approach is not knowing when the various changes occurred and appropriately dealing with imputation for missing values. Due to this ambiguity and the limitations of my data, any longitudinal estimates would be highly objectionable.

In order to examine whether or not my results are driven by the way I chose the factor analysis weights and scores, I re-run the training regressions using variations of the enforcement index: the 1991 scores and weights, the 2009 scores and weights, the index with 1991 scores assigned to year 1996 and the 2009 scores assigned to all other years using the 1991 weights. These results are presented in *Panel A* of Table 1.10. They show the results are robust to whatever variation of the factor analysis weights I choose.

In *Panel B* of Table 1.10, I consider how the baseline estimates change if instead I utilize Bishara's (2011) index from 1991 and 2009, the 1992 and 2001 indices developed by Garmaise (2011), or the method developed by Lubotsky and Wittenberg (2006).[45] The Lubotsky-Wittenberg method takes the linear regression of training on the six individual enforcement dimension from Bishara (2011) and uses the coefficients on the dimensions as weights in a weighted sum of the dimensions of enforcement to generate the linear factor which best mitigates attenuation bias. In this scenario, however, factor analysis is preferable for two reasons: (1) It is not clear how to extend the Lubotsky-Wittenberg method to a difference-in-differences context,[46] and (2) the weights on the dimensions of enforcement are

[45] Garmaise considers various dimensions of non-compete enforcement for each state using the same Malsberger text as Bishara (2011), though he assigns each dimension a binary score and simply adds them up. See Appendix 1.8.3 for a complete description of the questions Garmaise considers.

[46] To generate their index, I run a regression of training on the dimensions of enforcement and other individual characteristics without state fixed effects and no interaction of the dimensions. I take the coefficients from the dimensions of enforcement and use them as weights in the weighted sum to generate an index of

negative in some instances, which defeats the purpose of making an index that reflects true 'enforceability' within a state.

The robustness checks show that there is very little difference between the factor analysis estimates and the Bishara indices. This is not unexpected, since the correlation between these indices is greater than 0.93. The Garmaise index points in the expected direction but is insignificant. These differences are not unexpected because the Garmaise index varies less than the factor analysis index.[47] Furthermore, the generated factor analysis index has two benefits over the Garmaise index which allow it more precision: (1) It is more finely coded, and coded by a lawyer specializing in non-compete case law, and (2) the arbitrary weights chosen by Garmaise may overemphasize the importance of various dimensions of non-compete enforcement, while the factor analysis generates weights which account for the covariation in the dimensions. The differences with the Lubotsky and Wittenberg index are also relatively small.

I next discretize enforcement into high enforcement and low enforcement, with zero as the dividing line.[48] This check allays fears that the linear specification of the enforcement index is inappropriate. The intent-to-treat estimate from the baseline specification is 0.018 with a standard error of 0.009 and a corresponding p-value of 0.05. This corroborating result is not surprising given the data in Figure 1.5.4.

In addition to worries about the validity of the index, one might be concerned about the fact that California and Florida, which represent the ends of the enforcement distribution might be driving the results.[49] To address these concerns, I re-run the baseline specification

enforcement. I then standardize this generated index by subtracting the mean and dividing by the standard deviation. The results presented in Table ?? use this index in the preferred difference-in-differences specification.

[47]The Garmaise index has a standard deviation of 1.17 and the factor analysis index has a standard deviation of 1.32.

[48]The index was standardized to be mean zero with standard deviation of 1 in a sample where each state is given a weight of zero.

[49]Excluding North Dakota does not affect the results because it includes only 79 observations, which

without California, Florida, or both. The results are presented in *Panel C* of Table 1.10. As expected from Figure 1.5.4, the results show that the common effect for high litigation occupations is robust to the exclusion of both California and Florida.

constitutes 0.1% of the sample.

Table 1.10: Index and State Exclusion Robustness Checks

Panel A: Factor Analysis Index Specification Robustness Check

Intent-to-Treat Effect	Baseline	91SW/09SW	91SW/09S	91SW	09SW	91S/09W	91W/09S
High Litigation	0.007**	0.006**	0.004*	0.005*	0.007**	0.006**	0.004*
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
Observations	70,374	70,374	70,374	70,374	70,374	70,374	70,374

Panel B: Other Non-Compete Indices Robustness Check

		Bishara Index		Garmaise Index		Lubotsky-Wittenberg	
Intent-to-Treat Effect	Baseline	1991	2009	1992	2001	1991	2009
High Litigation	0.007**	0.007**	0.006**	0.002	0.002	0.007**	0.008***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	70,374	70,374	70,374	70,374	70,374	70,374	70,374

Panel C: State Exclusion Robustness Check

Intent-to-Treat Effect	Baseline	No CA	No FL	No CA, FL
High Litigation	0.007**	0.011**	0.007**	0.012*
	(0.003)	-0.005	(0.003)	(0.007)
Observations	70,374	62,880	66,964	59,470

Note: *** p<0.01, ** p<0.05, * p<0.1. The reported estimates are the intention-to-treat coefficients on high litigation occupations. Robust standard errors are in parentheses, clustered at the state level. The dependent variable is an indicator equal to one if the worker received firm-sponsored training in the last year. The omitted group is low litigation occupations. In the column headings, Baseline refers to the index using the 2009 factor analysis weights where the 1996 panel receives the 1991 scores and the 2001, 2004, and 2008 data receive the 2009 scores. In *Panel A*, the letters S and W refer to the Scores and Weights from the respective years which were used in the construction of the alternative index. For example, 91SW/09SW refers to using the 1991 scores and weights for data in 1996 and 2009 scores and weights for data from 2001, 2004, and 2008. In *Panel B*, Bishara 1991 and 2009 use his aggregated index for those years (with his weights), and the same for Garmaise 1992 an 2001. The Lubotsky-Wittenberg refers to using their 2006 method with the individual dimensions from Bishara (2011). In *Panel C*, the specification simply drops the noted states. The set of controls in each specification are the same as the baseline specification discussed on page 182.

1.7 Conclusion and Policy Implications

The skills of the workforce are important for economic growth and productivity, and the government has long recognized this (Heckman et al. 1999). Concerns about underinvestment in firm-sponsored training are commonplace because firms do not want their workers to leave and utilize any training they provided for another firm, especially a competitor. The enforcement of covenants not to compete is a legal labor market friction which restricts the flow of workers across competitors and increases firms' incentives to provide employee training.

To examine the impact of non-compete enforcement on firm-sponsored training, I extend the classic on-the-job training model to incorporate non-compete enforcement and on-the-job-search. The model shows that the common assertion that higher non-compete enforcement increases firm-sponsored training outcomes relies upon the contractibility of training. The model also points out that if workers adhere to their contracts because they believe them to be enforceable or because they feel ethically bound to abide by them, then additional non-compete enforcement should not impact firm-sponsored training.

In order to empirically examine the impact of non-compete enforcement, I first improve upon the Bishara (2011) index of non-compete enforcement by generating weights to aggregate his scores using factor analysis. Comparing occupations likely to experience non-compete litigation to occupations less likely to experience such litigation, I find that a one standard deviation increase in the non-compete enforcement index increases training for high litigation occupations by 3% relative to low litigation occupations. This relative impact is between 3% and 8% for each of the first 20 years of tenure, and between 2 and 7% for workers aged 22 to 42. Importantly, the impact of enforcement on training rises monotonically in the first 20 years of tenure. This result provides evidence that indeed the not-contractible model is appropriate for training later in tenure, suggesting that there is a role for non-compete enforcement in reducing the positive externality to training.

These positive training effects tend to be localized in high skill and high earning occupations such as computer and mathematical occupations, healthcare practitioners, and managers, though personal care and service occupations are also profoundly affected, presumably because of their interactions with clients. Courts that wish to improve training outcomes via non-compete enforcement should considering reducing the burden of proof on the plaintiff, or alternatively requiring additional consideration for non-competes signed after the inception of employment. Laws such as the one currently under consideration in Massachusetts's which only enforces non-competes lasting more than 6 months on employees earning more than $250,000, and Colorado's law enforcing non-competes only on upper-management employees, exploit the training benefits these occupations receive without hurting workers in occupations in which there are little to no training benefits from enforcement. The training effects coincide with an enforcement effect on the potential experience of new hires: firms in lower enforcing states tend to hire more experienced workers, presumably because they are unwilling to bear their training costs.

Legal scholars have recently been advocating for lower enforcement (Hyde 2003, Lobel 2013) based on studies which focus on the negative impacts of non-competes, but these positions are empirically premature because little is known about the ways in which firms and different types of workers are benefitting from enforcement. While non-competes have been shown to harm worker mobility (Marx et al. 2009, Garmaise 2011, Lavetti et al. 2011), and reduce innovation associated with increases in venture capital (Samila and Sorenson 2011), Lavetti et al. (2011) find that doctors who sign non-competes earn more because they are entrusted with more clients, and Marx and Younge (2013) find that enforcement increases Tobin's q. Recent work by Balasubramanian et al. (2013) shows that the life-cycle effects of non-competes on new ventures are heterogeneous based on whether the new venture is a within-industry spin-out or not. Specifically, while within industry spin-outs have lower entry rates in higher enforcing states, the ones that form in higher enforcing states tend to start larger, grow faster, and live longer than other new ventures. This paper finds that some occupations in higher enforcing states do receive more skill upgrading paid for by the firm. Whether or not reducing enforcement is welfare enhancing is a challenging question because of the vastness of the impacts of these agreements. More empirical work on both the costs and benefits of enforcement is needed.

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1.8 Appendix: Training the Enemy?

1.8.1 Legal Literature Review of Non-Competes

The enforcement of non-compete agreements has varied greatly throughout its 600 year history, balancing the values of freedom of contract, personal economic freedom, and business ethics. The history begins in Europe's craft guild system in the 15th century, where master craftsmen sought to prevent their apprentices from competing for their business (Blake 1960). The first known legal case dealing with restrictions on the practice of a craft is the *Dyer's Case* of 1414, where a dyer reneged on his obligation to not compete with the man to whom he sold his business. The first courts litigating covenants not to compete took an anti-enforcement stance, primarily because it was clear that master craftsmen were seeking to prevent product market competition from budding apprentices. Courts have long held the belief that enforcing non-competes reduces competition both in the product market and in the labor market, and this has been routinely cited as a rationale for not enforcing non-competes (Anenson 2005, Blake 1960).

The Reasonableness Argument

In *Mitchell vs. Reynolds* in 1711, the court developed the first test of reasonability, which would later be refined in the 1932 *Restatement of Contracts* and adopted by many US states. Notably, California and North Dakota have never adopted any kind of reasonableness test and refuse to enforce almost all non-competes. The "reasonableness criterion" holds that a restraint is reasonable only if it (1) is no greater than is required for the protection of the employer, (2) does not impose undue hardship on the employee, and (3) is not injurious to the public.

Implicit in (1) is an understanding of why firms need protection in the first place: Pro-enforcement scholars suggest that by allowing the unfettered flow of workers who posses valuable information courts might be dismantling current or future competition instead of protecting it because of the reduced incentive to innovate. Of states that enforce non-competes, most agree that confidential information, client lists, and trade secrets can serve as a basis for the enforcement of a non-compete. To be precise, the definition of trade secrets from the Uniform Trade Secrets Act is

> *... information, including a formula, pattern, compilation, program, device, method, technique, or process, that: (i) derives independent economic value, actual or potential, from not being generally known to, and not being readily ascertainable by proper means by, other persons who can obtain economic value from its disclosure or use, and (ii) is the subject of efforts that are reasonable under the circumstances to maintain its secrecy.*

This definition of a trade secret is broad enough to include even advertising and marketing strategies.

The courts also consider the harm done to the worker in the protection of the firm's interests. For workers with limited opportunities, the courts have recognized the resulting lack of ability to earn a living. Famously, the court in *Arthur Murray Dance Studios, Inc. v. Litten* declared

> *The average, individual employee has little but his labor to sell or to use to make a living. He is often in urgent need of selling it and in no position to object to boiler*

> *plate restrictive covenants placed before him to sign. To him, the right-to-work and support his family is the most important right he possesses. His individual bargaining power is seldom equal to that of his employer.*

Legal scholars on both sides of the enforcement debate (Arnow-Richman 2001, Stone 2002, Callahan 1985) note the importance of whether or not the worker has had the chance to bargain over the non-compete. Indeed, Malsberger's series on non-compete enforcement by state regards with great importance the enforcement of non-competes signed after the employment relationship has begun, when the worker presumably has no outside option (Malsberger 1996, Leibman and Nathan 1987).

The last consideration of the reasonableness criterion, is how injurious enforcement would be to society. The historical view of this consideration is that if the worker's occupation is important enough to society, such as a doctor, then preventing that worker from employing his craft may have significant ill effects on the society as a whole. This portion of the reasonableness criterion is cited much less often as a reason for not enforcing the covenant: Whitmore (1990) found that in his survey of 105 randomly selected non-compete cases, only 19 mentioned injury to the public in their decisions. Some occupations, notably lawyers, broadcasters, and doctors have been explicitly carved out of non-compete enforcement in some states (Stroud 2002).

The Pro-Enforcement Argument

Pro-enforcement scholars (Callahan 1985, Sterk 1993) argue that non-competes should generally be enforced because the reasonableness criteria are no longer valid. They argue that non-competes actually enhance competition because they encourage firms to invest in research and development and human capital. Furthermore, allowing parties to contract with each other internalizes any externalities. Sterk (1993) writes:

> *Standard Coasean theory would suggest that no one benefits from inalienability rules, at least in the absence of transaction costs, because this benefit enjoyed by persons well-endowed with human capital is offset by a loss of freedom to enter into mutually beneficial exchanges that involve alienating human capital.*

While negotiation and renegotiation provide the first best solution in a world with perfect information, one wonders whether workers are able to correctly forecast the value their knowledge and skills that they will develop at the firm.

In addition, Callahan (1985) compares a non-compete to a ten year exclusive supply contract from a manufacturer to a retailer. She argues that the supply contract is not considered anticompetitive because there is ex ante competition for such a contract. Why, she asks, should non-compete contracts be enforced differently, especially since for the types of workers who will likely sign non-competes there is likely such competition at the hiring stage and these workers are sophisticated enough to be able to bargain ex ante over the terms of the contract?

At the time of Callahan's writing, it was unknown what types of workers signed non-competes, but occupations as strange as manicurists, carpet installers, liquor deliverymen, bartenders, cosmetologists, pest exterminators, garbage collectors, janitors, night-watchmen, undertakers, and security guards have been litigated for non-compete violations. Given their broad coverage, Callahan's claim that only certain types of workers would be willing to sign non-competes is unsubstantiated.

The Anti-Enforcement Argument

Hyde (1998, 2003) and Gilson (1999) argue that the restriction of labor mobility is excessively costly because spillovers provide the necessary ingredients for growth, pointing to Silicon

Valley as a success story. Hyde (1998) argues that even in states that do not enforce non-competes, firms still have enough incentives to invest in human capital and research and development so that they can be first movers in the market. Gilson (1999) cites the growth of Silicon Valley as an argument to lessen the enforcement of non-compete laws, though with some caveats. He argues that the lack of non-compete enforcement in California provides a coordination mechanism which prevents a prisoner's dilemma: if all firms would be better off if information were shared via employee mobility, then letting workers leave is not incentive compatible if firms can keep the worker via enforceable non-competes. He claims that the reason Silicon Valley has been able to reinvent itself over and over again relative to Route 128 is because workers are free to move between firms.

Fallick, Fleischman, and Rebitzer (2006) consider Gilson's propositions by analyzing mobility decisions in the CPS. They try to answer three questions: (1) Is the interfirm mobility of employees in the computer industry indeed higher in Silicon Valley than in other IT clusters in states with enforceable non-competes. (2) Is there a California effect on the rate of interfirm mobility for computer industry employees? (3) Do mobility patterns observed in the computer industry hold for employees in the same location who are not employed in the IT industry They restrict sample to male, 4 year college educated workers, living in metropolitan areas having IT clusters. The sample is from 1994 to 2001 and is pooled to get a large number of observations. They find evidence that job hopping is higher in Silicon Valley, that controlling for California reduces the significance of Silicon Valley effect, suggesting that there is a California effect. They find that there are also relatively high mobility rates in LA and San Diego within the IT industry, though within-industry job changes are nearly 90% greater in Silcon Valley than the mean. Outside of the IT sector, there is little evidence that job changes are more likely in California; indeed they seem to be lower in California than elsewhere in the nation. This last bit of evidence suggests that agglomeration economies are indeed an important part of the industrial cluster story.

Recent developments in January 2013 call into question firms' responses to the lack of non-compete enforcement in Silicon Valley. Specifically, there have developed "gentleman's agreements" between major tech employers in Silicon Valley whereby firms agree to not actively recruit employees of other companies (Blagdon 2013). It is unclear if these agreements have developed as a result of the lack of non-compete enforcement, or if they exist in areas where non-compete agreements are more likely to be enforced. More research in this area is warranted.

Other Trade Secret and Non-Solicitation Agreements

Non-competes are not the only method for protecting confidential information, though they do possess certain desirable qualities. While employed, every employee has a fiduciary duty not to compete against his employer by providing information to competitors or by recruiting clients to join them in a new venture. This fiduciary duty ends, however, when the worker leaves the firm. To prevent the disclosure of trade secrets and the loss of valuable clientele, most jobs that involve signing a non-compete also involve signing a non-disclosure agreement and also potentially a non-solicitation and/or a non-poaching agreement. The non-disclosure agreement precludes the worker from sharing trade secrets of their previous employer with the new employer. The difficulty with these contracts, however, is that it can be difficult in court to prove that trade secrets have been leaked. Non-solicitation and non-poaching agreements are meant to restrict the employee from soliciting clients or employees from the previous employer for a certain amount of time. The difficulty with these contracts is that it can be hard to verify which party solicited which party, for clients can choose which provider of services they wish to use, and employees employed "at will" can leave whenever they want for whatever reason they want. The benefit of utilizing non-compete agreements in addition to these alternatives is that non-competes prohibit the move to a competitor in the first place, thus preventing the worker from sharing trade secrets or taking clientele.

Additionally, courts have employed (though seldom) the doctrine of inevitable disclosure to prevent an employee from working for a competitor when it would not be possible for the worker to perform his new job without disclosing confidential information learned during his previous employment relationship. The typical criticism of inevitable disclosure is that it is essentially a non-compete over which the employee was not allowed to bargain (Garrison and Wendt 2008). For this reason, courts have been hesitant to use it, though the often cited case of *PepsiCo v. Redmond* demonstrates how broadly it can be applied (Hyde 2003). Non-compete enforcement may be presumed to be a better alternative to inevitable disclosure because it has the benefit of consideration: at least the employee knowingly signed and potentially bargained over the terms of the agreement.

1.8.2 Proofs and Efficiency

Proof Of Proposition 1

Proposition 1: For a given enforcement level, λ, optimal training levels are higher when training is contractible at the hiring stage, $T_c^*(\lambda) > T_{nc}^*(\lambda)$.

Proof. The marginal cost of training is the same in the contractible and not contractible cases. Because $c''(T) > 0$ and $y''(T) < 0$, finding which case has a higher marginal benefit for each level of training. The marginal benefit of training under the contractible case and not contractible cases are

$$MB_c(T,\lambda) = y'(T)\Big(G(\hat{a}) + (1-\lambda)\int_{\hat{a}}^{\bar{a}} ag(a)da\Big)$$
$$MB_{nc}(T,\lambda) = y'(T)G(\hat{a})(1-\beta)(1-(1-\lambda)\mathbf{E}[a])$$

Proof by contradiction. Assuming that $MB_{nc}(T,\lambda) > MB_c(T,\lambda)$, then

$$G(\hat{a})(1-\beta)(1-(1-\lambda)\mathbf{E}[a]) > G(\hat{a}) + (1-\lambda)\int_{\hat{a}}^{\bar{a}} ag(a)da$$
$$-G(\hat{a})\beta(1-(1-\lambda)\mathbf{E}[a]) > (1-\lambda)\int_{\hat{a}}^{\bar{a}} ag(a)da$$

which is a contradiction. □

Efficiency

Two questions of great importance from the perspective of state judiciaries are, given the worker's quit decision and the firm's training decision, what level of non-compete enforcement maximizes total surplus, and how do the resulting training and movement decisions compare to the efficient outcomes? There are two ways to think about efficient outcomes in this case: (1) Outcomes from an unconstrained social planner, and (2) outcomes from a social planner constrained by the informational structure of the model.

Consider first an unconstrained social planner.[50] The social planner makes the worker quit as long as the competitor firm is more productive than the incumbent firm, $a > 1$. In addition, since the social planner is able to perfectly forecast the productivity of the competitor firm, he makes the training choice at the initial firm based on maximizing either $y(T) - c(T)$ if $a \leq 1$, or $ay(T) - c(T)$ if $a > 1$. With this completely unconstrained social planner it is clear

[50]By unconstrained, I mean the social planner has perfect foresight and is not constrained by the informational structure of the model

any non-zero non-compete enforcement is welfare reducing.

Contrasting with these unconstrained efficient outcomes are the outcomes which occur when the social planner is constrained by the timing and information structure of the model. Specifically, the constrained social planner initially chooses the non-compete enforcement level, then must make the training decision given his initial choice of non-compete enforcement, without knowing the productivity of the competitor firm, and then must decide when the worker will quit. Intuition developed from the unconstrained social planner case suggests that the constrained efficient non-compete enforcement level is zero. This makes sense because increasing non-compete enforcement reduces the ability of the worker to move, but does not incentivize the social planner to train the worker more, or reduce inefficient quits. I show this formally in the appendix, but present only the setup and the three choices of the constrained social planner.

If the social planner does not know what kind of firm the worker will meet in advance, then the problem becomes one of maximizing expected surplus by choosing the non-compete enforcement level, the amount of training, and when to quit. In this context, given a chosen non-compete enforcement level, the social planner will make the worker quit in the poaching period when $(1-\lambda)a > 1$, that is when the productivity of the worker at the competitor firm is greater than 1 in expectation, which leads to an efficient quit probability of $1 - G(\frac{1}{1-\lambda})$. Given the previously chosen level of λ and the optimal quit probability of $1 - G(\frac{1}{1-\lambda})$, the efficient training choice can be written as:

$$T^*(\lambda) = \underset{T}{\operatorname{argmax}} \quad G(\frac{1}{1-\lambda})y(T) + (1 - G(\frac{1}{1-\lambda}))(1-\lambda)\mathbf{E}[a|a > \frac{1}{1-\lambda}]y(T) - c(T)$$

The social planner knows he will choose training level $T^*(\lambda)$ given his initial choice of λ.

Working backward, the optimal choice of λ is given by

$$\lambda^* = \underset{\lambda}{\operatorname{argmax}} \quad \mathbf{E}[S(T^*(\lambda), \lambda)]$$

where $S(T, \lambda)$ is the total surplus function. The resulting optimal choice of λ is indeed zero for all choices of $G(a)$. The choices of a constrained social planner are summarized in the following proposition:

Proposition 4. *The constrained efficient non-compete enforcement level, λ^*, training level, T^*, and quit decision are given by:*

$$\begin{aligned} \lambda^* &= 0 \\ y'(T^*)(G(1) + \int_1^{\bar{a}} ag(a)da) &= c'(T^*) \\ P(Quit) &= 1 - G(1) \end{aligned} \tag{1.8.1}$$

Proof. Define total surplus as:

$$\begin{aligned} S(T, \lambda) &= G(\frac{1}{1-\lambda})y(T) + (1 - G(\frac{1}{1-\lambda}))(1-\lambda)\mathbf{E}[a | a > \frac{1}{1-\lambda}]y(T) - c(T) \iff \\ S(T, \lambda) &= y(T)\left(G(\frac{1}{1-\lambda}) + (1-\lambda)\int_{\frac{1}{1-\lambda}}^{\bar{a}} ag(a)da\right) - c(T) \end{aligned}$$

The optimal choice of T solves $T^*(\lambda) = \underset{T}{\operatorname{argmax}} \quad S(T, \lambda)$ and the optimal choice of λ solves $\lambda^* = \underset{\lambda}{\operatorname{argmax}} \quad S(T^*(\lambda), \lambda)$. Using the envelope theorem and Leibniz Rule, first show that $\lambda^* = 0$ directly:

$$\begin{aligned} \frac{\partial S(T^*(\lambda), \lambda)}{\partial \lambda} &= y(T^*(\lambda))\left(g(\frac{1}{1-\lambda})\frac{1}{(1-\lambda)^2} - \int_{\frac{1}{1-\lambda}}^{\bar{a}} ag(a)da + (1-\lambda)(-\frac{1}{1-\lambda}g(\frac{1}{1-\lambda})\frac{1}{(1-\lambda)^2})\right) \\ \frac{\partial S(T^*(\lambda), \lambda)}{\partial \lambda} &= -y(T^*(\lambda))\int_{\frac{1}{1-\lambda}}^{\bar{a}} ag(a)da \ \leq 0 \end{aligned}$$

The total surplus is downward sloping for all $\lambda \in [0,1]$, implying that the maximum occurs where $\lambda = \lambda^* = 0$. Given this, the optimal training decision is given by the first order condition:

$$y'(T^*)(G(1) + \int_1^{\bar{a}} ag(a)da) = c'(T^*)$$

$\square$

Consider next the practical question facing state legislatures today: Given how firms will train their workers and how workers will make quit decisions, what is the optimal non-compete enforcement level? How do the resulting training and quit decisions compare to each other and the constrained efficient choice? To distinguish between the various actors, in this section the agent making the non-compete enforcement choice will be called the government, from which it is implied that the state legislatures pick a particular enforcement policy, but cannot force the firm to train a certain level or force the worker to quit.

Case 1: Contractible Training

Consider first the case where firms compete by offering $\{W, T\}$ contracts in the first period. The government chooses the optimal non-compete enforcement level taking into account the chosen training level, denoted $T_c^*(\lambda)$ from (1.4.6), and the quit probability determined by the worker from (1.4.3), $1 - G(\hat{a})$. The government's problem is formally written as:

$$
\begin{aligned}
\max_{\lambda} \quad \mathbf{E}[S(T_c^*(\lambda), \lambda)] =& G(\hat{a}(\lambda)) y(T_c^*(\lambda)) \\
& + (1 - G(\hat{a}(\lambda)))(1 - \lambda)\mathbf{E}[a|a > \hat{a}] y(T_c^*(\lambda)) - c(T_c^*(\lambda)) \\
s.t. \quad c'(T_c^*(\lambda)) =& y'(T_c^*(\lambda)) \Big(G(\hat{a}(\lambda)) + (1 - G(\hat{a}(\lambda)))(1 - \lambda)\mathbf{E}[a] \Big) \\
G(\hat{a}(\lambda)) =& pr(a < \frac{\beta}{1 - \lambda} + (1 - \beta)\mathbf{E}[a])
\end{aligned}
$$

Fortunately, the training condition solves $T_c^*(\lambda) = \underset{T}{\operatorname{argmax}}\, \mathbf{E}[S(T, \lambda)]$ given the worker's quit decision, where $S(T, \lambda)$ is the total surplus function. This allows the use of the envelope theorem when choosing the optimal enforcement level. The optimal choice of λ, denoted λ_c^* solves:

$$
y(T_c^*(\lambda_c^*)) \left(\frac{\beta}{(1 - \lambda_c^*)^2} g(\hat{a}_c^*)(1 - (1 - \lambda_c^*)\hat{a}_c^*) - \int_{\hat{a}_c^*}^{\bar{a}} a g(a) da \right) = 0 \quad \text{if} \ \lambda_c^* \in [0, 1] \tag{1.8.2}
$$

Proposition 5. *Optimal non-compete enforcement levels are weakly constrained inefficient when training is contractible,* $\lambda_c^* \geq 0$.

The intuition is that non-compete enforcement balances the tendency for inefficient quits and the inability to move to high productivity firms. If there is a high likelihood of an inefficient quit then optimal non-compete enforcement will be greater than zero. If there is a small likelihood of an inefficient quit then optimal enforcement will be zero.

Proof. The first order condition for optimal non-compete enforcement in the contractible case is given by

$$
y(T_c^*(\lambda_c^*)) \left(\frac{\beta}{(1 - \lambda_c^*)^2} g(\hat{a}_c^*)(1 - (1 - \lambda_c^*)\hat{a}_c^*) - \int_{\hat{a}_c^*}^{\bar{a}} a g(a) da \right) = 0 \quad \text{if} \ \lambda_c^* \in [0, 1]
$$

Comparing this expression to the first order condition for enforcement for the constrained social planner reveals an additional term positive term:

$$\frac{\beta}{(1-\lambda_c^*)^2} g(\hat{a}_c^*)(1-(1-\lambda_c^*)\hat{a}_c^*) \ \geq 0$$

This positive term represents the marginal benefit of higher enforcement which is that it can prevent inefficient quits. For a given $g(a)$, if the likelihood of an inefficient quit is high enough, then this term will be positive enough to generate a non-zero enforcement level.

In order ensure a maximum, an optimally non-zero enforcement level must yield a negative second order condition. The second order condition is given by:

$$\left.\frac{\partial^2 S(T_{nc}^*(\lambda),\lambda)}{\partial \lambda^2}\right|_{\lambda=\lambda_c^*} = \left(g'(\hat{a}_c^*)\frac{\beta^2}{(1-\lambda_c^*)^4} + g(\hat{a}_c^*)\frac{2\beta}{(1-\lambda_c^*)^3}\right)(1-(1-\lambda_c^*)\hat{a}_c^*) + g(\hat{a}_c^*)\left(\frac{\beta^2}{(1-\lambda_c^*)^3} + \frac{2\beta(1-\beta)\mathbf{E}[a]}{(1-\lambda_c^*)^2}\right) \tag{1.8.3}$$

All terms are positive except possibly $g'(\hat{a})$. The second order condition indicates that λ_c^* identifies a maximum only if $g'(\hat{a}) < 0$ and the corresponding term is more negative than the other positive terms. □

Given the optimal selection of λ from (1.8.2), the optimal training decision solves:

$$c'(T_c^*(\lambda_c^*)) = y'(T_c^*(\lambda_c^*))\left(G(\hat{a}(\lambda_c^*)) + (1-\lambda_c^*)\int_{\hat{a}_c^*}^{\bar{a}} a g(a)da\right) \tag{1.8.4}$$

How does the training level chosen compare to the constrained efficient choice?

Proposition 6. *Given training is contractible, training levels resulting from optimally chosen non-compete enforcement, $T_c^*(\lambda_c^*)$, are weakly constrained inefficient, $T_c^*(\lambda_c^*) \leq T^*$.*

The intuition for this proposition starts by noting that the marginal cost functions from the constrained social planner and the government are the same, but the marginal benefit functions from (1.8.4) and (1.8.1) are slightly different. Finding which marginal benefit function is greater will determine which scenario leads to a larger amount of training. The constrained social planner makes his training decision based on the probability the worker will move to a more productive firm. When training is contractible and non-compete enforcement is optimally non-zero, the chance the worker moves to a better firm is reduced relative to the constrained efficient case and thus the corresponding training decision is weighted more heavily towards the initial firm's output, resulting in training levels below the constrained efficient levels.

Proof. The proof is broken down into three levels: (1)$\mathbf{E}[a] \geq 1$, (2) $\mathbf{E}[a] < 1$ and $\lambda_c^* = 0$, and (3) $\mathbf{E}[a] < 1$ and $\lambda_c^* > 0$.

Consider first the case in which $\mathbf{E}[a] \geq 1$. In order for the firm to be willing to hire the worker, it must be that $y(T) \geq w(T)$, which boils down to $\mathbf{E}[a] \leq \frac{1}{1-\lambda}$.

I first show that if $\mathbf{E}[a] \geq 1$ then $\lambda_c^* = 0$. To show this, I need to show that:

$$
\begin{aligned}
&\frac{\partial S(T_c^*(\lambda), \lambda)}{\partial \lambda} \leq 0 \\
\iff\ & y(T_c^*(\lambda))\left(\frac{\beta}{(1-\lambda)^2} g(\hat{a})(1-(1-\lambda)\hat{a}) - \int_{\hat{a}}^{\bar{a}} a g(a) da\right) \leq 0 \\
\iff\ & \frac{\beta}{(1-\lambda)^2} g(\hat{a})(1-(1-\lambda)\hat{a}) \leq \int_{\hat{a}}^{\bar{a}} a g(a) da
\end{aligned}
\tag{1.8.5}
$$

If $\mathbf{E}[a] \geq 1$ then $\hat{a} = \frac{\beta}{1-\lambda} + (1-\beta)\mathbf{E}[a] \geq 1 + \frac{\lambda\beta}{1-\lambda}$.

$$\begin{aligned}
\hat{a} \geq& 1 + \frac{\lambda\beta}{1-\lambda} \\
\int_{\hat{a}}^{\bar{a}} ag(a)da \geq& \int_{\hat{a}}^{\bar{a}} (1 + \frac{\lambda\beta}{1-\lambda})g(a)da \\
=& \frac{(1-\lambda)^2 + \lambda\beta - \lambda^2\beta}{(1-\lambda)^2} \int_{\hat{a}}^{\bar{a}} g(a)da \\
\geq& \frac{\lambda\beta}{(1-\lambda)^2} \int_{\hat{a}}^{\bar{a}} g(a)da \\
\geq& g(\hat{a})\frac{\lambda\beta}{(1-\lambda)^2} \\
\geq& g(\hat{a})\frac{\lambda\beta}{(1-\lambda)^2}(1 - \hat{a}(1-\lambda))
\end{aligned}$$

This completes the proof that if $\mathbf{E}[a] \geq 1$ then $\lambda_c^* = 0$. In this situation, the firm will only hire a worker if $\mathbf{E}[a] \leq 1$. Therefore the only case of interest here is where $\mathbf{E}[a] = 1$, which implies that $\hat{a} = 1$. In this situation it is straightforward to see that the marginal benefit of training is the same as in the efficient case, leading to efficient training choice.

Next, consider the second case when $\mathbf{E}[a] < 1$ and $\lambda_c^* = 0$. In this case, $\hat{a} = \beta + (1-\beta)\mathbf{E}[a] < 1$. A direct comparison of the marginal benefits of training will show that the constrained efficient choice is weakly greater. The marginal benefit functions are given by:

$$\begin{aligned}
MB(T, 0) =& y'(T)\left(G(1) + \int_{1}^{\bar{a}} ag(a)da\right) \\
MB_c(T, \lambda_c^*) =& y'(T)\left(G(\hat{a}(\lambda_c^*)) + (1-\lambda_c^*)\int_{\hat{a}_c^*}^{\bar{a}} ag(a)da\right)
\end{aligned}$$

Given that $\lambda_c^* = 0$, $\mathbf{E}[a] < 1$, and $\hat{a} < 1$:

$$\begin{aligned} MB(T,0) >& MB_c(T,\lambda_c^*) \\ \iff G(1) + \int_1^{\bar{a}} ag(a)da >& G(\hat{a}) + \int_{\hat{a}}^{\bar{a}} ag(a)da \\ \iff G(1) - G(\hat{a}) >& \int_{\hat{a}}^1 ag(a)da \\ \iff \int_{\hat{a}}^1 G(a)da >& G(\hat{a})(1-\hat{a}) \end{aligned}$$

where the second to last line uses integration by parts and the last line is true because $G(a)$ is increasing in a and $\hat{a} < 1$.

The last case to show is when $\mathbf{E}[a] < 1$ and $\lambda_c^* > 0$. In this situation, the first and second order conditions hold, giving λ_c^* which solves

$$\frac{\beta}{(1-\lambda_c^*)^2} g(\hat{a}_c^*)(1-(1-\lambda_c^*)\hat{a}_c^*) = \int_{\hat{a}_c^*}^{\bar{a}} ag(a)da$$

To begin, I first prove that $\hat{a} < 1$ if $\mathbf{E}[a] < 1$ and $\lambda_c^* > 0$ by contradiction. Taking the above first order condition, solving for $\hat{a}$ and assuming that it is greater than 1 yields:

$$\begin{aligned} \hat{a} = \frac{1}{1-\lambda} - \frac{1-\lambda}{\beta} \frac{\int_{\hat{a}}^{\bar{a}} ag(a)da}{g(\hat{a})} >& 1 \\ \iff \lambda\beta g(\hat{a}) >& (1-\lambda)^2 \int_{\hat{a}}^{\bar{a}} ag(a)da \\ \implies \lambda\beta g(\hat{a}) >& (1-\lambda)\hat{a} \int_{\hat{a}}^{\bar{a}} g(a)da \\ \iff \lambda\beta g(\hat{a}) >& \beta \int_{\hat{a}}^{\bar{a}} g(a)da + (1-\beta)\mathbf{E}[a] \int_{\hat{a}}^{\bar{a}} g(a)da \end{aligned}$$

The last line is clearly a contradiction since $\beta \int_{\hat{a}}^{\bar{a}} g(a)da > \lambda\beta \int_{\hat{a}}^{\bar{a}} g(a)da$. Therefore $\hat{a} < 1$ if $\mathbf{E}[a] < 1$ and $\lambda_c^* > 0$.

To show that $T^*(0) > T_c^*(\lambda_c^*)$ I directly compare the marginal benefit functions, given now that $\hat{a} < 1$, $\lambda_c^* > 0$, and $\mathbf{E}[a] < 1$:

$$\begin{aligned} MB(T,0) >& MB_c(T,\lambda_c^*) \\ \iff G(1) + \int_1^{\bar{a}} ag(a)da >& G(\hat{a}) + (1-\lambda_c^*)\int_{\hat{a}}^{\bar{a}} ag(a)da \\ \iff G(1) - G(\hat{a}) >& \int_{\hat{a}}^1 ag(a)da - \lambda_c^* \int_{\hat{a}}^{\bar{a}} ag(a)da \\ \iff \int_{\hat{a}}^1 G(a)da >& G(\hat{a})(1-\hat{a}) - \lambda_c^* \int_{\hat{a}}^{\bar{a}} ag(a)da \end{aligned}$$

where the second to last line uses integration by parts and the last line is true because $G(a)$ is increasing in a and $\hat{a} < 1$.

This completes the proof that training is weakly constrained inefficient. □

One objection to this proposition is that counteroffering and Bertrand type competition in the poaching stage may ensue if the worker gets a better offer. If this were true, then the worker would never leave for a less productive firm because his initial firm could always outbid the other firm. In this situation, non-compete enforcement has no upside, the optimal enforcement choice would be to not enforce, and the constrained efficient outcomes would be achieved. There is little evidence suggesting that Bertrand type competition is actually occurring for most workers. As discussed in Barron et al. (2006), only 30% of firms were willing to make counteroffers for the most recently hired worker.

Case 2: Training Not Contractible

Consider the second case in which firms only contract on first period wages. In this situation, because the training decision is made via profit maximization instead of total surplus maximization, the envelope theorem cannot be used to simplify the analysis. The firm takes as given the non-compete enforcement level, and chooses its training level to maximize profits, subject to the quit condition of the worker. From above, the optimal training choice, $T^*_{nc}(\lambda)$, satisfies (1.4.9). The government, knowing that the firm will choose to train this amount, faces the following maximization problem:

$$\begin{aligned}
\max_{\lambda} \quad \mathbf{E}[S(T^*_{nc}(\lambda),\lambda)] &= G(\hat{a}(\lambda))y(T^*_{nc}(\lambda)) \\
&\quad +(1-G(\hat{a}(\lambda)))(1-\lambda)\mathbf{E}[a|a>\hat{a}]y(T^*_{nc}(\lambda)) - c(T^*_{nc}(\lambda)) \\
s.t. \quad c'(T^*_{nc}(\lambda)) &= G(\hat{a})y'(T^*_{nc})(1-\beta)(1-(1-\lambda)\mathbf{E}[a]) \\
G(\hat{a}) &= pr(a < \frac{\beta}{1-\lambda} + (1-\beta)\mathbf{E}[a])
\end{aligned}$$

Solving for the optimal λ is not as straightforward as before. The optimal non-compete enforcement level, λ^*_{nc}, solves:

$$\begin{aligned}
&y(T^*_{nc}(\lambda^*_{nc}))\left(g(\hat{a}^*_{nc})\frac{\beta}{(1-\lambda^*_{nc})^2}(1-(1-\lambda^*_{nc})\hat{a}^*_{nc}) - \int_{\hat{a}^*_{nc}}^{\bar{a}} ag(a)da\right)+ \\
&\frac{\partial T^*_{nc}(\lambda)}{\partial\lambda}\left(y'(T^*_{nc}(\lambda^*_{nc}))\left(G(\hat{a}^*_{nc})+(1-\lambda^*_{nc})\int_{\hat{a}^*_{nc}}^{\bar{a}} ag(a)da\right) - c'(T^*_{nc}(\lambda^*_{nc}))\right) = 0 \quad if\ \lambda^*_{nc} \in [0,1]
\end{aligned} \tag{1.8.6}$$

Comparing this first order condition to the first order condition in the contractible case (1.8.2), the difference is the second term. In the not contractible case, the positive second term reflects the fact that higher non-compete enforcement improves training outcomes which increases social surplus. As a result of this higher marginal benefit of enforcement, the optimal λ^*_{nc} in this case will be weakly higher than in the contractible case.

Proposition 7. *Optimal non-compete enforcement levels are weakly higher when training is not contractible,* $\lambda^*_{nc} \geq \lambda^*_c$.

Proof. The enforcement gradient of the total surplus function when training is contractible is given by:

$$\frac{\partial S(T_c^*(\lambda),\lambda)}{\partial\lambda} = y(T_c^*(\lambda))\left(\frac{\beta}{(1-\lambda)^2}g(\hat{a})(1-(1-\lambda)\hat{a}) - \int_{\hat{a}}^{\bar{a}} ag(a)da\right)$$

The enforcement gradient of the total surplus function when training is not contractible is given by:

$$\begin{aligned}\frac{\partial S(T_{nc}^*(\lambda),\lambda)}{\partial\lambda} =& y(T_{nc}^*(\lambda))\left(g(\hat{a})\frac{\beta}{(1-\lambda)^2}(1-(1-\lambda)\hat{a}) - \int_{\hat{a}}^{\bar{a}} ag(a)da\right)+ \\ & \frac{\partial T_{nc}^*(\lambda)}{\partial\lambda}\left(y'(T_{nc}^*(\lambda))\left(G(\hat{a})+(1-\lambda)\int_{\hat{a}}^{\bar{a}} ag(a)da\right) - c'(T_{nc}^*(\lambda))\right)\end{aligned}$$

Comparing the gradients at the optimal enforcement level when training is contractible yields:

$$\left.\frac{\partial S(T_c^*(\lambda),\lambda)}{\partial\lambda}\right|_{\lambda=\lambda_c^*} \leq \left.\frac{\partial S(T_{nc}^*(\lambda),\lambda)}{\partial\lambda}\right|_{\lambda=\lambda_c^*}$$

because in addition to reducing inefficient quits, in the not contractible case increased enforcement incentivizes to train more. Since the gradient of the total surplus function is always greater under the not contractible case, it will yield $\lambda_{nc}^* > \lambda_c^*$ if $\lambda_c^* > 0$ and $\lambda_{nc}^* \geq \lambda_c^*$ if $\lambda_c^* = 0$.

□

Consider now the training choice chosen by the firm when the government sets λ_{nc}^* via (1.8.6). The firm's training choice solves:

$$c'(T_{nc}^*(\lambda_{nc}^*)) = G(\hat{a}(\lambda_{nc}^*))y'(T_{nc}^*(\lambda_{nc}^*))(1-\beta)(1-(1-\lambda_{nc}^*)\mathbf{E}[a]) \qquad (1.8.7)$$

Suppose now that courts decide to enforce non-competes differentially and optimally depending upon whether the parties contracted on the training level. Which enforcement scheme leads to more training?

Proposition 8. *Optimal training levels resulting from optimally chosen non-compete enforcement levels are weakly higher when training is contractible, $T_c^*(\lambda_c^*) \geq T_{nc}^*(\lambda_{nc}^*)$.*

Proof. The proof proceeds by noting that $T_c^*(\lambda_c^*) > T_c^*(\lambda_{nc}^*)$ and then uses Proposition 1 to show $T_c^*(\lambda_c^*) \geq T_{nc}^*(\lambda_{nc}^*)$. In the contractible case, the impact of increased non-compete enforcement on training is given by:

$$\frac{\partial T_c^*(\lambda)}{\partial \lambda} = \frac{y'(T_c^*(\lambda))\Big(g(\hat{a})\frac{\beta}{(1-\lambda)^2}(1-(1-\lambda)\hat{a}) - \int_{\hat{a}}^{\bar{a}} ag(a)da\Big)}{c''(T_c^*(\lambda)) - y''(T_c^*(\lambda))\Big(G(\hat{a}) + (1-\lambda)\int_{\hat{a}}^{\bar{a}} ag(a)da\Big)} \tag{1.8.8}$$

In the case where $\lambda_c^* = 0$, it is also true from the first order condition that:

$$\left.\frac{\partial T_c^*(\lambda)}{\partial \lambda}\right|_{\lambda=\lambda_c^*} \leq 0$$

Since $\lambda_{nc}^* \geq \lambda_c^*$ then $T_c^*(\lambda_c^*) \geq T_c^*(\lambda_{nc}^*)$ and by Proposition 1, $T_c^*(\lambda_{nc}^*) > T_{nc}^*(\lambda_{nc}^*)$.

In the case where $\lambda_c^* > 0$ (and the second order condition holds), since $\lambda_{nc}^* > \lambda_c^*$ then $T_c^*(\lambda_c^*) > T_c^*(\lambda_{nc}^*)$ and by Proposition 1, $T_c^*(\lambda_{nc}^*) > T_{nc}^*(\lambda_{nc}^*)$. This completes the proof.

□

This last proposition is striking because it shows that the most effective way courts can encourage training is not necessarily increasing enforcement of non-competes, but instead encourage bargaining over training.

1.8.3 Enforcement Indices

Bishara 2011 Index

Question #	Question	Criteria	Question Weight
Q1	Is there a state statute that governs the enforceability of covenants not to compete?	10 = Yes, favors strong enforcement 5 = Yes or no, in either case neutral on enforcement 0 = Yes, statute that disfavors enforcement	10
Q2	What is an employer's protectable interest and how is that defined?	10 = Broadly defined protectable interest 5 = Balanced approach to protectable interest 0 = Strictly defined, limiting the protectable interest of the employer	10
Q3	What must the plaintiff be able to show to prove the existence of an enforceable covenant not to compete?	10 = Weak burden of proof on plaintiff (employer) 5 = Balanced burden of proof on plaintiff 0 = Strong burden of proof on plaintiff	5
Q3a	Does the signing of a covenant not to compete at the inception of the employment relationship provide sufficient consideration to support the covenant?	10 = Yes, start of employment always sufficient to support any CNC 5 = Sometimes sufficient to support CNC 0 = Never sufficient as consideration to support CNC	5
Q3b	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?	10 = Continued employment always sufficient to support any CNC 5 = Only change in terms sufficient to support CNC 0 = Neither continued employment nor change in terms sufficient to support CNC	5
Q3c	Will continued employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?	10 = Continued employment always sufficient to support any CNC 5 = Only change in terms sufficient to support CNC 0 = Neither continued employment nor change in terms sufficient to support CNC	5
Q4	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 covenant enforceable? If so, under what circumstances will the courts allow reduction and what form of reduction will the courts permit?	10 = Judicial modification allowed, broad circumstances and restrictions to maximum enforcement allowed 5 = Blue pencil allowed, balanced circumstances and restrictions to middle ground of allowed enforcement 0 = Blue pencil or modification not allowed	10
Q8	If the employer teminates the employment relationship, is the covenant enforceable?	10 = Enforceable if employer terminates 5 = Enforceable in some circumstances 0 = Not enforceable if employer terminates	10

Source: Bishara (2011).

Garmaise Index

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.

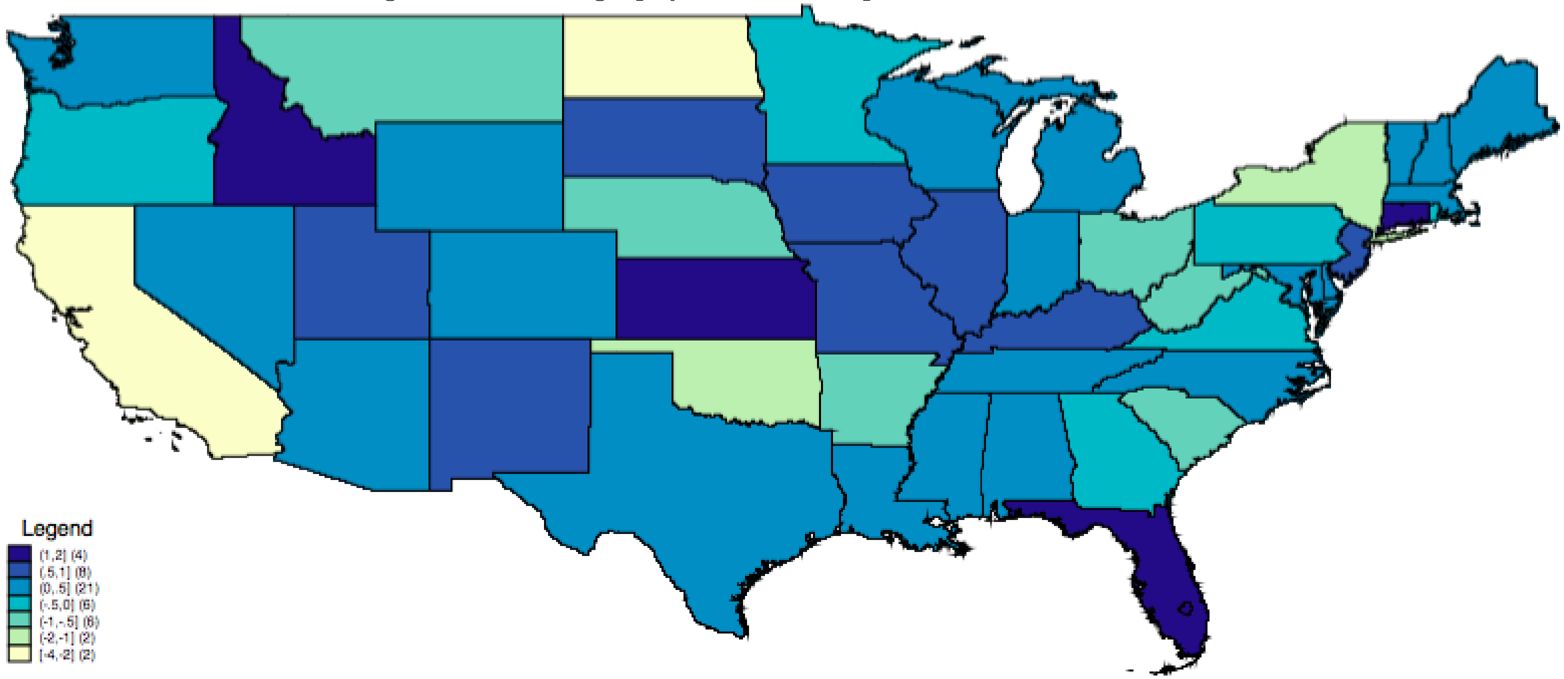
Question 12: What damages may an employer recover and from whom for breach of a covenant not to compete?

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.

1.8.4 Supporting Figures and Tables

Map of 2009 Enforcement

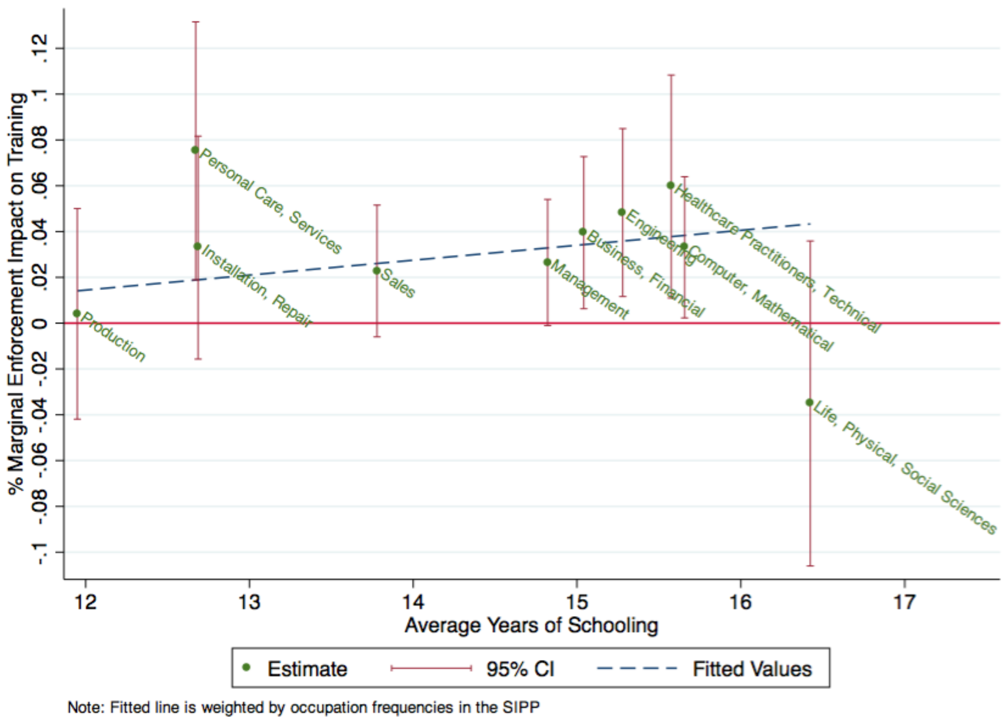
Figure 1.8.1: Geography of Non-Compete Enforcement in 2009



Characterizing the Occupation Effects

Is the impact of non-compete enforcement localized in occupations with high average earnings, schooling, on-the-job-training, tenure, or within-industry concentration? In Figures 1.8.2 to 1.8.7, I take the intent-to-treat estimates for each occupation of the high litigation group, divide them by the proportion of respondents reporting receiving training in their occupation and plot them against occupation specific averages of interesting variables. The lines of best fit are weighted by the frequency of occupations in the SIPP data.

Figure 1.8.2: Enforcement Impact on Training by Occupation and Education



Figures 1.8.2 1.8.3 show that there is a relatively strong positive relationship between average occupation-specific earnings and the impact of non-compete enforcement. Figure 1.8.4 shows that workers in high tenure occupations tend to have about the same training impact as workers with low average tenures. Figures 1.8.5 and 1.8.6 show an interesting contrast.

Figure 1.8.3: Enforcement Impact on Training by Occupation and Earnings

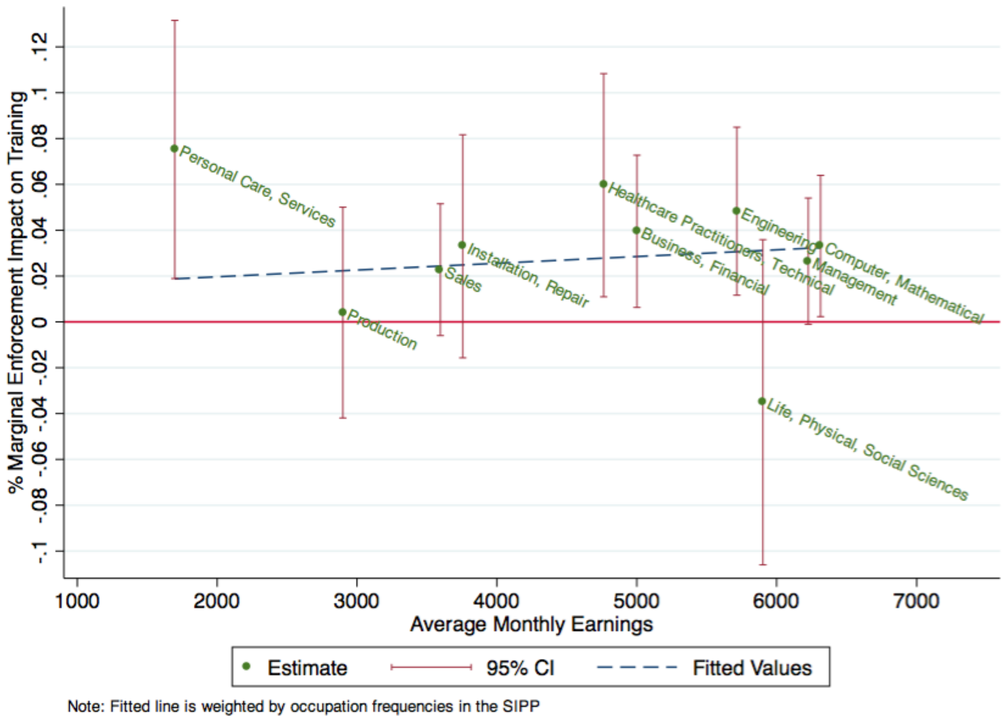
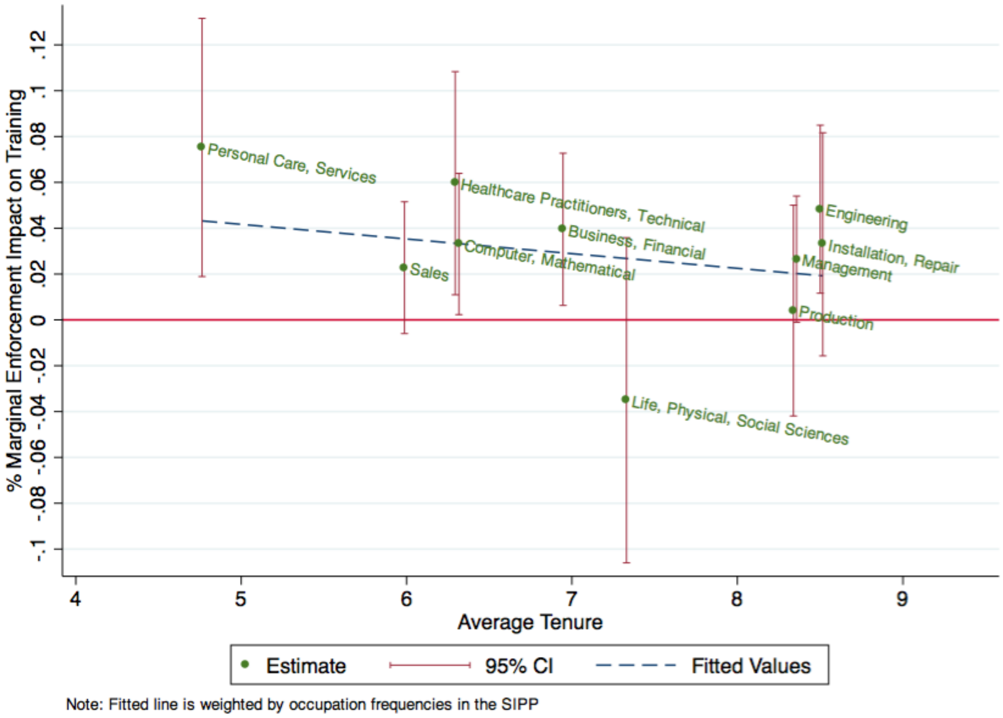


Figure 1.8.5 plots the marginal impact of enforcement by the occupation-specific mean of firm-sponsored training in the data. It appears that enforcement effects are stronger in occupations which report receiving more firm-sponsored training. Figure 1.8.6, on the other hand, plots the marginal impact of enforcement by the proportion of 6-digit occupations within the aggregated 2 digit occupation which are characterized by the BLS Occupational Employment Statistics data as having at least *some on-the-job-training*. The line of best fit slopes slightly downward, indicating that non-compete enforcement tends to reduce firm-sponsored training in occupations with a higher proportion of sub-occupations that receive at least some training.[51] A resolution to this discrepancy arises if those 2 digit occupations

[51] Note that for Figure 1.8.6 the on-the-job-training measure comes from the BLS Occupational Employment Statistics data in which they assigned every Standard Occupational Classification (SOC) occupation at the 6 digit level a level of on-the-job-training (OTJT). The categories they utilized were none, short, moderate, long, residency, apprenticeship. In order to provide an aggregate statistic for the 2 digit SOC code, I took the proportion of occupations within the 2 digit occupation category with at least some training. This statistic is highly dependent on the number of 6 digit occupations within of the 2 digit categories.

Figure 1.8.4: Enforcement Impact on Training by Occupation and Tenure

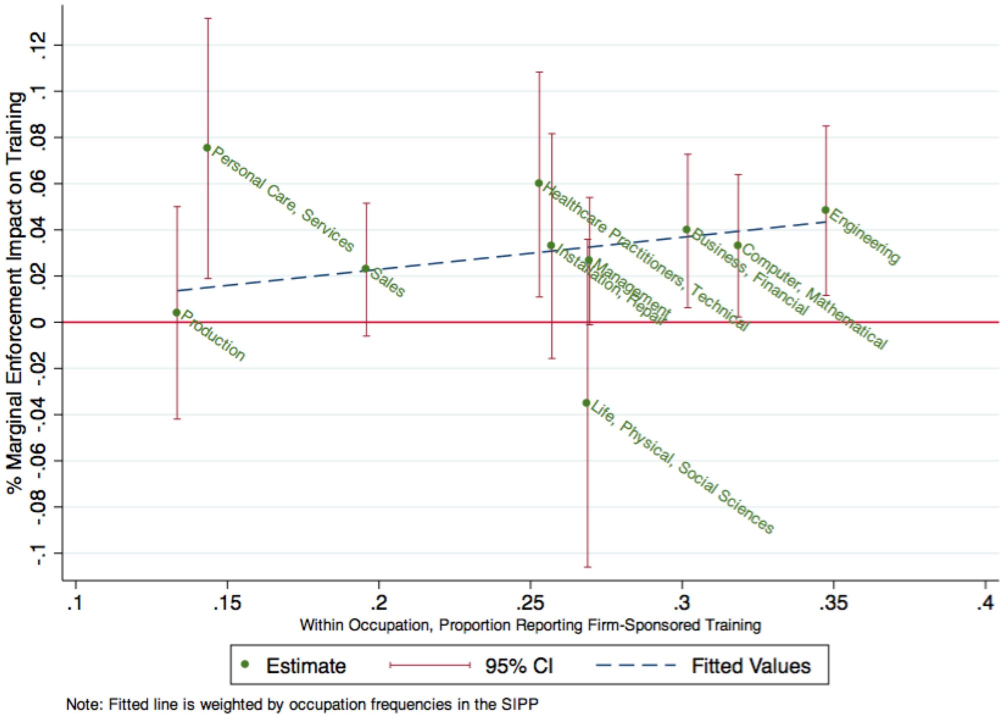


for which each of the 6 digit sub-occupations have some amount of on the job training, such as Installation and Repair, are less likely to to report receiving firm-sponsored training. Alternatively, employees in these occupations may receive a lot of training upfront but not necessarily later on in tenure when the data picks them up. Figure 1.8.7 shows that the enforcement impact is unrelated to the concentration of an occupation within an industry. This is particularly surprising since non-compete enforcement precludes moves to competitors, which are presumably in the same industry.[52] The aggregation up to 2 digit occupation and industry codes may be too blunt, however, and as a result masks significant effects at a more disaggregated level.

Overall these plots show that non-compete enforcement has a stronger impact on occupations

[52]For Figure 1.8.7, the x-axis is the highest proportion of the occupation in any industry at the 2 digit NAICS Industry level.

Figure 1.8.5: Enforcement Impact on Training by Occupation and Firm-Sponsored Training



with more years of schooling and higher earnings. For state governments looking to improve training outcomes for low-skill workers in the high litigation group, non-compete enforcement does not appear to be a particularly appealing lever, except for personal care and services occupations.

Effects Across Tenure and Age Tables

This table presents the results represented in Figure 1.6.1. It shows the intent-to-treat estimates for subsets of tenure categories.

Figure 1.8.6: Enforcement Impact on Training by Occupation and Some OTJT

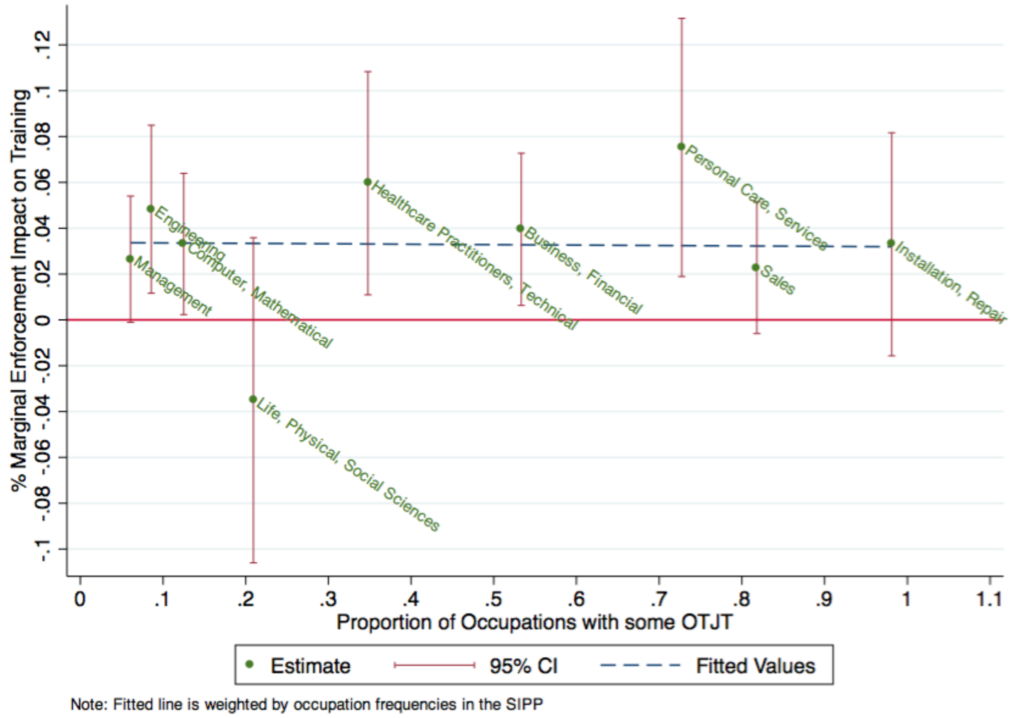


Table 1.11: Training and Non-Compete Enforcement over Tenure

	Tenure in Years				
Intent-to-Treat Effect	0-5	5-10	10-15	15-20	20+
High Litigation	0.007**	0.007*	0.015**	0.019*	-0.007
	(0.003)	(0.004)	(0.006)	(0.011)	(0.006)
Observations	39,176	14,226	7,191	4,422	5,359

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is an indicator for receiving firm-sponsored training in the last year. Robust standard errors are in parentheses, clustered at the state level. The set of controls consist of an enforcement main effect, potential experience, potential experience squared, tenure main effects, hours worked, and indicators for working in a metro area, bachelors degree, graduate school degree, male, white, establishment and firm size 25-99, establishment and firm size 100+, whether the worker is unionized, NAICS 2 digit industries, year. State corporate tax rates, indicators for exceptions to at-will employment, and whether the state is a right-to-work state are all interacted with the high litigation or occupation specific main effects.

Figure 1.8.7: Enforcement Impact on Training by Occupation and Occupation-Industry Concentration

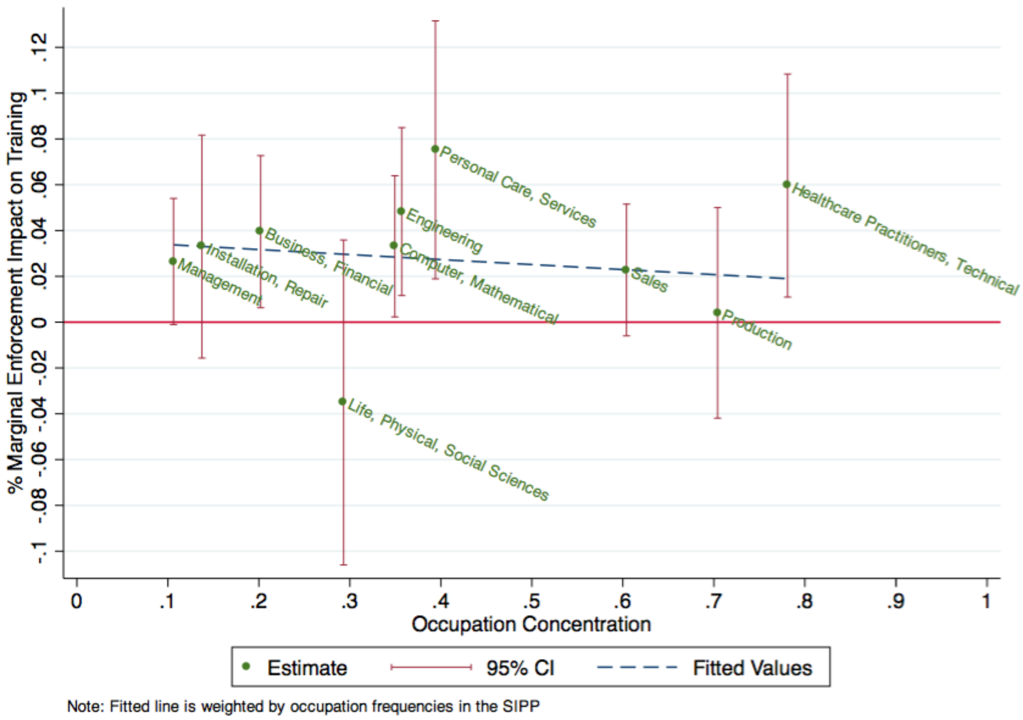


Table 1.12: Training and Non-Compete Enforcement over Age

	Age in Years				
Intent-to-Treat Effect	22-28	29-35	36-42	42-48	49-55
High Litigation	0.014*	0.004	0.016***	-0.004	0.001
	(0.008)	(0.004)	(0.005)	(0.005)	(0.006)
Observations	11,988	15,563	16,200	13,188	13,435

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is an indicator for receiving firm-sponsored training in the last year. Robust standard errors are in parentheses, clustered at the state level. The set of controls consist of an enforcement main effect, potential experience, potential experience squared, tenure main effects, hours worked, and indicators for working in a metro area, bachelors degree, graduate school degree, male, white, establishment and firm size 25-99, establishment and firm size 100+, whether the worker is unionized, NAICS 2 digit industries, year. State corporate tax rates, indicators for exceptions to at-will employment, and whether the state is a right-to-work state are all interacted with the high litigation or occupation specific main effects.

Dividing Industry by Tradable Non-Tradable

Jensen and Kletzer (2005) show the share of total employment by tradable versus non-tradable NAICS sectors. From their Table 4, I classify an industry as tradable if more employment is observed in the tradable portion of that industry. Table 1.13 categorizes NAICS 2 digit industries by whether they are tradable or non-tradable.

Table 1.13: Mapping NAICS 2 Digit Codes to Tradable and Non-Tradable Industries

Non Tradable	Tradable
Utilities	Agriculture, Forestry
Construction	Mining, Quarrying, Oil Extraction
Retail Trade	Manufacturing
Administrative and Support Services	Transportation and Warehousing
Educational Services	Information
Healthcare and Social Services	Wholesale Trade
Arts, Entertainment, and Recreation	Finance and Insurance
Accommodation and Food Services	Real Estate and Rental
Other Services (Except Public Sector)	Professional, Scientific, Technical
	Management of Companies

CHAPTER II

Enforcing Covenants Not to Compete: The Lifecycle Impact on New Firms[53]

2.1 Introduction

'Noncompete agreements...can be a significant impediment to people who aspire to start their own firms...'

-Rami Essaid, entrepreneur, in the Wall Street Journal, Aug 14, 2013

The creation of new firms and their subsequent performance are important concerns to researchers, entrepreneurs and policy makers. An important avenue of new firm formation is through employees of an existing firm leaving their employment to establish a new firm, sometimes referred to as a 'spinout'. Spinouts are of special interest because they have been known to perform better than other types of new firms (Agarwal et al. 2004; Klepper 2007;

[53]Disclaimer: Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed. This research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program, which was partially supported by the following National Science Foundation Grants SES-9978093, SES-0339191 and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation. The authors also thank the Kauffman Foundation, the Harold Price Center for Entrepreneurial Studies at UCLA Anderson School of Management and the Academic Senate of the University of California, Los Angeles for supporting this work.

Chatterji, 2008; Klepper 2009), likely due to human capital that the founders developed at their previous employers. However, founders of spinouts are often contractually bound to their prior employers via agreements that place constraints on what the employees can do after their separation. For instance, by signing covenants not to compete (CNC), employees may be forbidden from joining or establishing a competing firm for a specified period of time or within a stipulated geographic area.

While CNC are commonplace in all US states, there are significant inter-state variations in the degree of enforcement. For instance, such agreements are void in California (Cal. Bus. & Prof. Code §16600) but states such as Florida only prohibit overly restrictive covenants (F.S. §542.335). Naturally, these constraints and the degree to which they are enforced are likely to influence the decision of individuals to engage in entrepreneurial activity, especially when the new firms are formed as spinouts and become direct competitors with their parent firms. Fearing possible litigation from employers, potential spinout founders might be discouraged to leave their parent firms (Samila and Sorensen, 2011; Marx et al, 2011). In line with this argument, Samila and Sorensen (2011) found that CNC enforcement inhibits new firm formation. Along similar lines, Marx et al. (2009) and Fallick et al. (2006) respectively found a link between CNC enforcement and the mobility of inventors and mobility of workers in California's IT cluster.

While these studies show that CNC enforcement influences the entry of new firms, we have no empirical evidence on the impact of non-compete enforcement on new firms beyond their entry. In particular, it is likely that CNC enforcement affects not only the entry rate of new firms, but also their size and growth. Murphy et al. (1991) argue that when people are free to choose, they select occupations that offer the highest return on their abilities. If CNC enforcement acts as an entry barrier, then only founders with high-potential ideas may risk the cost of potential CNC related lawsuits and found direct competitors to their parent firms. In such a case, those spinouts that do eventually form in states with strict CNC enforcement will have higher-quality ideas, and hence have a larger initial size (Cooper et al, 1989). CNC enforcement also affects new firm growth both directly and indirectly. By making it harder to hire talented employees who are also bound by enforceable CNC, stricter CNC enforcement inhibits the growth of new firms. On the other hand, by limiting employee mobility, CNC enforcement may indirectly promote growth by protecting the dissemination of valuable knowledge, and incentivizing firms to invest in growth-enhancing activities such as research and development.

Aside from the limited focus on entry, there are two other gaps in our understanding of these important economic processes. First, to our knowledge, there are no firm-level studies on this question. While Marx et al (2011) use inventor level analyses, Samila and Sorenson (2011) focus on aggregate new firm formation at the MSA level, identifying the relative impact of non-compete enforcement by the amount of new venture capital in the region. More importantly, the absence of a firm-level treatment implies that prior studies treat all new firms uniformly even though CNC enforcement will likely have a disparate impact depending on the type of new firm. In particular, incumbent firms are most likely to litigate CNC against new firms formed by their employees only if those new firms compete with them.

In this study, we use matched employer-employee data from 1991 to 2010 on about 5.5 million new firms from 30 US states across all industries. We identify 'within-industry spinouts', new firms established by employees leaving an incumbent firm and present in the same NAICS 4-digit industry as the incumbent firm. Our baseline index of CNC enforcement is the index developed in Starr (2013), which utilizes factor analysis to re-weight six observed dimensions of CNC enforcement initially quantified for the year 1991 by Bishara (2011). A major empirical challenge to identify the impact of CNC enforcement on new firms is to show that the estimated differences of firm behavior are not driven by other factors specific to the states and correlated with CNC enforcement. Our identification strategy is a difference-in-differences (DID) approach that exploits an unusual aspect of law firms that is uniform across states: courts do not enforce any non-compete covenants between them and departing lawyers. Using law firms as a control group allows us to use both the fullness of our data and state fixed effects in the estimation, which significantly reduces concerns about omitted variable bias. We further argue that any bias resulting from the comparison between law and non-law firms is likely to bias our estimates downward. The per se prohibition on CNC for lawyers was first suggested by the American Bar Association in 1961 and later codified in 1983 as Rule 5.6 of the Model Rules of Professional Conduct (Buffkin 1999). To account for the effect of inter-industry differences in the formation and performance of new firms, we contrast within-industry spinouts to other new firms in the same NAICS 4-digit industry. Thus, we compare how the difference between within-industry non-law spinouts and other new non-law firms and the corresponding difference between within-industry law firm spinouts and other new law firms vary with the degree of non-compete enforcement.

Based on these analyses, and consistent with Samila and Sorensen (2011), we find that the rate of entry of within-industry spinouts is negatively correlated with CNC enforcement. However, the within-industry spinouts that are established tend to be larger and faster growing relative to all other new firms. In particular, our specifications indicate that a unit-change in the enforcement index is associated with about a 0.1% decrease in entry rate of within-industry spinouts, a 1.1% increase in initial employment among within-industry spinouts, and a 0.4% increase in employment growth over the first 3 years of their life. In contrast, based on a difference-in-difference between law firms and non-law firms, we find that non-compete enforcement has no effect on the entry rate of all new non-law firms, reduces the initial size, but has no effect on their employment growth over the first 3 years. These results are consistent with CNC enforcement having a selection effect on within-industry spinouts. Our results are robust to several alternative specifications and to alternative measures of non-compete enforcement including the 1992 and 2002 indices in Garmaise (2011).

Together, our results make two substantive contributions to the literature on new ventures. First, we show that non-compete covenants not only have an influence on the entry of new firms but also over the entire lifecycle of such firms. Second, our study is the first to demonstrate that such covenants have a heterogeneous effect depending on the type of firm. By doing so, we hope to improve our understanding of the mechanisms that link CNC enforcement to new firm formation and performance. In addition to these, by highlighting the unique role of spinouts, our study also contributes to the broader literature on spinouts (e.g., Agarwal, 2004; Klepper, 2001, 2007; Klepper and Sleeper 2005).

The rest of the paper is organized as follows. Section II provides details on both covenants not to compete and related legal processes, and presents the potential effects of CNC enforcement on the life-cycle of new firms. The data and empirical methodology are described in Section III , and the results are presented in Section IV. Section V discusses and concludes.

2.2 Covenants Not to Compete

2.2.1 CNC and CNC Enforcement

Covenants not to compete are post employment restraints which prohibit employees from either joining competitors or starting a competing firm for a specified amount of time and in a specified geographic region. Since the departure of employees can disseminate proprietary knowledge or technology, both valuable sources of competitive advantage (Barney, 1991; Grant, 1996), firms use CNC to protect such trade secrets, client lists, and other intangible assets. CNC appear in contracts for all types of workers, ranging from CEOs to minimum wage workers. Indeed, legal scholars believe them to be commonplace in employment contracts (Stone, 2002).

The degree of CNC enforcement varies considerably among states, and often depends upon the process by which the CNC was signed and potentially violated. Once a firm decides to take a former employee to court over a potential violation of the employee's CNC, judges are bound by either state statute or state precedents in case law.[54] Most states enforce CNC according to some version of the "Rule of Reason," which was initially developed in 1711. The "Rule of Reason" balances the protection needed by the firm with the harm done to the employee and society (Blake 1960). A necessary condition for the enforcement of CNC in any state is that the employee possesses trade secrets, access to clients or client lists, or other types of confidential information which gain value from not being publicly known. Some states will also enforce CNC on the basis of extraordinary training expenses. Given the possession of some kind of protectable interest, states exhibit significant variation in the circumstances under which they consider a CNC enforceable.

2.2.2 Effects of CNC Enforcement on New Firms

By binding employees and their knowledge to one firm, the ubiquitous use of CNC in employment contracts has direct and indirect consequences for both the creation of new firms and their subsequent growth and performance. The first direct consequence of CNC enforcement is an additional cost to entrepreneurs who wish to start a spinout that competes with their

[54] Alternatively, employees who move to a state less likely to enforce a non-compete may file suit in that state in an attempt to have their CNC annulled. Simultaneously, their former employer may file suit in the state where the employee formerly worked. In this complicated situation, there is sometimes a race to the decision, where the slower state can choose to abide by the other state's judgement. Each state, however, does not necessarily have to abide by the judgement of another state. See *Advanced Bionics Corp. v. Medtronic, Inc.* 59 P.3d 231, 238 (California 2002) for a complicated case and Glynn 2008 for more on choices of forum, law, and interjurisdictional issues.

parent firm. In particular, stricter CNC enforcement increases the likelihood that the parent firm may file a lawsuit against the founders for violating CNC. This, in turn, not only entails additional litigation costs for the new firm but also may cause it to fail if the lawsuit is decided in favor of the parent firm. Given this additional cost, only employees who identify very good business opportunities are willing to pay the potential costs of future lawsuits. Assuming the distribution of the quality of new business ideas is the same across states for a given industry, stricter enforcement of CNC reduces the number of new within-industry spinouts by making it harder for low-quality firms to enter, which increases the observed quality of those that are created. New ventures that do not compete with their parent firm do not face this additional cost and their creation is not affected by CNC enforcement. As a result of this selection effect, within-industry spinouts in stricter enforcing states are, on average, of higher quality and begin at a larger size (Cooper et al., 1989) than similar spinouts in other states.

The second direct effect of stricter CNC enforcement is reduced mobility of employees who now face a greater likelihood of CNC-related litigation. This reduced mobility of employees impacts the growth of new firms, irrespective of whether they are within-industry spinouts or not, because growth depends upon the ability of the new firm to attract talented employees, which can be limited if those employees are also bound by enforceable CNC. Like the founders, employees are not likely to join a firm at which their expected costs, including potential CNC litigation from their parent firm, exceed their expected utility from joining the new firm. Consequently, new firms in stricter enforcing states will grow slower, conditional on the underlying idea quality.

CNC enforcement also has indirect effects on firm behavior, particularly through its effect on employee mobility. By allowing firms to better appropriate the gains from their investments, a reduction in employee mobility increases the incentives for firms to invest in human capital through training, research and development and intra-firm information sharing. In particular, if employees can move freely to a firm's competitors, the firm may not be willing to invest in building transferable skills (and knowledge) that are valuable to competitors (e.g., industry-specific expertise). Similarly, they may be less willing to share sensitive information that may be valuable to competitors or may hurt the firm if that information were leaked to competitors. In line with these arguments, Starr (2013) shows empirically that firms in stricter enforcing states are more likely to provide skill-upgrading training to their employees.[55] Also, Conti (2013) finds that incumbent firms in stricter enforcing states are willing to pursue riskier R&D strategies because these firms are more likely to minimize the leakages of the payoffs from high-risk R&D projects. The impact of such increases in human capital and R&D investment and intra-firm information sharing may open up high quality business opportunities that would not have otherwise existed. This indirect effect, in turn, increases the initial size of new firms by improving the quality of new business ideas, and also enables the new firm to grow faster by making these types of investments.[56]

Together, these arguments suggest that while stricter CNC enforcement reduces the entry rate of within-industry spinouts relative to other new firms, it increases the initial size of within-industry spinouts. The effect on growth of new firms appears to be ambiguous.

[55] Garmaise (2011) argues that stricter CNC enforcement reduces incentives for managers to invest in their own human capital. However, it is not clear how investment by managers in their own, transferable human capital will translate into firm growth.

[56] Reduced mobility may also impede R&D investments and consequently growth, by not only limiting the development of firm absorptive capacity but also of an industry-wide spillover pool from which firms can draw ideas. This may be particularly relevant when combining knowledge across firms is important. However, employees switching jobs is only one of many possible spillover mechanisms, and it is not clear why formal knowledge-sharing mechanisms between firms may not achieve the same objectives. Reduced mobility may also potentially decrease employee human capital by reducing the experience they gain by working at multiple firms.

2.3 Data and Empirics

The data for the study come from two micro-level datasets at the U.S. Census Bureau: the 'Longitudinal Business Database' (LBD) and the 'Longitudinal Employer Household Dynamics' (LEHD). The LBD is the universe of all establishments in the US that have at least one employee.[57] For each year from 1976 to 2010, this dataset contains information on employment at the establishment in addition to data on the industry, geography and corporate ownership of the establishment. It does not contain information on revenues or on any other expenditure.

The LEHD is a composite matched employer-employee dataset comprising multiple state-level databases. The two relevant databases within the LEHD are (a) the 'Employment History File' (EHF), which provides the employment history of all individuals that work in establishments in states that participate in this program and (b) the 'Employer Characteristics File' (ECF) which contains information on all employers (i.e. establishments) in states that participate in this program. These databases, maintained separately for each participating state, contain quarterly information on employment and payroll for the establishment, and wage information for the individual. In particular, for each individual, for each quarter, the database provides the payroll they obtained from each firm they worked for (identified by the 'State Employer Identification Number' or the SEIN). Like the LBD, the LEHD does not contain information on revenues or on any other expenditure. The first year the LEHD data are available for at least three states is 1991. The geographical coverage of the data increases over time as more states begin to participate in this program. Our study was based on 30 states: AR, CA, CO, FL, GA, HI, IA, ID, IL, IN, LA, MD, ME, MT, NC, NJ, NM,

[57] An establishment is defined as 'An establishment is a single physical location at which business is conducted and/or services are provided' (http://www.census.gov/econ/susb/definitions.html, retrieved Sep 24, 2013). If very distinct activities are performed at a location, then they are classified as separate establishments.

NV, OK, OR, RI, SC, TN, TX, UT, VA, VT, WA, WI, and WV.

To obtain the list of new firms from the LBD, we selected all establishments that were born in the states and during the years for which we had data from the LEHD. We then excluded establishments belonging to multi-unit firms or to firm that were started more than 1 year prior to the start of the establishment. This sample, which represents almost all new employers in the LEHD-relevant states and years, consisted of about 5.5 million firms. We then identified a subset of these new firms as within-industry spinouts, the details of which are provided below.

2.3.1 Identifying within-industry spinouts

We identified within-industry spinouts based on employee-movement data from the LEHD. We began by identifying 'clusters' of one or more employees moving from one establishment ('the predecessor establishment') to another ('the successor establishment') within the same state during a 1-year period. We chose 1-year as many non-compete agreements last for 1 year. Of these clusters, we restricted our attention to clusters that had less than 20 employees as potential spinouts. The reasoning for this criterion was that clusters with large numbers of employees are more likely to be data errors or administrative problems such as name changes rather than true spinouts. From these clusters, we excluded clusters where the predecessor establishment was too small relative to cluster size; specifically, we imposed a condition that the cluster size be at most 50% of employment at the predecessor establishment. We also excluded clusters where the successor establishment was too large relative to cluster size; specifically, we imposed a condition that the cluster size be at least 75% of employment at the successor establishment. These conditions were aimed at reducing the likelihood of simple ownership changes being identified as spinouts. We also excluded clusters where the successor establishment was more than one year old at the time of the employees moving to the establishment. Broadly, these clusters represent groups of employees moving from an existing firm to join a new firm.

This preliminary list was then refined by using data from the LBD to exclude SEINs owned by firms present in multiple states. From this list, we identify 'within-industry spinouts' as spinouts that have the same 4-digit NAICS code as their parent firm. About 8.4% of all new firms were classifed as within-industry spinouts. Additional descriptive statistics are provided in Table 2.14. [58]

2.3.2 Measuring CNC Enforcement

Malsberger's series *Covenants Not to Compete: A State by State Survey* tracks the case law for each state along numerous dimensions of enforcement including state statutes, what constitutes a protectable interest, the burden of proof on the plaintiff, handling of overbroad contracts, employee consideration, the manner of separation, and others. Bishara (2011) and Garmaise (2011) each quantify these various dimensions of enforcement. Bishara's index scores six dimensions from zero to ten, while Garmaise gives a binary score to twelve dimensions. We use as a baseline the 1991 enforcement index developed in Starr (2013) which modifies the Bishara (2011) index by performing factor analysis to re-weight the six dimensions of enforcement.[59] The Starr index has an advantage of removing the redundancy of the six dimensions of enforcement and capturing finer granularity of the way enforcement is construed along a spectrum of weak to strong enforcement. Table 2.15 from Starr (2013) reports the means for the six dimensions of enforcement and the factor analysis weights,[60]

[58]It is likely that many of these within-industry spinouts are not competitors with their parent firms. Averaging the effects of non-compete enforcement on competitor within-industry spinouts and non-competitor within-industry spinouts biases the estimated effects towards zero.

[59]A similar procedure cannot be performed on the Garmaise index because factor analysis does not work with binary variables.

[60]The highly positive correlations between the underlying dimensions suggests that any re-weighting scheme produces highly correlated indices. Indeed, the correlation between the Starr index and the ini-

and Figure 2.5.1 presents the result enforcement scores by state for 1991 and 2009.

The enforcement index, normalized to be mean 0, standard deviation 1, is a continuous measure which runs from -4 to 2. California and North Dakota are unusual because of their ban on non-competes; every other state enforces them to some extent. As Figure 2.5.1 shows, there is not much variation across time, but significant variation across states.[61] Some states have introduced some small changes over time, such as Oregon's law that employers notify workers in advance of their non-compete or Colorado's law of enforcing non-competes only on upper level management workers, but others have not changed their policies at all. California, for example, adopted their prohibition of CNC in 1872 (Gilson 1999) and has not touched it since.

2.3.3 Identification

A lack of longitudinal variation, an unbalanced panel across states and time, and strict disclosure requirements preclude the possibility of using longitudinal variation in CNC enforcement to identify the lifecycle impact of on new firms. Simple cross-sectional correlations between CNC enforcement are likely biased, however, because other state specific factors that affect the creation and performance of new firms, such as state specific tax, labor, and commercial laws, are likely to be correlated with CNC enforcement. Therefore, to identify the impact of CNC enforcement, we exploit the fact that among all industries, law is the only industry in which CNC are prohibited in every state. Under the assumption that law firms are equally affected by these state specific policies,[62] differencing within a state between all new non-law firms and new law firms cancels out the state specific factors.

tial Bishara (2011) index is 0.94.

[61] During our time period, only Louisiana had a significant reversal in its non-compete laws but disclosure regulations prevent us from studying this reversal with restricted data. In all of our specifications we include a dummy for Louisiana during this time period to partial out this effect

[62] We argue below that the difference between law and non-law firms is likely to be biased downward.

The primary argument for the CNC ban on lawyers, which has recently convinced some states to allow similar carve-outs for physicians, is that limiting a lawyer's ability to practice is equivalent to limiting a client's choice of attorney (Buffkin 1999, Stroud 2002).[63] All of our main specifications utilize a difference-in-differences (DID) specification, comparing non-law firms to law firms, and non-law within-industry spinouts to other non-law new ventures.[64]

The American Bar Association first wrote in Formal Opinion 300 in 1961, then in Disciplinary Rule 2-108(A) in 1969, and finally codified in 1983 as Rule 5.6 of the ABA's Model Rules of Professional Conduct what has become a per se prohibition on the enforcement of covenants not to compete (Buffkin 1999). Model Rule 5.6 states:

> A lawyer shall not participate in offering or making:
>
> (a) a partnership, shareholders, operating, employment, or other similar type of agreement that restricts the right of a lawyer to practice after termination of the relationship, except an agreement concerning benefits upon retirement; or
>
> (b) an agreement in which a restriction on the lawyer's right to practice is part of the settlement of a client controversy.

Stroud (2002) writes, "Rule 5.6 effectively separated law firms from the group of professions included under the reasonableness analysis for covenants not to compete." The first challenge to this ban occurred in *Dwyer v. Jung* in 1975, wherein partners had signed an agreement such that upon dissolution of the practice the clients would be equally divided and each partner could not access another partner's clients for five years. The judge leaned on Disciplinary Rule 2-108(A) in his findings and invalidated the contracts, distinguishing between general restrictive covenants and those between lawyers. While courts have generally reaffirmed *Dwyer*, some have enforced other indirect components of contracts between

[63] Other studies such as Garmaise (2011) and Conti (2013) utilize the CNC law changes in Texas in 1994, Florida in 1996, and Louisiana in 2003 to identify the impact of CNC enforcement. Census disclosure requirements and the years for which we have the LEHD prevent us from utilizing this longitudinal variation. For this reason, we exploit only the cross-sectional variation in enforcement.

[64] Law firms are identified by NAICS code 54111. The analysis is conducted at the NAICS 4 digit level, and 54111 is treated as a separate 4 digit industry.

lawyers such as retirement benefits[65] and forfeiture of compensation clauses.[66] Stroud (2002) reviews this development of case law.

To examine the impact of enforcement on the creation of new ventures, we aggregate the establishment level data to the state-year-industry level. The baseline creation specification is

$$Y^c_{ist} = \gamma_1 NL_i * Enfc_s + \lambda_{it} + \theta_s + \phi Z_{ijst} + v_{ijst} \tag{2.3.1}$$

where Y^c_{ist} refers to the two creation outcomes of interest, the entry rate of new ventures and within-industry spinouts in state s and industry i at time t, NL_i refers to an industry i being 'not law,' $Enfc_s$ refers to the state level enforcement of non-competes, λ_{it} are industry-by-year fixed effects, θ_s are state fixed effects, and Z_{ijst} are other controls.[67] The coefficient γ_1 represents the impact of a one standard deviation increase in non-compete enforcement on the entry rate of new ventures and within-industry spinouts relative to the impact on law firms.

To examine the impact of non-compete enforcement on the initial size of new ventures and their employment growth we use the following DID specifications at the establishment level:

$$\begin{aligned} Y_{ijst} =& \alpha_0 + \alpha_1 NL_i * Enfc_s + \alpha_2 WSO_{ijst} * NL_i + \alpha_3 WSO_{ijst} + \lambda_{it} + \theta_s \\ & + \phi Z_{ijst} + v_{ijst} \end{aligned} \tag{2.3.2}$$

$$\begin{aligned} Y_{ijst} =& \beta_0 + \beta_1 NL_i * Enfc_s * WSO_{ijst} + \beta_2 NL_i * Enfc_s + \beta_3 Enfc_s * WSO \\ & + \beta_4 WSO_{ijst} * NL_i + \beta_5 WSO_{ijst} + \lambda_{it} + \theta_s + \phi Z_{ijst} + v_{ijst} \end{aligned} \tag{2.3.3}$$

[65]See Miller v. Foulston 790 P.2d 404 (Kan. 1990)

[66]See Haight. Brown & Bonesteel v. Superior Court. 285 Cal. Rptr. 845, 846.

[67]Entry rate here is defined as the number of new firms in a state-year-NAICS 4 digit industry divided by the total number of establishments in the state-year-NAICS 4 digit industry.

where Y_{ijst} refers to the outcomes of interest, initial employment and employment growth, WSO_{ijst} refers to a dummy for firm j being a within-industry i spinout at time t in state s. Specification (2.3.2) examines the impact of non-compete enforcement on law and non-law new firms; the coefficient α_1 estimates the change in Y_{ijst}, relative to law firms, for non-law firms as a result of a unit change in the enforcement index. Specification (2.3.3) is a difference-in-difference-in-difference specification, where we look for heterogeneous impacts of enforcement based on whether the non-law new venture is a within-industry spinout. The coefficient of interest is β_1, which represents the effect of the enforcement (relative to the corresponding difference for law firms) on non-law within-industry spinouts relative to other new non-law firms.

For the sake of completeness, we also performed a survival analysis. This analysis follows the same structure as (2.3.2) and (2.3.3), but is estimated using maximum likelihood under the assumption that survival follows a Weibull distribution.[68] Due to the computationally intensive nature of this estimation procedure, we use three digit NAICS fixed effects and separate year fixed effects (instead of four digit NAICS by year fixed effects).

Included in Z_{ijst} are firm level and industry level characteristics. At the firm level, we include a dummy for whether the new firm is a spinout or not (irrespective of within- or outside-industry), and the log of initial employment for the growth and duration regressions. At the industry level, we include the log of the total number of firms and the log of total employment at the state-industry-year level and at the state-year level. In addition, the industry share of total number of firms and total employment are included. The reason for including these variables is that the number of firms and total employment within an industry are strongly correlated with the propensity for the creation of a new firm, as well as the future growth of the firm as employment within the industry represents the pool of talent from which the

[68] Robustness checks using lognormal and log-logistic distributions are also run.

new firm can recruit. In some sense, these size of industry variables may be ‘bad’ controls in the sense that they are also affected by CNC enforcement: if industries are sorting into high or low enforcing states then the size of an industry in a given state-industry-year may be correlated with CNC enforcement. By controlling for industry size, we factor out this effect from the enforcement coefficient. There is also a dummy to account for Louisiana between 2002 and 2003, which represents the only major non-compete reversal in our time frame. To help isolate the impact of CNC enforcement, we include the following state level regressors: exceptions to at-will employment (Autor et al., 2006), top state corporate tax rates, and whether the state is a right to work state, all interacted with the non-law firm dummy. To account for time varying industry trends, all specifications except the duration specification include 4 digit NAICS by year fixed effects.

In addition to these DID estimates, we provide pooled estimates without state fixed effects, which provide insight into the extent to which omitted state-level variables and the difference between law and non-law new firms affect our results. The estimating equations for these pooled OLS specifications follow the same structure as above, without the difference between law and non-law:

$$Y^c_{ist} = \gamma'_1 Enfc_s + \lambda_{it} + \phi Z_{ijst} + v_{ijst} \tag{2.3.4}$$

$$Y_{ijst} = \alpha'_1 Enfc_s + \alpha'_2 WSO_{ijst} + \lambda_{it} + \phi Z_{ijst} + v_{ijst} \tag{2.3.5}$$

$$Y_{ijst} = \beta'_1 Enfc_s * WSO_{ijst} + \beta'_2 WSO_{ijst} + \beta'_3 Enfc_s + \lambda_{it} + \phi Z_{ijst} + v_{ijst} \tag{2.3.6}$$

Equation (2.3.4) is the pooled OLS version of the DID creation regression (2.3.1), while equations (2.3.5) and (2.3.6) are the pooled OLS versions of (2.3.2) and (2.3.3).

The advantage of using lawyers as a control group in a DID approach is not only being able to use all the data in our sample, but also being able to use state fixed effects, which account for other non-time varying state-specific effects. Thus, any other potentially confounding variables must not only differentially affect law and non-law firms, but must also be correlated with CNC enforcement. One such possible reason is that lawyers actually enforce non-competes. Given that *all* employment disputes in 2005 made up only 2.1% (Langton and Cohen, 2008) of all civil state court cases, this potential effect is likely to be small. A related possibility is that law firms make indirect attempts at restricting the mobility of their employees (e.g., by writing and litigating more complicated employment contracts). To the extent that higher enforcing states also enforce these indirect attempts at restricting the ability of lawyers to practice, this will also bias our estimates downward because it makes law firms like other firms along the enforcement dimension. A third potential concern could be the nature of the difference between law firms and other firms: law firms are primarily focused on client relationships, whereas other firms might be focused on engineering or some other production process. If, even though it is unlikely, courts do not treat client relationships and trade secrets equally, and this disparity in treatment is correlated with overall CNC enforcement, then this will bias our estimates. However, even then, the impact of this is likely to be small since our sample covers a wide range of industries including services, which are arguably similar to law firms. Nevertheless, we run a robustness check comparing law firms to only a subset of similar firms in the Professional, Scientific, and Technical (NAICS 54) industry. Finally, law firms may be different from other firms on other dimensions, e.g., law firms tend to be smaller than other firms. However, the inclusion of industry-year fixed effects accounts for all such differences, albeit within the limitations of fixed effects. An alternative concern is that debates on new firm formation are driving changes in the enforcement index. Reverse causality, however, does not appear to be an issue here due to the longstanding nature of these laws and the lack of changes over time.

While the estimates on the enforcement index are likely biased down, the standard errors may also be affected because our estimation procedure does not account for two types of measurement error in our enforcement index. First, each of the dimensions of CNC enforcement are measurement error ridden proxies of latent enforcement intensity. Second, there is error in the factor analysis process which generates the enforcement index that is not accounted for. We corroborate our findings by re-running all our specifications using different versions of the enforcement index, including the Garmaise (2011) indices from 1992 and 2001. If the error is contained in the factor analysis generation process, then the Garmaise indices should not be contaminated.

Finally, note that our estimates reflect intention to treat effects, for enforcement policies are likely to only affect workers who have signed CNC. While identifying the impact of the treatment on the treated would also be informative, this is the parameter that state legislatures care about because they can only choose the enforcement policy; they cannot force firms to use non-competes in their employment contracts.

2.4 Results

Table 2.16 presents the results of our analyses. Throughout these analyses, the aggregated indices are normalized to be mean 0, standard deviation 1, and standard errors are clustered at the state level. Panel A examines entry rate, defined as the ratio of the number of within-industry spinouts to the total number of establishments in a given state-industry-year, and provides the coefficients on the enforcement index (or its interaction with the non-law dummy in the DID specifications) from state-industry-year level regressions. The estimates from the pooled regressions suggest that non-compete enforcement and the entry rate for all new ventures and within-industry spinouts are positive but not statistically significant. The preferred DID estimates show that the entry rate for all new firms is negative but not statistically significant, while the estimate for within-industry spinouts is negative and statistically significant at the 5% level. These estimates indicate that a unit increase in non-compete enforcement reduces the entry rate of within-industry spinouts by 0.1 percentage points.

The first row of Panel B in Table 2.16 focuses on log initial employment, defined as the employment as observed in the first year of the firm's appearance in the LBD when the firm has a strictly positive employment. For all new firms, the coefficient on the enforcement index is strongly negative in both the pooled and difference-in-difference specifications. In contrast, the coefficients pertaining to within-industry spinouts is positive (0.0264 in the pooled specification, and 0.0111 in the DID specification) and and significant at the 1% level. Thus, a one standard deviation increase in CNC enforcement reduces the initial size of new firms by 1.6% percent. Relative to other new ventures, however, a one standard deviation increase in CNC enforcement increases initial employment in within-industry spinouts by 1.1%.

The second through fifth rows study employment growth over the first few years of the new firm's life. Within the first three years of a new firms life, the pooled results and DID results indicate the same pattern: CNC enforcement reduces employment growth for new firms, but, relative to other new ventures within-industry spinouts grow faster. The DID results suggest that a one standard deviation increase in CNC enforcement reduces the growth rate in employment for all new firms in the first 3 years by 0.6%. For within-industry spinouts, the marginal effect relative to other new ventures is 0.4% and significant at the 5% level. Thus, within-industry spinouts not only start larger but also grow faster in the first three years in stricter enforcing states.

The effects of CNC enforcement employment growth between years 3-5 and 5-7 suggest that within-industry spinouts grow slower than other new ventures after an initial period of fast growth. For new ventures overall, the pooled OLS results do not reach conventional levels of statistical significance. The DID results for new ventures in years 3-5 do show positive but not statistically significant effects, while years 5-7 show a positive and statistically significant effect at the 5% level. Our estimates suggest that a one standard deviation increase in non-compete enforcement increases employment growth between years 5 and 7 for all new firms by 0.8%. [69]

Finally, the last row presents the results of duration regressions with a Weibull survival distribution curve. The reported coefficients represent the actual β coefficients. Exponentiating the results allows a hazard ratio interpretation. The positive coefficient for all new firms in the DID specification, 0.0162, suggests that a one standard deviation increase in non-compete enforcement increases the hazard of failure to 1.02, thus reducing the lifespan of new firms. The coefficients for all new firms switch from negative and significant in the pooled specification to positive and significant in the DID specification. The coefficient for within-industry spinouts is negative and significant in the pooled specification but positive and not statistically significant in the DID specification.[70]

Together, these results strongly suggest that CNC enforcement has an effect on the entire lifecycle of new firms, and differential effects for within-industry spinouts.

[69] In analyses not presented, we found no significant differences between within-industry spinouts and other new firms in 5-year and 7-year growth rates.

[70] The cause of the sign switch between the pooled OLS and DID specifications is not the assumed survival distribution or the particular enforcement index chosen. One interpretation of the sign switch is that there is an omitted state-level variable correlated with non-compete enforcement and the survival of firms which is reversing and biasing upward the estimate from the pooled specification. If this is correct, then including state fixed effects in the DID specification provides the correct estimate. Alternatively, if the omitted variable is correlated with non-compete enforcement and causes law firms to live longer than other new firms in stricter enforcing states, then the DID estimates are biased downward.

2.4.1 Robustness Checks

We repeated our analyses using alternative measures of CNC enforcement. Specifically, we used our factor-analysis-weighted-index constructed from the 2009 data in Bishara (2011), and the Garmaise (2011) indices from 1992 and 2001. Tables 2.17 through 2.22 present these results for each entry, initial size, growth, and survival. Generally, the results are qualitatively similar to those from our baseline table, 2.16.

One possible concern is that law firms are not a proper control group for all other firms in the economy. In order to make the industry mix more comparable, we restrict the sample to only firms in the same two digit NAICS code as law firms: NAICS 54 refers to professional, scientific, and technical services.[71] We cannot present the corresponding numerical results because they are currently under disclosure review. Qualitatively, however, the results of this robustness exercise do not undermine the main conclusions of the empirical work that non-compete enforcement reduces entry of within-industry spinouts, but increases their initial size and growth in the first 3 years.

2.5 Discussion and Conclusion

This study uses data on several million new firms across 30 states, and finds that CNC enforcement reduces the initial size of new firms, but increases their growth between years 5 and 7. These results are economically significant. If California (-3.73) adopted Florida's

[71]The Professional, Scientific, and Technical Services sector comprises establishments that specialize in performing professional, scientific, and technical activities for others. These activities require a high degree of expertise and training. The establishments in this sector specialize according to expertise and provide these services to clients in a variety of industries and, in some cases, to households. Activities performed include: legal advice and representation; accounting, bookkeeping, and payroll services; architectural, engineering, and specialized design services; computer services; consulting services; research services; advertising services; photographic services; translation and interpretation services; veterinary services; and other professional, scientific, and technical services.

laws (+1.33), the average initial employment of new firms in California would fall by 8%, but the growth between year's 5 and 7 would increase by 8%.

Importantly, CNC enforcement appears to have a large impact on the lifecycle of within-industry spinouts. Relative to other new ventures, increases in CNC enforcement result in fewer, but larger, faster-growing within-industry spinouts. Based on our results in Table 2.16, if California adopted Florida's CNC laws, it would result in a 0.5% decrease in the entry rate of within-industry spinouts in California, a 5.6% increase in their average initial employment, and a 2.0% higher growth rate in the first 3 years of their life. Adding these, our results imply that the difference in CNC enforcement between Florida and California is associated with an 7.6% difference in the size of 3-year old firms.

While these results show a potentially strong link between CNC enforcement and new firm formation and performance, we now attempt to identify which mechanisms are at work. The generally accepted argument is that such covenants reduce the mobility of individuals including those of entrepreneurs from their employers (Samila and Sorensen, 2011; Marx et al, 2011), which explains the lower entry rate of within-industry spinouts relative to other new ventures.

We then turn to the impact of CNC enforcement on the initial employment and subsequent employment growth for new ventures and the differential effects for within-industry spinouts. For new firms, the fact that stricter CNC enforcement reduces the initial size of new ventures suggests that new firms struggle to hire employees, who are also likely bound by CNC, perhaps because the underlying average quality of the firms is low. For the firms that survive, the boost in employment growth in years 5 to 7 suggests that those firms are benefitting from the protection that CNC enforcement offers, either by investing more in growth enhancing activities or simply by perfecting their product without leaking its secrets. Relative to other new ventures, the fact that within-industry spinouts start out larger and grow faster initially suggests that they do not have the same hiring issues that other new ventures have. A possible way to reconcile these two sets of results is to treat the effect of CNC enforcement as an 'entry barrier' or an additional cost that within-industry spinouts must pay to enter the market. Then, assuming that the quality of ideas varies across entrepreneurs, this additional cost would result in lower entry among entrepreneurs with lower quality ideas. The higher observed employment and growth is then a result of higher quality ideas. Thus, in this argument, the CNC enforcement does not cause the performance of any individual firm to be higher than it would otherwise be; rather, the measured average performance across firms is higher because stricter enforcement increases the cut-off 'quality' needed for entry.

This selection argument need not be the whole story. In Table 2.17, we examine the robustness of the entry results using other variations of the CNC index. For within-industry spinouts, the impact is always negative in the DID specification, but only statistically significant for two of the four indices. This is not surprising because the category of within-industry spinouts contains both spinouts that compete with their parent firm and spinouts that compete in the industry but not with their parent firm. An alternative mechanism which would increase the quality of within-industry spinouts is increased investment by parent firms to develop the human capital of their employees. Stricter CNC enforcement buys the loyalty of employees who sign CNC, and therefore provides firms with a higher incentive to invest in training, R&D, and share sensitive information with their employees. These investments may provide employees with high quality ideas to start new firms within the same industry that need not necessarily be competitors with their parent firms.

This study has implications for the research on new venture formation and entrepreneurship, as well as for managers concerned about new entry. Our results suggest that the effect of CNC enforcement is not limited to its effect on the mobility of workers or the formation of new firms, but also affects the growth of new firms. Our study also implies that in order to evaluate its full impact, we should examine the effect of CNC enforcement on the whole process of new venture formation, and not just on entry. While CNC enforcement can reduce the rate of within-industry spinout formation, the reduction might be the results of low quality firms not entering as spinouts. This view provides new insights for incumbent firms: strong CNC enforcement might not necessarily reduce the level of competitive threats from spinouts.

Turning to the welfare effects of CNC enforcement, our results do not allow us to unambiguously state whether they increase or decrease welfare. Note that our suggested mechanism is not a simple story of higher entry barriers that lead to welfare losses. Here, the entry barriers are higher only for a subset of the firms. Thus, if the mean 'productivity' is higher for this subset of firms than for all other firms, then CNC enforcement would be welfare destroying. However, if this subset of firms has a lower mean productivity than some other subset of firms, then the welfare effect is not clear. For instance, incumbent firms very likely have higher productivities than within-industry spinouts, and it is possible that they fill in the gap left by within-industry spinouts in states with higher CNC enforcement. In such an event, CNC enforcement may actually increase welfare.[72] Beyond this, there may be longer term effects of competition on innovation, which have been ignored in this discussion. The study also does not consider the possibility that CNC enforcement could change the intensity of industry competition. We leave a comprehensive analysis of these issues as a subject for future research.

A natural extension of this study would be to perform additional analyses to support the selection argument. For instance, one could study if the dispersion of productivity of within-industry spinouts is negatively correlated with CNC enforcement, and in particular if the

[72] Of course, in this argument, it is not clear why spinouts would enter knowing that incumbents have higher productivities. One way to address this would be assume that, *ex-ante*, firms do not know their productivities but only know some statistic about the productivity distribution that they may draw their productivity from.

lower end of the productivity distribution (e.g., the 10^{th} percentile) is higher in higher-enforcement states. Another potentially interesting avenue for future research is to examine the inter-industry heterogeneity in the impact of CNC enforcement. As alluded to earlier, noncompete agreements are most effective at protecting assets that can be transferred out through the departure of employees, and when other means to protect those assets such as patents, copyrights, trade secrets, or the need for complementary assets are available then this may reduce the benefits of enforcing CNC. Thus, we should expect to see significant inter-industry variation in the effects of CNC enforcement.

To conclude, noncompete agreements do indeed appear to be a 'significant impediment to people who aspire to start their own firms', especially, if they want to start a firm that competes with their current employer.

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Figure 2.5.1: Factor Analysis Enforcement Index for 2009 and 1991

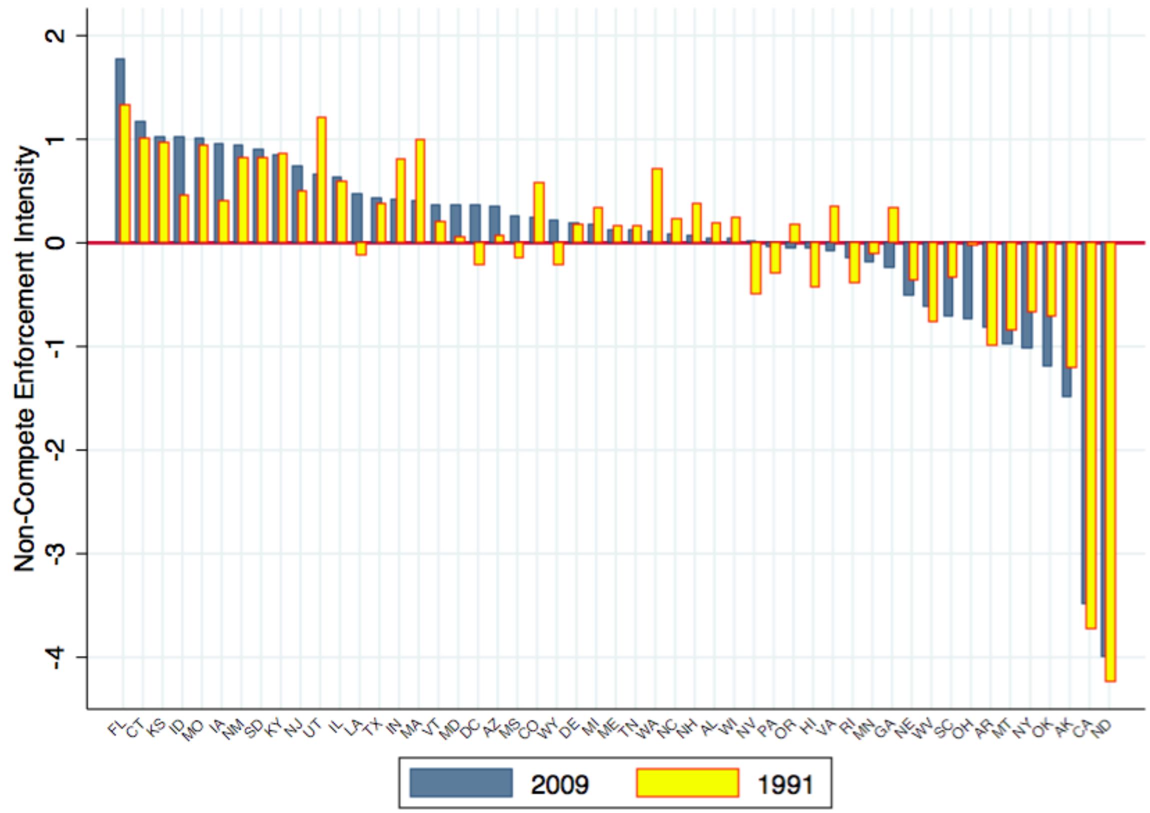


Table 2.14: Descriptive Statistics

Panel A: All New Firms

	Full Sample		Law		Non-Law	
	Mean	SD	Mean	SD	Mean	SD
Within-industry Spinout Dummy	0.08	0.28	0.23	0.42	0.08	0.27
New Firm Entry Rate	0.10	0.10	0.07	0.02	0.10	0.10
Employment Yr 0	5.86	60.5	2.92	7.3	5.93	61.2
Employment Yr 3	7.66	55.4	3.79	8.08	7.76	56.1
Employment Yr 5	8.74	62.6	4.18	7.68	8.87	63.5
Employment Yr 7	9.64	73.7	4.51	8.44	9.8	74.8
Non-law Firm Dummy	0.98	0.15	0.00	0.00	1.00	0.00
Enforcement	-0.40	1.77	-0.38	1.78	-0.40	1.77
Industry Employment	44996	69052	51564	39853	44849	69557
Industry Size	4303	5220	9649	7032	4184	5109
Industry Share-Employment	0.01	0.01	0.01	0.00	0.01	0.01
Industry Share-Firms	0.01	0.01	0.03	0.00	0.01	0.01
Observations	5,538,121		120,961		5,417,160	

Panel B: Within-industry Spinouts

	Full Sample		Law		Non-Law	
	Mean	SD	Mean	SD	Mean	SD
Employment Yr 0	3.89	17.4	2.33	3.44	4.00	17.9
Employment Yr 3	5.76	50.2	3.14	4.04	5.95	52.0
Employment Yr 5	6.67	58.3	3.55	4.04	6.92	60.6
Employment Yr 7	7.36	59.2	3.90	6.74	7.67	8.17
Non-law Firm Dummy	0.94	0.24	0.00	0.00	1.00	0.00
Observations	466,116		28,106		438,010	

Table 2.15: Factor Analysis Index from Starr (2013)

	1991			2009			
Question	Mean	SD	CFA Weight	Mean	SD	CFA Weight	Bishara Weight
Statute of Enforceability	4.90	1.53	10	4.96	1.79	10	10
Employer's Protectable Interest	5.80	2.03	7.1	6.51	1.93	21	10
Plaintiff's Burden of Proof	5.36	2.06	9.3	5.59	1.93	14	10
Worker's Consideration	7.74	2.30	10	7.94	2.27	8	7.5
Overbroad Contracts	5.71	3.07	3.6	5.83	2.91	4	5
Quit v. Fire	6.23	2.32	10.7	6.45	2.37	7	10

Note: The weight for statute of enforceability is normalized to 10 here for comparability, but in the analysis the aggregated indices are normalized to be mean 0, standard deviation 1.

Table 2.16: The Effect of Non-compete Enforcement on New Firms: 1991 Index

Panel A: Entry Rate Analysis

	Pooled OLS		DID	
Dependent Variable	All New Firms (γ_1')	Within-Industry Spinout (γ_1')	All New Firms (γ_1)	Within-industry Spinout (γ_1)
Entry Rate	0.0003	0.0002	-0.0014	-0.0010^{**}
	(0.0016)	(0.0002)	(0.0013)	(0.0004)
State FE			X	X
Year by Industry FE	X	X	X	X

Panel B: Initial Employment, Employment Growth, and Survival Analysis

	Pooled OLS		DID	
Dependent Variable	All New Firms (α_1')	Within-Industry Spinout (β_1')	All New Firms (α_1)	Within-Industry Spinout (β_1)
Initial Employment	-0.0154^{***}	0.0264^{***}	-0.0163^{***}	0.0111^{***}
	(0.0040)	(0.0019)	(0.0049)	(0.0017)
Employment Growth 0-3 yrs	-0.0121^{***}	0.0059^{***}	-0.0049	0.0040^{**}
	(0.0028)	(0.0014)	(0.0037)	(0.0017)
Employment Growth 3-5 yrs	-0.0007	-0.0025^{**}	0.0034	-0.0043^{***}
	(0.0013)	(0.0011)	(0.0034)	(0.0012)
Employment Growth 5-7 yrs	-0.0015	0.0005	0.0081^{**}	-0.0085^{***}
	(0.0013)	(0.0003)	(0.0030)	(0.0012)
Survival	-0.0194^{**}	-0.0068^{**}	0.0162^{***}	0.0056
	(0.0091)	(0.0029)	(0.0050)	(0.0040)
State FE			X	X
Year by Industry FE	X	X	X	X

*** p<0.01, ** p<0.05, * p<0.1. This table presents the results of pooled OLS and DID regressions of Equations (2.3.2) and (2.3.3). The left-most column represents the dependent variable. Robust standard errors are in parentheses, clustered at the state level. Other controls can be found on page 182.

Table 2.17: Entry Rate Robustness Check: Different Enforcement Indices

	Pooled OLS		DID	
Enforcement Index	All New Firms (γ_1')	Within-Industry Spinout (γ_1')	All New Firms (γ_1)	Within-industry Spinout (γ_1)
SB 1991	0.0003	0.0002	−0.0014	−0.0010**
	(0.0016)	(0.0002)	(0.0013)	(0.0004)
SB 2009	0.0001	0.0000	−0.0009	−0.0008
	(0.0013)	(0.0002)	(0.0015)	(0.0005)
Garmaise 1992	−0.0005	0.0001	0.0000	−0.0010
	(0.0013)	(0.0002)	(0.0015)	(0.0006)
Garmaise 2001	0.0005	0.0001	0.0003	−0.0011*
	(0.0014)	(0.0002)	(0.0014)	(0.0006)
State FE			X	X
Year by Industry FE	X	X	X	X

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. SB stands for the Starr (2013) reweighting of the Bishara (2011) scores. This table presents the results of pooled OLS and DID regressions of Equations (2.3.2) and (2.3.3). The left-most column represents the dependent variable. Robust standard errors are in parentheses, clustered at the state level. Other controls can be found on page 182.

Table 2.18: Initial Size Robustness Check: Different Enforcement Indices

	Pooled OLS		DID	
Enforcement Index	All New Firms (α_1')	Within-Industry Spinout (β_1')	All New Firms (α_1)	Within-Industry Spinout (β_1)
SB 1991	-0.0154^{***}	0.0264^{***}	-0.0163^{***}	0.0111^{***}
	(0.0040)	(0.0019)	(0.0049)	(0.0017)
SB 2009	-0.0105^{***}	0.0263^{***}	-0.0177^{***}	0.0119^{***}
	(0.0028)	(0.0017)	(0.0047)	(0.0020)
Garmaise 1992	-0.0115^{**}	0.0290^{***}	-0.0214^{**}	0.0143^{***}
	(0.0049)	(0.0064)	(0.0082)	(0.0037)
Garmaise 2001	-0.0141^{***}	0.0271^{***}	-0.0229^{***}	0.0140^{***}
	(0.0038)	(0.0068)	(0.0072)	(0.0045)
State FE			X	X
Year by Industry FE	X	X	X	X

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. SB stands for the Starr (2013) reweighting of the Bishara (2011) scores. This table presents the results of pooled OLS and DID regressions of Equations (2.3.2) and (2.3.3). The left-most column represents the dependent variable. Robust standard errors are in parentheses, clustered at the state level. Other controls can be found on page 182.

Table 2.19: Employment Growth Years 0-3 Robustness Check: Different Enforcement Indices

	Pooled OLS		DID	
Enforcement Index	All New Firms (α_1')	Within-Industry Spinout (β_1')	All New Firms (α_1)	Within-Industry Spinout (β_1)
SB 1991	-0.0121^{***}	0.0059^{***}	-0.0049	0.0040^{**}
	(0.0028)	(0.0014)	(0.0037)	(0.0017)
SB 2009	-0.0091^{***}	0.0064^{***}	-0.0066^{*}	0.0051^{***}
	(0.0027)	(0.0014)	(0.0038)	(0.0014)
Garmaise 1992	-0.0095^{***}	0.0076^{***}	-0.008	0.0056^{*}
	(0.0032)	(0.0019)	(0.0064)	(0.0028)
Garmaise 2001	-0.0095^{***}	0.0077^{***}	-0.0098^{*}	0.0060^{***}
	(0.0025)	(0.0011)	(0.0052)	(0.0020)
State FE			X	X
Year by Industry FE	X	X	X	X

*** p<0.01, ** p<0.05, * p<0.1. SB stands for the Starr (2013) reweighting of the Bishara (2011) scores. This table presents the results of pooled OLS and DID regressions of Equations (2.3.2) and (2.3.3). The left-most column represents the dependent variable. Robust standard errors are in parentheses, clustered at the state level. Other controls can be found on page 182.

Table 2.20: Employment Growth Years 3-5 Robustness Check: Different Enforcement Indices

	Pooled OLS		DID	
Enforcement Index	All New Firms (α_1')	Within-Industry Spinout (β_1')	All New Firms (α_1)	Within-Industry Spinout (β_1)
SB 1991	−0.0007	−0.0025**	0.0034	−0.0043***
	(0.0013)	(0.0011)	(0.0034)	(0.0012)
SB 2009	−0.0006	−0.0021	0.0018	−0.0050***
	(0.0011)	(0.0015)	(0.0038)	(0.0009)
Garmaise 1992	−0.0008	−0.0018	0.0003	−0.0085***
	(0.0015)	(0.0023)	(0.0053)	(0.0025)
Garmaise 2001	−0.0012	−0.0008	−0.0029	−0.0074***
	(0.0011)	(0.0024)	(0.0048)	(0.0025)
State FE			X	X
Year by Industry FE	X	X	X	X

*** p<0.01, ** p<0.05, * p<0.1. SB stands for the Starr (2013) reweighting of the Bishara (2011) scores. This table presents the results of pooled OLS and DID regressions of Equations (2.3.2) and (2.3.3). The left-most column represents the dependent variable. Robust standard errors are in parentheses, clustered at the state level. Other controls can be found on page 182.

Table 2.21: Employment Growth Years 5-7 Robustness Check: Different Enforcement Indices

	Pooled OLS		DID	
Enforcement Index	All New Firms (α'_1)	Within-Industry Spinout (β'_1)	All New Firms (α_1)	Within-Industry Spinout (β_1)
SB 1991	-0.0015	0.0005	0.0081^{**}	-0.0085^{***}
	(0.0013)	(0.0003)	(0.0030)	(0.0012)
SB 2009	-0.0012	0.0003	0.0074^{**}	-0.0081^{***}
	(0.0011)	(0.0003)	(0.0033)	(0.0011)
Garmaise 1992	-0.0008	0.0001	0.0078	-0.0093^{***}
	(0.0016)	(0.0006)	(0.0050)	(0.0020)
Garmaise 2001	-0.0015	0.0001	0.0035	-0.0079^{***}
	(0.0012)	(0.0005)	(0.0045)	(0.0016)
State FE			X	X
Year by Industry FE	X	X	X	X

*** p<0.01, ** p<0.05, * p<0.1. SB stands for the Starr (2013) reweighting of the Bishara (2011) scores. This table presents the results of pooled OLS and DID regressions of Equations (2.3.2) and (2.3.3). The left-most column represents the dependent variable. Robust standard errors are in parentheses, clustered at the state level. Other controls can be found on page 182.

Table 2.22: Survival Robustness Check: Different Enforcement Indices

	Pooled OLS		DID	
Enforcement Index	All New Firms (α_1')	Within-Industry Spinout (β_1')	All New Firms (α_1)	Within-Industry Spinout (β_1)
SB 1991	-0.0194^{**}	-0.0068^{**}	0.0162^{***}	0.0056
	(0.0091)	(0.0029)	(0.0050)	(0.0040)
SB 2009	-0.0164^{*}	-0.0080^{***}	0.0145^{**}	0.0078^{**}
	(0.0089)	(0.0030)	(0.0058)	(0.0038)
Garmaise 1992	-0.0038	-0.0105^{***}	0.0246^{**}	0.0125^{**}
	(0.0130)	(0.0034)	(0.0112)	(0.0059)
Garmaise 2001	0.004	-0.0107^{***}	0.0268^{***}	0.0140^{**}
	(0.0112)	(0.0022)	(0.0100)	(0.0056)
State FE			X	X
Year by Industry FE	X	X	X	X

*** p<0.01, ** p<0.05, * p<0.1. SB stands for the Starr (2013) reweighting of the Bishara (2011) scores. The estimates are from the weibull model. This table presents the results of pooled OLS and DID regressions of Equations (2.3.2) and (2.3.3). The left-most column represents the dependent variable. Robust standard errors are in parentheses, clustered at the state level. Other controls can be found on page 182.

CHAPTER III

Who Signs Noncompete? Evidence From a New Survey

3.1 Introduction

Covenants not to compete (noncompetes) are contracts between employers and employees that prohibit the employee's movement to competitor firms for a fixed amount of time post separation. Given the long history of noncompetes dating back to 1407 (Blake 1960), the often presumed ubiquity of their use in employment contracts (Stone 2002), the fervent debate over their enforcement (e.g., Hyde 2003, Lobel 2013), and the enormous effects scholars claim they have on workers, firms, and the economy (Marx et al. 2009, Gilson 1999, Balasubramian et al. 2014), it is surprising that we know very little about how frequently and under what circumstances noncompetes are used. Using the results from a recent large scale survey of labor force participants, this study fills this major gap in our knowledge by characterizing the types of workers who sign noncompetes and the industries which use them most frequently.

Since noncompetes have the potential to misallocate labor across firms and states (Marx et al. 2011a), affect investment in human capital (Starr 2014, Garmaise 2011), the functioning of internal labor markets, firm productivity, and worker productivity, it is important to understand exactly what types of workers sign noncompetes. Previous work has studied only a select group of occupations, finding that 70-80% of CEOs (Bishara et al. 2014, Garmaise 2011), 40% of physicians (Lavetti et al. 2013), and 45% of engineers (Marx 2011) sign noncompetes. This study greatly expands our knowledge because it not only looks at a broad array of occupations, but also examines how noncompete incidence varies according to many other employee level and firm level variables such as education, earnings, whether the worker possesses a legitimate business interest, expected employment duration, firm and establishment size, industry, poaching frequency, and state level noncompete enforcement.

The data for this study come from a new survey focusing on the use and impacts on noncompetes that we wrote and implemented using an online platform. The results suggest that noncompetes are a common part of the employment relationship. Sixty three percent of the sample reports knowing about noncompetes, while 40% of those who have ever heard of noncompetes have signed one. In the last 5 years, respondents have signed a noncompete with between 21.8% and 25.7% of their employers. In their current position, between 11.3% and 19.6% of the respondents have signed noncompetes.

The two digit occupations with the highest incidence of noncompetes are architecture and engineering, computer and mathematical, installation and repair, business and finance, arts and entertainment, and management, all with at least 20% of worker signing noncompetes. As suggested by these results, noncompetes are strongly and positively correlated with both education and earnings. Yet even for those who earn $40k a year the incidence of noncompetes is still at least 15.3%. With regards to occupation specific duties, workers in those occupations which allow the worker to learn company trade secrets are at least 12 percentage points more likely to sign a noncompete than those who work directly with clients and have access to client-specific information.

The industries which have the highest incidence of noncompetes are information, management of companies, professional, scientific, and technical services, manufacturing, finance and insurance, and arts and entertainment. While the literature has primarily focused on tech industries, noncompetes are prevalent in most industries, with projections suggesting that at least 12% sign noncompetes in every industry.

While states differ widely in their noncompete enforcement policy (Bishara 2011, Garmaise 2011), the incidence of noncompetes is only weakly correlated with the noncompete enforcement policy of the state. Indeed, even for California, which refuses to enforce noncompetes, at least 11% of Californians still sign them.

There are two central takeaways from the paper: (1) the incidence of noncompetes is higher in places where the literature suggests it would be higher; (2) but the incidence of noncompetes in other places where we would expect it to be nonexistent, such as for the less educated and those with lower incomes, is still surprisingly high.

The structure of the paper is as follows: Section 3.2 discusses the organization of the survey, the data collection process, and the sampling frame. Section 3.3 examines the relationship between the incidence of noncompetes and various worker and firm level variables. Section 3.4 provides a multivariate analysis explaining the utilization of noncompetes, and Section 3.5 concludes.

3.2 Data and Survey Methodology

The data comes from a large scale online survey the authors developed using Qualtrics software. The survey has three parts: (1) lifetime experiences with noncompetes, (2) knowledge of noncompete laws and perceptions of enforcement, and (3) experiences with noncompetes in a current job. The project was run through Qualtrics, who outsourced the collection of the data to their panel partners. Potential respondents to the survey had previously agreed to respond to online surveys and were sent the survey via an e-mail link or as part of an online game.

The sample population are labor force participants aged 18 to 75, who are either unemployed or employed in either the private sector or in a public healthcare system. For compensation, the respondents were either paid \$1.50, given credits in a particular online game they were playing, or were entered into sweepstakes drawings to earn prizes or other rewards. The median finish time for the survey was 25 minutes. Due to the length and intensive nature of the questions, we employed the use of 'attention filters,' which require the respondents to answer in a certain way or else they are discontinued from the survey. At the time of this writing, the survey is still in the field and thus we are unable to determine exactly the number of survey takers who were dropped in this way.

Via the use of quotas, the online survey platform gave us greater control over response rates and sample selection bias. The target for this survey was 10,000 completed surveys with 50% male, 60% with at least a bachelor's degree, 50% earning at least \$50,000 from their current, highest paying job, and 30% over the age of 55. These numbers were chosen either to align with the corresponding moments in the data for labor force participants in the 2012 American Community Survey, or to oversample certain groups of the population for further subgroup comparisons. In addition, to examine smaller states with particularly unique noncompete laws, we oversampled respondents from Colorado, Oregon, Massachusetts, and Florida.

3.2.1 Representativeness and Weighting

The nonrandom sample selection process, both from the quotas and the online survey platform, suggests that final Qualtrics sample is not representative of labor force participants aged 18 to 75. To understand the extent of the bias, we compare observable outcomes from the Qualtrics sample to a nationally representative sample of labor force participants from the 2011 American Community Survey (ACS). Columns (1) and (2) of Table 3.23 shows that the two samples are clearly different. In particular the unweighted Qualtrics sample shows particularly different education levels and proportion female relative to the ACS. For example, only 19.7% of individuals receive a bachelors degree in the ACS, while 31.1% in the Qualtrics sample received a bachelors. This particular difference arises directly because of the quota that 50% of the sample have a bachelors degree.[73]. The Qualtrics sample is also over 60% female, while the proportion in the ACS is 47.4%.

To weight the Qualtrics sample to make it comparable to the nationally representative sample, we first estimate a logit model in which the outcome is a dummy variable equal to one for being in the ACS sample. The covariates include age, hours worked per week, sex, education, race, class of the worker, and state. We construct weights equal to $\hat{p}/(1-\hat{p})$, where $\hat{p}$ is the propensity score corresponding to the predicted values of the logit regression, and apply them to the Qualtrics sample. The intuition behind the weighting scheme is that observations likely to be observed in a nationally representative sample are given a high weight, while observations unlikely to be seen are given lower weight. The weights range from 0.013 to 342391.2, with a mean of 21608.9 and a standard deviation of 20795.2. The huge range of weights likely arises from some of the spurious inputs of respondents on the survey.

Column (3) of Table 3.23 shows that the inclusion of these weights dramatically changes the mean outcomes in the Qualtrics sample, much more closely resembling the nationally representative sample. For example, the proportion with a bachelors degree in the weighted Qualtrics sample becomes 20.8%, much closer to the 19.7% in the ACS than the 31.1% in the unweighted sample. With regards to sex, the weighted sample almost identically matches the population gender distribution. The weighting similarly improves the balance for each

[73]Recall that at this point the sample is still being collected.

of the observable characteristics.

3.2.2 Employment Status and Worker Class

Table 3.24 tabulates the employment status and worker class for the weighted Qualtrics sample. Overall, 75% of the sample holds one job, while about 17% are currently unemployed. Those who are unemployed answer the questions regarding their previous employment relationship. The workers are primarily in the private sector, though 4% are employed by a public healthcare system. Within the private sector, 88.4% of the respondents work for a for profit firm.

3.3 Incidence

3.3.1 Ever Heard of or Signed a Noncompete?

A primary challenging in quantifying the incidence of noncompetes is that respondents may not know what noncompetes are, or may not know that they have signed them. Therefore, to begin our analysis of the incidence of noncompetes, we first ask respondents if they have ever heard of noncompetes, giving them a written explanation of what they are. Table 3.25 shows that 63.02% of the weighted sample claims to have heard of noncompetes, while 36.98% have not heard of them.

Table 3.26 shows the cross tabulation of whether respondents report ever signing a noncompete against whether or not they have heard of it. Overall, 24.7% of the total weighted sample has signed a noncompete. Of those who have heard of noncompetes, 39.2% report having signed a noncompete at some point in their life. The respondents also provide information on the number of employers in the past 5 years with which they have signed noncompetes

Table 3.23: Qualtrics, ACS Comparison

Education	ACS (1)	Unweighted (2)	Weighted (3)
Did not complete high school	10.1	1.3	9.3
High school grad	27.2	14.8	26.2
<1 year of college	6.7	9.5	6.9
>1 years of college, no degree	18.3	17.5	18.4
Associates (2 yr) degree	8.9	13.3	9.2
Bachelors degree	19.7	31.1	20.8
Masters degree	6.3	9.9	6.4
Professional degree	1.7	1.5	1.7
Doctoral degree	1.0	1.1	1.1
Race	**ACS**	**Unweighted**	**Weighted**
White alone	76.2	80.4	79.3
Black alone	10.7	5.4	9.4
Asian alone	5.4	4.7	5.4
Some other race alone	5.7	3.5	4.1
More than one race	1.9	6.0	1.7
Sex	**ACS**	**Unweighted**	**Weighted**
Male	52.6	38.5	52.2
Female	47.4	61.5	47.8
Class of Worker	**ACS**	**Unweighted**	**Weighted**
Private for profit	89.2	83.8	87.9
Private non-profit	10.8	16.2	12.1
Hours Worked Per Week	**ACS**	**Unweighted**	**Weighted**
Mean	39.0	41.6	38.2
Standard deviation	(11.6)	(107.8)	(13.2)
Age	**ACS**	**Unweighted**	**Weighted**
Mean	40.5	41.1	40.7
Standard deviation	(13.4)	(12.9)	(12.8)
Observations	**ACS**	**Unweighted**	**Weighted**
Unweighted Sample	950,547	4,630	
Weighted Sample	101,833,586		100,633,025

Table 3.24: Summary Statistics

	%	Cum.
Current job situation		
One job	75.14	75.14
More than one job	7.91	83.05
Unemployed	16.95	100
Type of Employer		
Private For Profit	87.93	87.93
Private Non-Profit	7.91	95.9
Public Health-care System	4.15	100

Table 3.25: Ever Heard of Noncompetes?

Heard of Noncompetes?	
Yes	63.02%
No	36.98%

(table not shown). Assuming that those who have never heard of a noncompete did not sign one, or those who cannot remember if they did or not, then respondents report signing a noncompete with 21.8% of their employers in the last 5 years. Those who have heard of noncompetes report signing a noncompete with 25.7% of their employers in the last 5 years.

Table 3.26: Ever Signed vs Ever Heard of Noncompetes?

	Heard of Noncompetes?		
	No(%)	Yes(%)	Total(%)
Ever signed a noncompete?			
Yes	0	39.2	24.7
No	0	57.7	36.4
Don't know if ever signed	0	3.1	2.0
Never heard of noncompetes	100	0	37.0
Total	100	100	100

Table 3.27 considers what percentage of those who have ever signed or have heard of noncompetes have also currently signed a noncompete in their job. Of those who have ever

signed a noncompete, 45.8% report having signed one in their current or most recent position. Of those who have definitely said that they have or have not signed a noncompete, 19.3% report signing a noncompete. Of those who have heard of noncompetes, 17.9% report having signed one currently. In the overall sample, including those who say they don't know if they have ever signed a noncompete, those who cannot remember if they have signed one currently, those who don't want to say they have signed, and those who have never heard of noncompetes, 11.3% report signing a noncompete in their current occupation.

Table 3.27: Currently Signed vs Ever Signed or Heard?

	Ever Signed?		Ever Heard?		
	Yes(%)	No(%)	Heard(%)	Never heard(%)	Total(%)
Currently signed a noncompete?					
Yes	45.8	0	17.9	0	11.3
No	44.8	100	75.3	0	47.4
Cannot remember	7.7	0	3	0	1.9
Don't want to say	1.6	0	0.6	0	0.4
Don't know if ever signed	0	0	3.1	0	2.0
Never heard	0	0	0	100	37.0
Total	100	100	100	100	100

Due to the innate challenge in identifying the incidence of noncompetes when workers themselves are unaware of the noncompetes they might have signed, we asked respondents who had ever signed a noncompete if they had ever unknowingly signed and later became aware of their noncompete. The responses indicate that 9.4% of workers experienced this phenomenon. The point of this exercise is to gain some insight into the extent to which workers who had not heard of noncompetes may have potentially signed them.

Throughout the rest of the paper, the incidence numbers will be presented two ways: (1) as a percentage of all respondents, assuming those who have not heard of noncompetes, those who cannot remember if they signed one, and those who do not want to say did not in fact sign one, and (2) as a percentage of those who have either replied "yes" or "no" that they have signed a noncompete in their occupation. While the first calculation should be seen as an underestimate because at least some of those respondents we assume did not sign likely did, while the second calculation likely overestimates the proportion who sign because those who have not heard of noncompetes may be less likely to have actually signed them. We prefer the conservative estimates of (1).

Next we break down the incidence of noncompetes by the worker's class, education level, occupation, annual compensation, expected employment duration, and job characteristics. Then we turn to firm characteristics such as industry, firm size, and poaching rates.

3.3.2 Worker Class

There has been no literature on the utilization of noncompetes in private for-profit, private non-profit, or public non-profits such as public healthcare systems. Anecdotes exists of unpaid interns or volunteers signing noncompetes, but there is no empirical evidence. To provide the first estimates, Table 3.28 cross tabulates worker class with noncompete status.

Table 3.28: Class of Worker and Noncompetes

	Private for profit (%)	Private non-profit (%)	Public healthcare (%)
Currently signed a noncompete?			
Yes	12.2	4.1	7.2
No	48.4	47.9	30.1
Cannot remember	2	0.8	2.5
Don't want to say	0.4	0.1	0.6
Don't know if ever signed	2	1.3	2.3
Never heard	35	45.6	57.3
Total	100	100	100

Table 3.28 shows that 12.2% of those in private for profit companies sign noncompetes, whereas 7.2% in public healthcare systems sign and 4.1% in private non-profits sign. These

numbers are upper bounds because they assume that all those who can't remember, don't want to say, don't know if they signed, or never heard of noncompetes, did not in fact sign. An upper bound is given by dividing the proportion who signed by the sum of those who signed and those who definitively did not sign. These upper bounds are 24.1% for private for profit firms, 7.9% for private non-profits, and 19% for those in a public healthcare system.

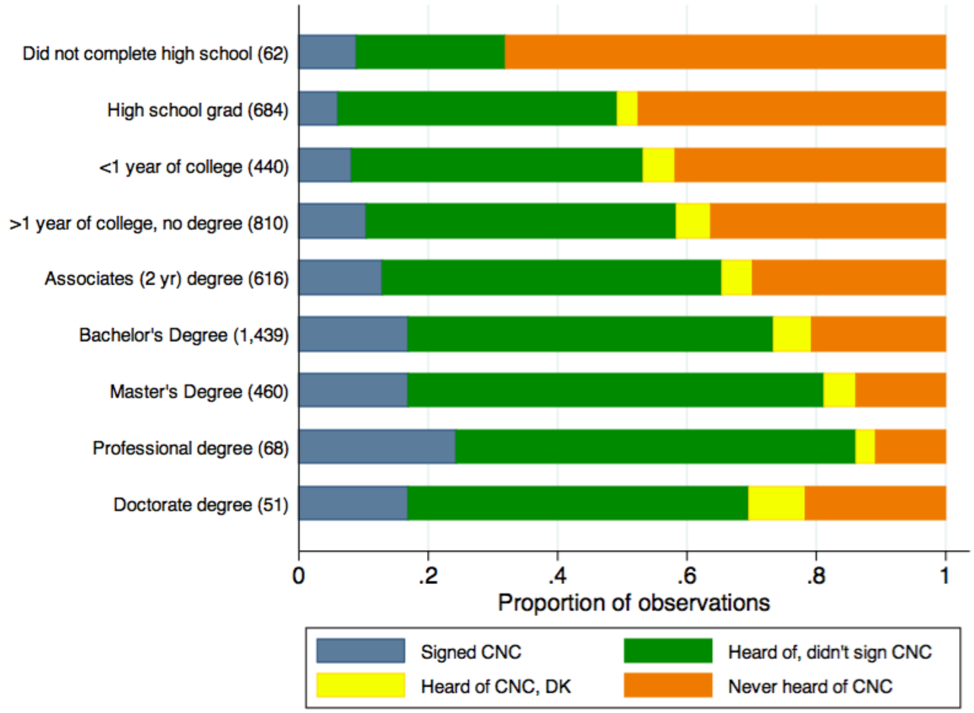
3.3.3 Incidence by Education

Figure 3.3.1 shows the proportion of each education bin that signs, doesn't sign, doesn't know, or hasn't heard about noncompetes. In order to understand the precision of these estimates, the underlying number of observations in the unweighted sample is in parentheses next to the education level. Interestingly, the proportion of those who never heard shrinks monotonically from 66.1% for those who did not complete high school to 11.8% for those with a professional degree. Receiving more education is positively correlated with having heard of noncompetes.

To better examine the proportion of those that sign noncompetes, Figure 3.3.2 plots the proportion of those that sign as both a percentage of those who have heard, and as a percentage assuming all others have not signed. The data show that the proportion of those who sign noncompetes increases substantially with education. The navy line shows that at least 6.2% of high school grads sign noncompetes, while at least 24.4% of those with a professional degree sign. More surprising, however, is incidence for those with less than a bachelor's degree. Often presumed only to be used with workers with professional degrees such as physicians, the conservative estimate of the incidence for those with just a bachelors degree is 17%.

The key takeaway from this section is that noncompetes are not only for highly educated workers. For the lowest incidence education category, high school graduates, the incidence

Figure 3.3.1: Proportion of Education Levels Signing CNC



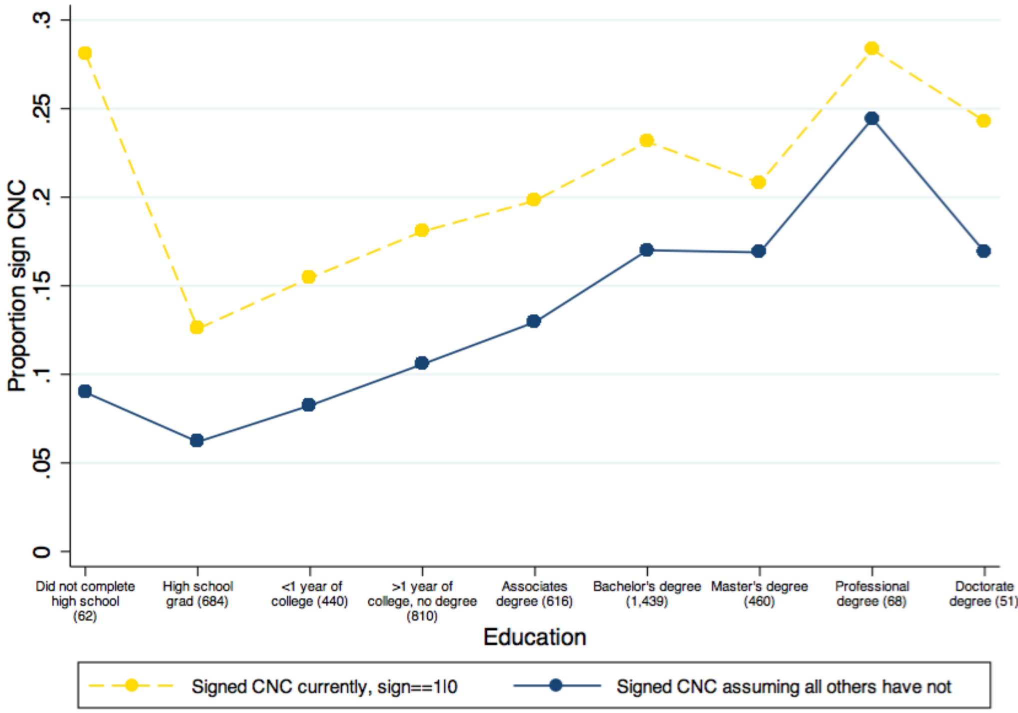
is still between 6.2% and 12.5%.

3.3.4 Incidence by Occupation

This section characterizes the incidence of noncompetes by occupation category, which the respondents self-selected from a list in the survey. The occupation categories in this section represent the 2 digit Standard Occupational Classification (SOC) system codes. Detailed information on job titles and job duties were also collected in order to pursue a finer occupational analysis, but have yet to be coded. Figure 3.3.3 shows the weighted occupation distribution in the data. The most frequent occupations are management, sales, office support, and food preparation and serving.

Figure 3.3.4 looks at the proportion of each occupation in the weighted sample that signs a

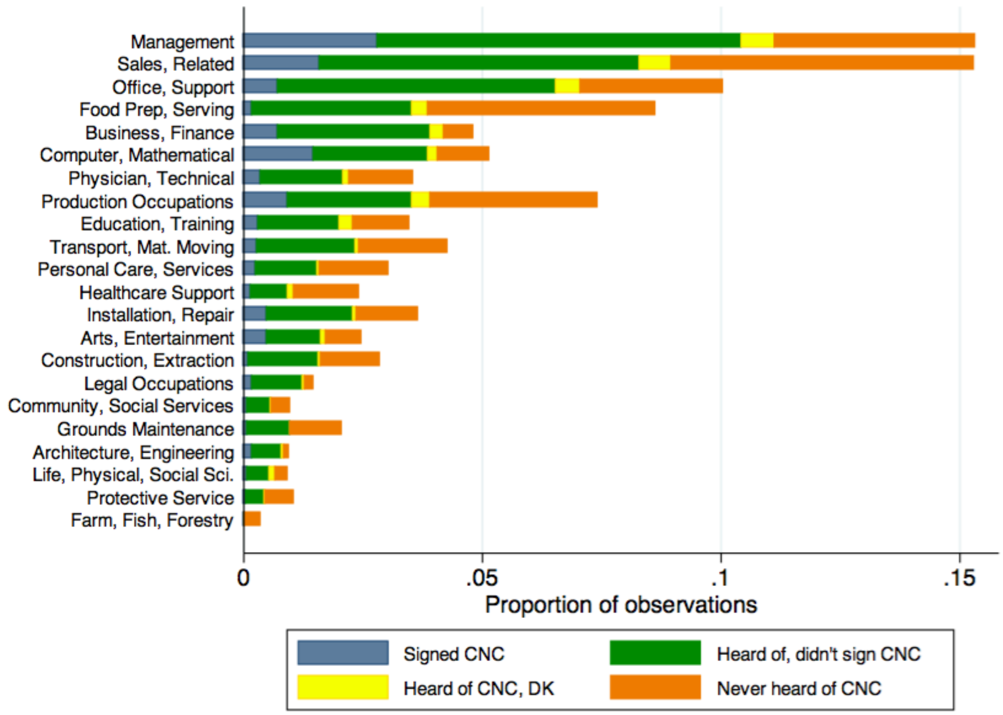
Figure 3.3.2: Education vs Signing CNC



noncompete. The number in the parentheses is the number of observations in the unweighted sample which correspond to that occupation. The occupations which are least likely to have heard of noncompetes' are architecture and engineering, legal occupations, business and finance, and computer and mathematical occupations. The fact that more people in these high skill, high earnings occupations suggests that regardless of whether they have currently signed a noncompete, they are more frequent in their line of work. The occupations which appear to sign noncompetes most frequently are management, computer and mathematical occupations, arts and entertainment, and architecture and engineering.

Table 3.29 gives the incidence by occupation assuming all others had not signed and as a percentage of those who definitely said yes they have signed or not. Occupations which have the highest rates of noncompete utilization are computer and mathematical occupations, with between 28.4% and 37.8%, architecture and engineering occupations with between

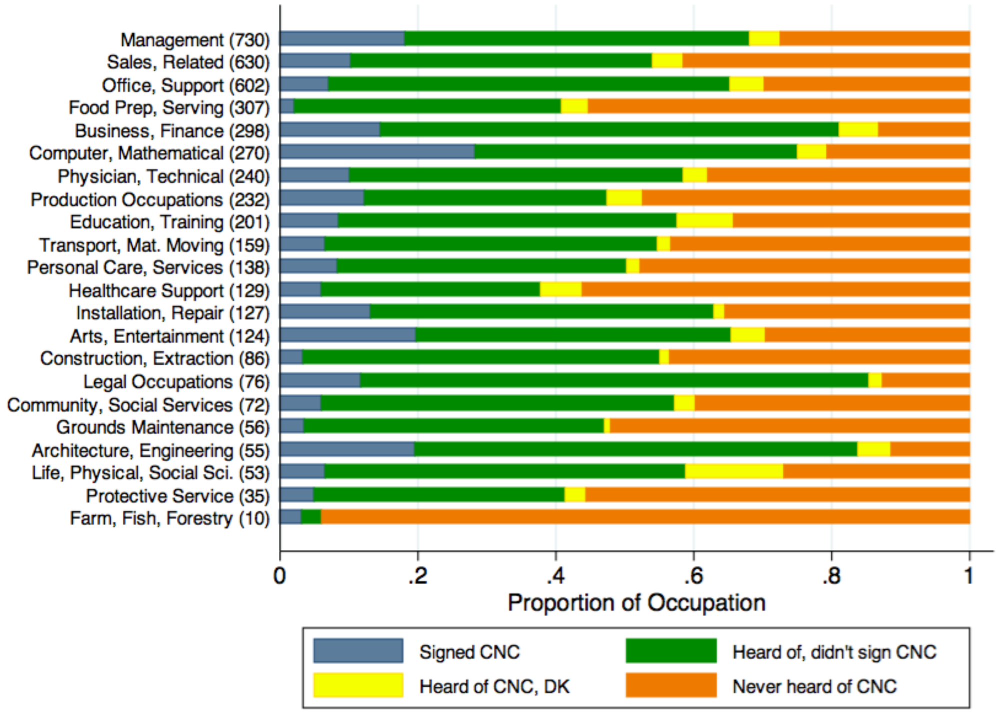
Figure 3.3.3: Occupation Distribution by Current CNC



19.6% and 23.4%, and management occupations with between 18.2% and 26.7%. A few of the more surprising occupations with high incidence rates are arts and entertainment, with between 19.7% and 30.1%, installation and repair, with between 13.2% and 21%, and production occupations with 12.3% to 25.9%. The occupations with the lowest utilization of noncompetes are food prep and serving (2.1%), farming, fishing, and forestry (3.2%), construction and extraction (3.4%) grounds maintenance (3.5%), healthcare support (6%), community and social services (6.2%), and life, physical, and social sciences (6.6%).

To sharpen our understanding of the incidence of noncompetes with each of these occupations, we asked the respondents what proportion of employees in their occupation and industry would sign a noncompete. The idea behind this question is that while the worker's experience is only one data point, his knowledge about the industry as a whole represents

Figure 3.3.4: Proportion of Occupation Signing CNC



many data points.[74]

Table 3.30 takes the average proportion that the weighted respondents suggest sign noncompetes within each occupation and across industries. These averages are displayed by the noncompete status of the respondent. Not surprisingly, those who sign noncompetes have drastically different views on their incidence in their occupation than people who have heard of them. Averaging together the projections from all the respondents, regardless of whether they had heard of noncompetes before or not, is comparable to the proportions from column (2) above, indeed even higher. The average projected incidence across all occupations is 21.8%, notably higher than the 11.4% in the weighted sample.

Figure 3.3.5 which shows histograms of the distribution of occupation projections by whether or not the respondent had currently signed a noncompete, provides a potential explanation

[74]See Rothschild and Wolfers 2013 for an example of this method in a voting context.

Table 3.29: % Signed by Occupation

Occupation	% Signed, assuming all others didn't sign (1)	% Signed if sign==1|0 (2)
Architecture, Engineering	19.6	23.4
Arts, Entertainment	19.7	30.1
Business, Finance	14.6	18.0
Community, Social Services	6.2	10.8
Computer, Mathematical	28.4	37.8
Construction, Extraction	3.4	6.1
Education, Training	8.6	14.9
Farm, Fish, Forestry	3.2	51.6
Food Prep, Serving	2.1	5.1
Grounds Maintenance	3.6	7.7
Healthcare Support	6.0	15.8
Installation, Repair	13.2	21.0
Legal Occupations	11.8	13.8
Life, Physical, Social Sci.	6.6	11.2
Management	18.2	26.7
Office, Support	7.1	10.9
Personal Care, Services	8.4	16.7
Physician, Technical	10.3	17.6
Production Occupations	12.3	25.9
Protective Service	5.0	12.1
Sales, Related	10.4	19.2
Transport, Mat. Moving	6.5	11.9
Total	11.4	19.3

for this upward bias in the projections. To provide a basis for comparison, note that the mean incidence projections would equal the incidence in the sample if all workers reported that everybody in their occupation and industry shared their same noncompete experience. If workers who did not sign a noncompete report that they believe other workers in their occupation and industry do sign, then this will increase the projected incidence of noncompetes relative to observed incidence in the sample. Similarly, if workers who did sign a noncompete suggest that less than 100% of their occupation and industry sign noncompetes, then this will bias downward the results. Since the noncompete signers make up only 11% of the sample, however, this downward bias is likely to be overshadowed by the higher projected

Table 3.30: Occupation Projections

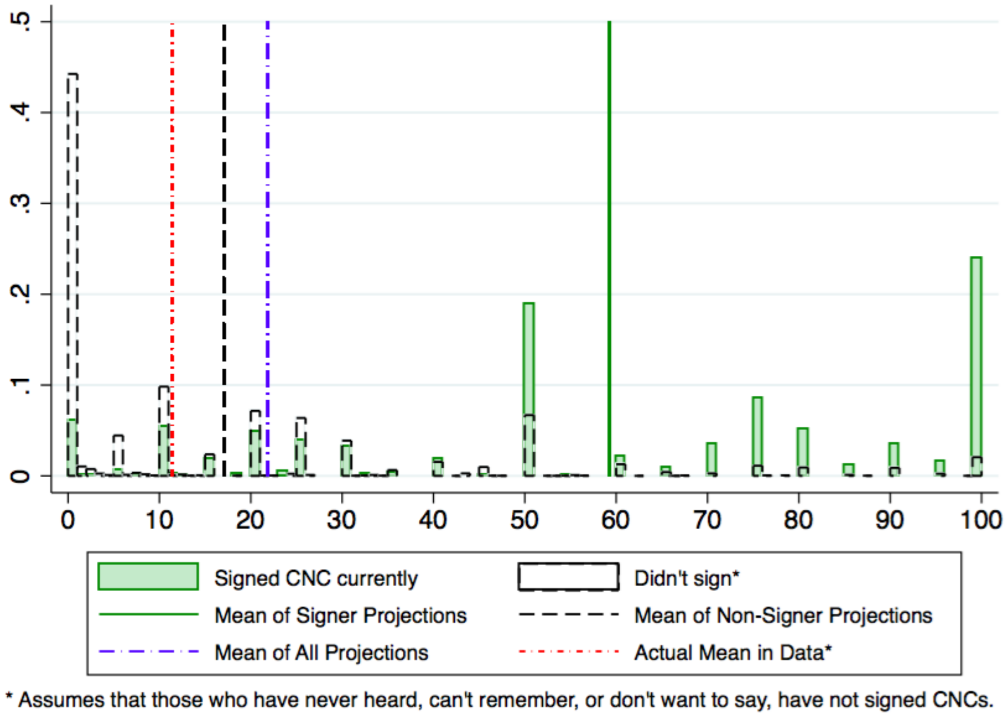
	Currently Signed (1)	Ever Signed (2)	Heard (3)	All (4)
Architecture, Engineering	72.5	45.5	31.6	33.2
Arts, Entertainment,	75.4	54.9	35.6	28.4
Business, Finance	53.4	37.2	27.9	27.6
Community, Social Services	83.5	36.8	21.5	16.1
Computer, Mathematical	60.9	45.0	36.7	33.6
Construction, Extraction	54.0	25.2	15.9	14.2
Education, Training	58.6	33.0	20.3	16.4
Farm, Fish, Forestry	10.0	10.0	5.2	12.0
Food Prep, Serving	36.8	25.0	15.6	17.2
Grounds Maintenance	77.5	37.9	22.6	19.4
Healthcare Support	62.1	43.0	26.8	21.4
Installation, Repair	58.0	43.3	25.5	22.8
Legal Occupations	52.1	32.9	23.0	21.4
Life, Physical, Social Sci.	51.8	28.2	23.0	24.0
Management	55.5	38.3	29.1	25.1
Office, Support	69.8	36.2	24.7	20.9
Personal Care, Services	55.7	37.8	21.5	20.5
Physician, Technical	52.8	37.1	25.3	20.9
Production Occupations	64.5	44.9	28.4	21.6
Protective Services	32.5	19.1	19.8	14.0
Sales, Related	59.2	41.5	25.7	20.6
Transport, Mat. Moving	61.4	36.1	18.6	15.0
Total	59.4	39.2	25.9	21.8

Note: These numbers represent averages of percentages from weighted respondents within the occupation row and column heading.

incidences of the non-signers.

Of those who have not currently signed a noncompete, 44.25% believe that they are not used in their occupation and industry at all. Correspondingly, of those who have signed a noncompete, 23.78% believe that 100% of workers in their occupation and industry sign them. Interestingly, while 19% of those who signed a noncompete say that 50% of workers in their occupation and industry sign noncompetes, the corresponding percentage of those who did not sign is 6.7%. This clustering at 50% may reflect uncertainty as respondents have no idea of the actual proportion and just guess 50%. It is clear from Figure 3.3.5 that the increase in the bias is coming from those who have not signed reporting that people in their occupation and industry have signed.

Figure 3.3.5: Distribution of Occupation Incidence Projections

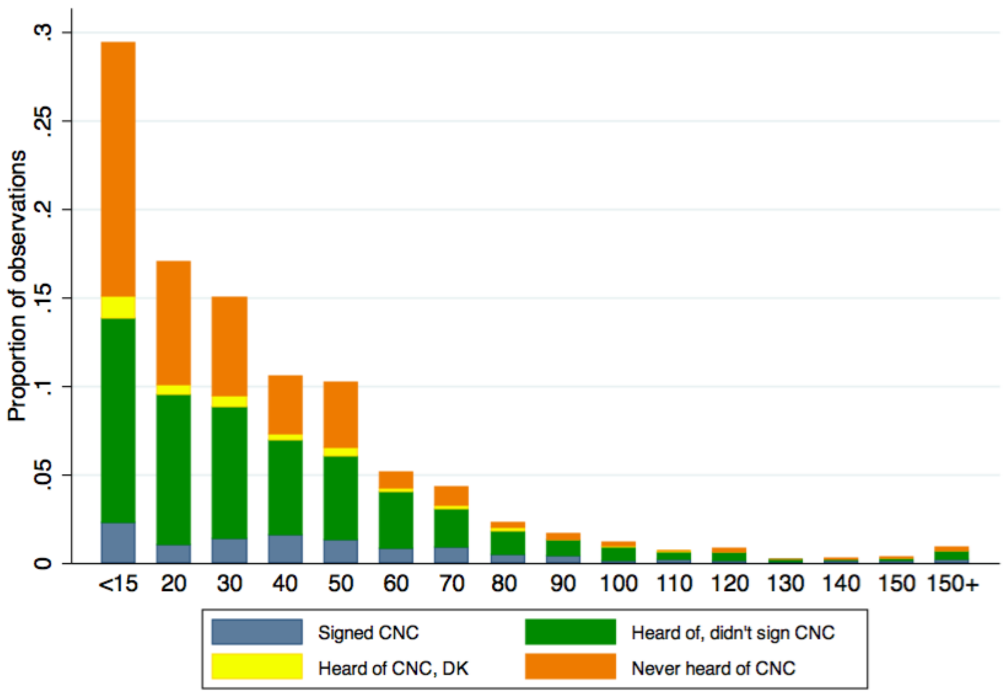


This section provides strong evidence that noncompetes are a regular feature in most occupations. LaVan (2000) and Whitmore (1990) both survey litigated noncompete cases and find that primarily salesmen, managers, and professionals go to court over their noncompetes. This new evidence shows that while the other workers may end up less in court, many of them are signing noncompetes just as often.

3.3.5 Incidence by Earnings

Next we consider how the incidence of noncompetes vary with earnings. The unweighted wage distribution is skewed to the right, with the modal respondent receiving less than $15k in annual compensation. The mean wage for the unweighted sample is $44k. Reweighting the sample to be nationally representative, the wage distribution is still skewed to the right, as can be seen in Figure 3.3.6.

Figure 3.3.6: Earnings Distribution by Current CNC

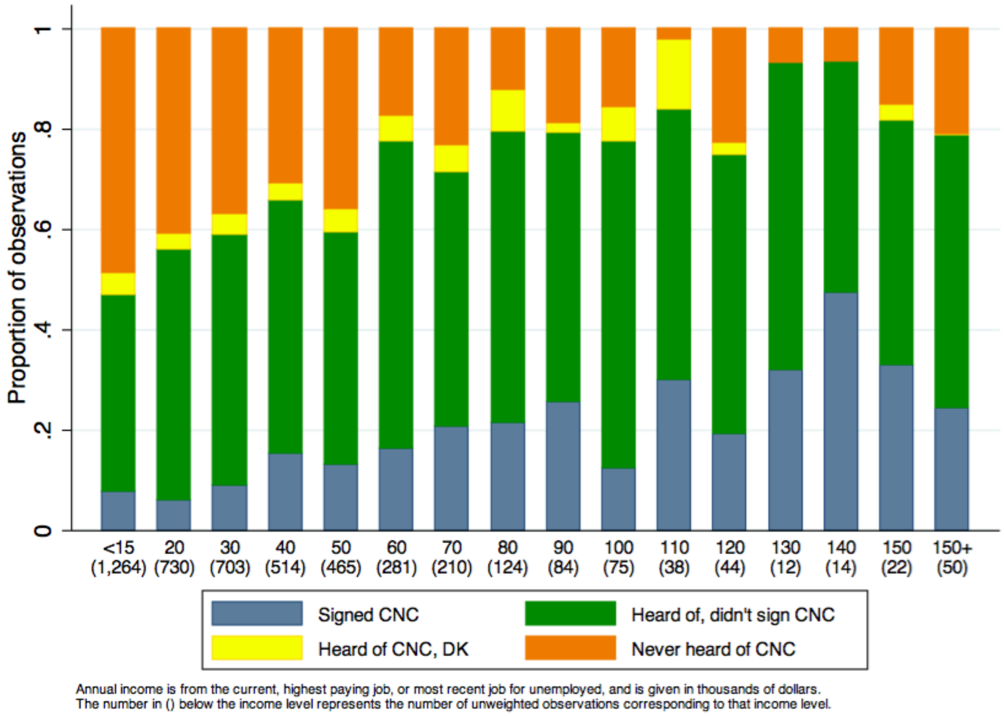


Annual income is from the current, highest paying job, or most recent job for unemployed, and is given in thousands of dollars.

Figure 3.3.7 shows the proportion of noncompete by annual compensation level. The proportion that have never heard of noncompetes falls markedly from almost 50% for those earning less than $15k to 2% for those earning $110k. This progression indicates that those earning more money are more aware of noncompetes. With regards to the incidence of noncompetes, this figure shows that the proportion of noncompete signers among all respondents generally increases with annual income.

Figure 3.3.8 shows the proportion of those who signed noncompetes as a fraction of the total weighted sample, and as a proportion of only those who have definitely signed or not. The figure shows that the proportion of noncompete signers rises rapidly from at

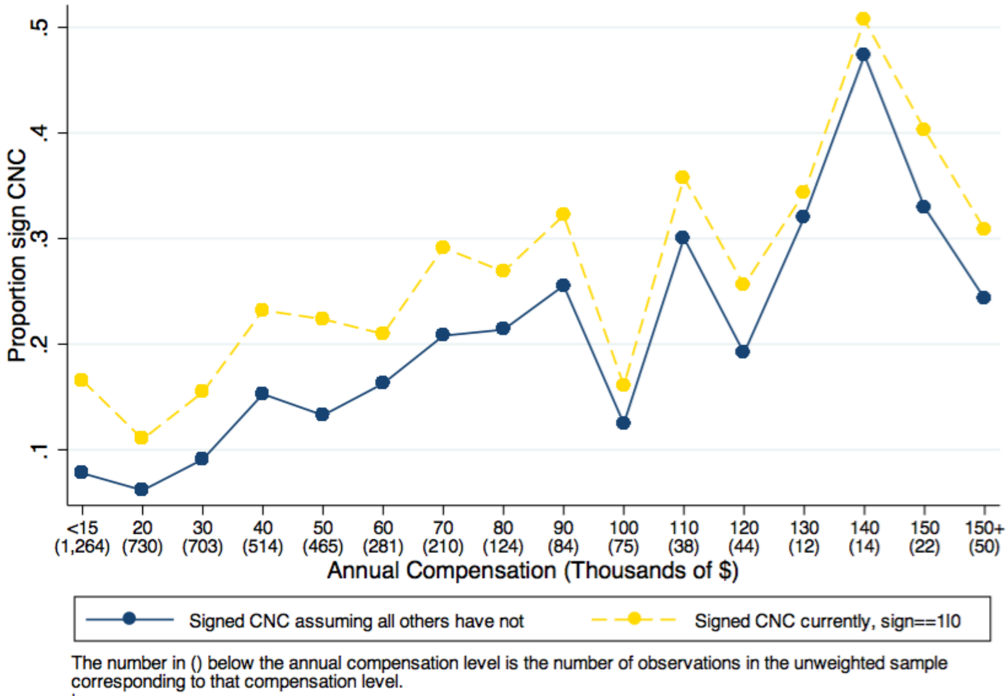
Figure 3.3.7: Earnings and Proportion Signing CNC



least 7.8% for those earning less than \$15k per year to at least 47.35% for those earning \$140k per year. While there is less data for the higher income levels, the trend is strongly positive. Importantly, however, the lower income respondents continue to sign noncompetes at relatively high rates. For example, the conservative estimate suggests that 15.3% of those earning \$40k per year sign noncompetes.

This section bears out the traditionally assumed association between the incidence of noncompetes and high paying jobs, but does not find evidence that lower paying jobs are entirely unaffected. Indeed, lower paying jobs continue to sign noncompetes at reasonably high rates.

Figure 3.3.8: Earnings and Signing CNC



3.3.6 Incidence by Legitimate Business Interest

The 'reasonableness criterion' stipulates that a necessary condition for the enforcement an employee's noncompete is that the employee's departure will harm the employer's legitimate business interests. Courts have traditionally defined these protectable interests as clients, trade secrets, and other sensitive information which is not generally known. With regard to clients, courts have often differentiated between their handling of cases in which the defendant works directly with clients and when the defendant has access to client lists or other client information (Malsberger 1996, Garmaise 2011). With this in mind, we asked respondents if they worked directly with clients, had access to client lists or client-specific information, or had knowledge or access to trade secrets.

Figure 3.3.9 shows the distribution of these characteristics of the job in the weighted sample.

Working directly with clients is the largest category, representing 33% of the sample, while 25% of the sample has none of the characteristics. A significant portion of the weighted sample, 13%, works with clients, have access to client lists, and know trade secrets.

Figure 3.3.9: Distribution of Business Interests by Signing CNC

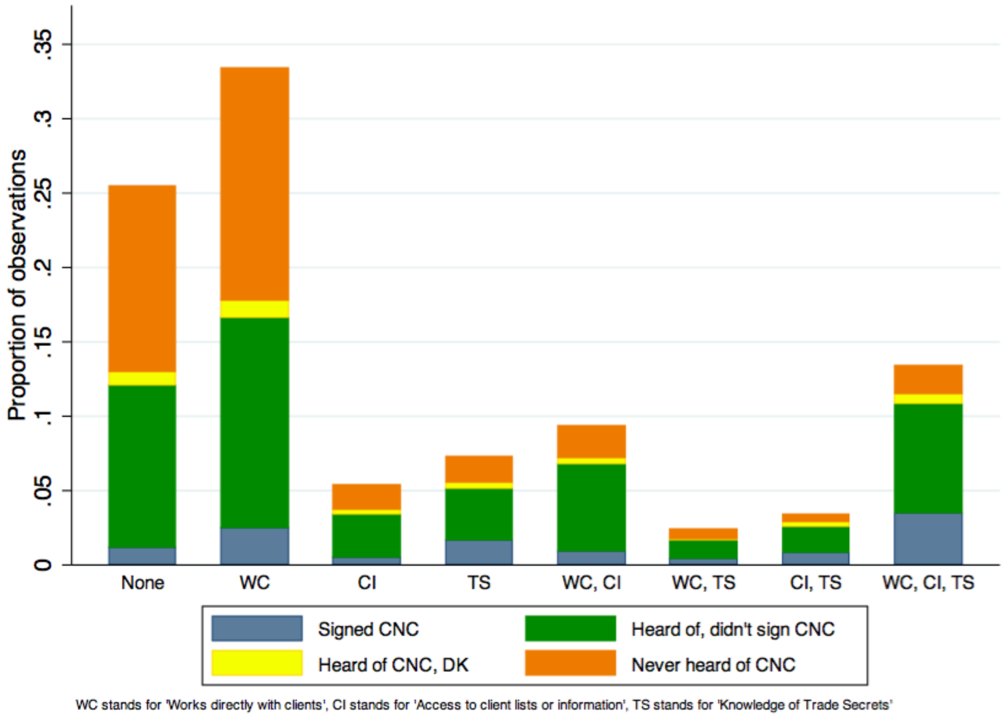
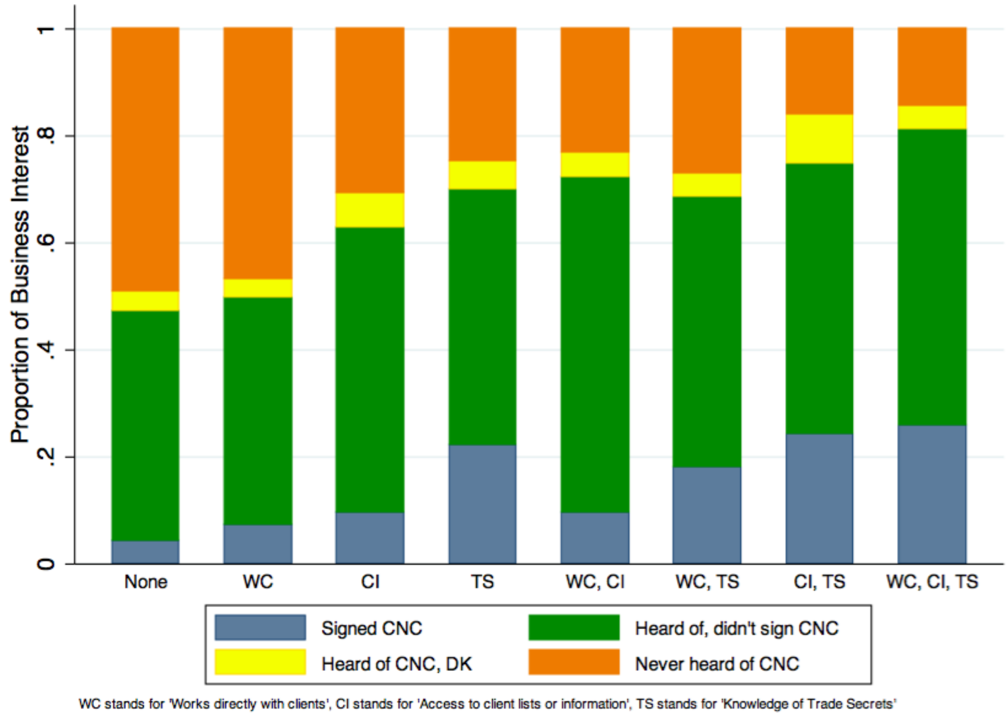


Figure 3.3.10 shows for each set of business interests the proportion of respondents according to their noncompete status. Expectedly, the lowest proportion of respondents who sign noncompetes is in the none category. As proportion of the total, those who know at least trade secrets are more likely to sign noncompetes. Those who only work with clients or do not have any of the job characteristics are the least likely to have heard of noncompetes, with 46.73% and 49.16% respectively. That proportion is is at least 16% higher than the closest business interest, access to client info with 30.73% not hearing of noncompetes.

Figure 3.3.11 plots the proportions of noncompete signers by whether in their position they work directly with clients, have access to client lists or client specific information, or know

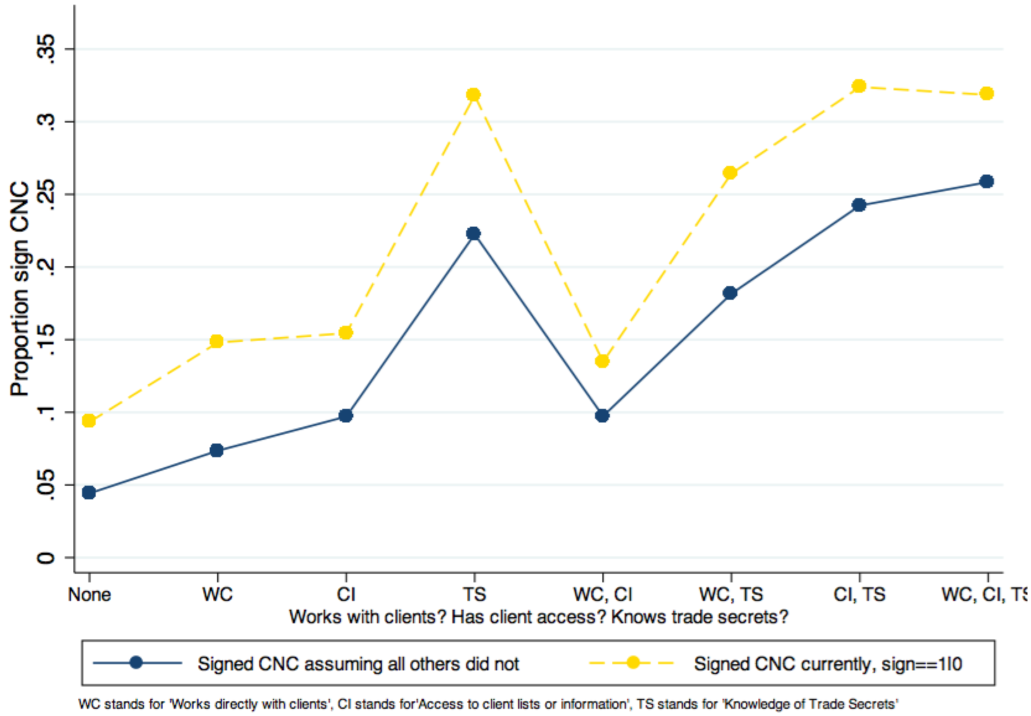
Figure 3.3.10: Proportion of Business Interest Signing CNC



trade secrets. As was apparent from Figure 3.3.10, the proportion of those who sign non-competes increases dramatically if the job includes knowing trade secrets. The incidence of noncompetes for respondents who at least know a trade secret lies between 18.7% (WC, TS) and 25.85 (WC, TS, CI)%. The highest incidence for a non-trade secret knowing position is 9.74%.

Are businesses more likely to use noncompetes in occupations which they share more valuable information or assets? Yes. Yet even for those who do not claim to have legitimate business interest worth protecting, between 4.4% and 9.3% still sign noncompetes.

Figure 3.3.11: Legitimate Business Interest vs Signing CNC



3.3.7 Incidence by Expected Employment Duration

The goal of this section is to examine to what extent the incidence of noncompetes varies by the expected employment duration. Firms and workers both face interesting incentives to offer and sign noncompetes based on the expected duration of the employment relationship. Employees deciding whether or not to sign a noncompete may only be willing to sign a noncompete if they intend to stay at the firm for a long time. Alternatively, if firms can appropriately identify the short term stayers, firms may be more likely to ask them to sign noncompetes because they know that they will be a potential threat in the near future.

Table 3.31 shows the cross tabulation of expected employment duration from the start of the employment relationship and noncompete status. Looking at the total number of observations across the bottom shows that the distribution of expected employment duration is highly skewed. The proportion of workers in the sample who expected to remain employed indefinitely is 73%. With regards to which type of employees have heard of noncompetes, 48% of those expecting to work less than one year have not heard of them while 36% of those who plan to work indefinitely have not heard of them.

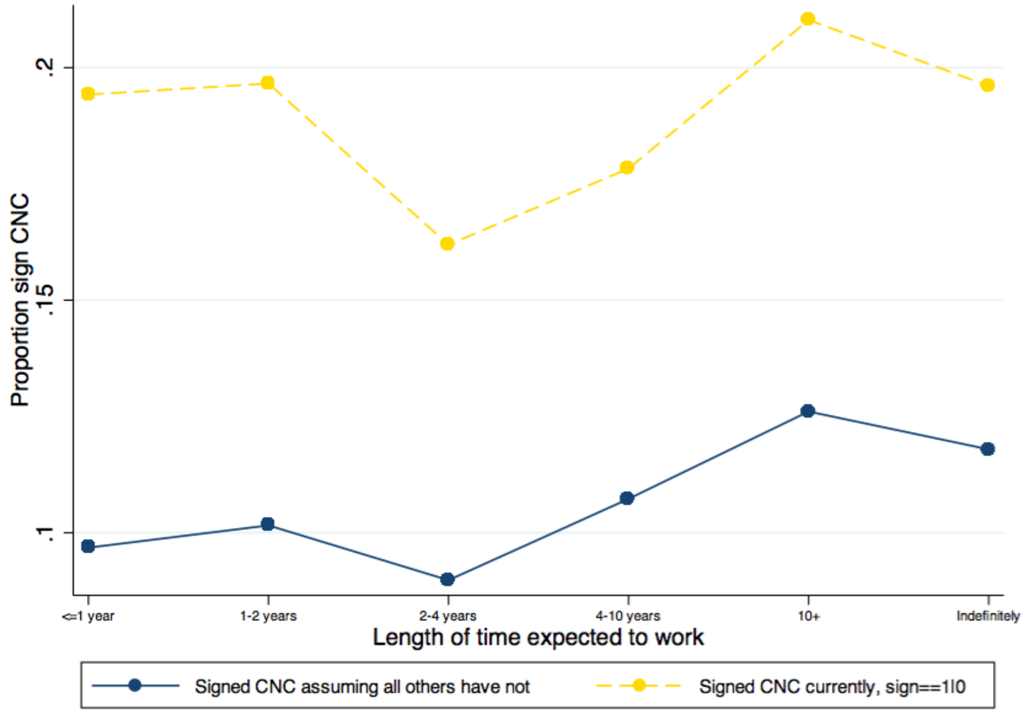
Table 3.31: Expected Employment Duration

	Expected to work					
	<1 year %	1-2 yrs %	2-4 yrs %	4-10 yrs %	10+ yrs %	Indefinitely %
Currently signed noncompete?						
Yes	9.7	10.2	9.0	10.7	12.6	11.8
No	40.2	41.5	46.5	49.4	47.4	48.3
Do not want to say	0	1.3	0.2	0.3	0	0.4
Cannot remember	0.8	3.3	3.1	2.1	1.7	1.9
Don't know if ever signed	1.7	3.1	2.7	2.1	4.9	1.8
Never heard	47.8	40.6	38.6	35.4	33.4	35.9
Total	100	100	100	100	100	100
Unweighted Proportion	5.3	4.7	5.5	10.3	1.8	72.4
Weighted Proportion	5.6	4.5	5.7	9.6	1.7	73.1

Figure 3.3.12 plots the percentage of noncompete signers both relative to all others and to all those who explicitly said they have or have not signed a noncompete. The results show that overall the proportion of noncompete signers does not vary with the expected duration. Between 9.7% and 19.4% of those who expect to work for less than one year sign noncompetes whereas the range goes from 11.8% to 19.6% for those who work indefinitely.

Overall, the data do not indicate a strong relationship between the expected length of work and the incidence of noncompetes. This could be the result of either the competing incentives of the firm and the worker, or it could be that firms which use noncompetes use them for all employees regardless of how long they expect them to be there.

Figure 3.3.12: Expected Employment Duration vs Signing CNC



3.3.8 Incidence by Establishment and Firm Size

Do firms vary in their use of noncompetes simply based on the size of their workforce? The theoretical prediction is ambiguous. While large firms by their nature are more likely to have standardized employment contracts to manage their workforce, their size mitigates the adverse effects from the departure of a key employee to a competitor. Smaller firms, on the other hand, are more likely have informal employee contracts, but also face severe consequences if a key employee were to be poached.

The distribution of employees across establishment and firm size is given by Table 3.32. While only 3.35% of the sample are employed in establishments with over 5,000 employees, 23.1% of the sample is employed by a firm with more than 5,000 employees. On the other side of the distribution, 34.5% of workers report working in an establishment with less than

25 employees, while 21.3% report working in a firm of that size. Outside of the bunching at the ends of the size distributions, the respondents are evenly distributed across the rest of the establishment and firm size categories.

Table 3.32: Establishment and Firm Size Distribution

	Establishment Size (%)	Firm Size(%)
Number of Employees		
<25 employees	35.35	21.74
25-100 employees	25.81	15.57
101-250 employees	14.90	8.97
251-500 employees	9.21	7.97
501-1000 employees	5.38	7.56
1001-2500 employees	4.09	6.87
2501-5000 employees	2.23	7.38
>5000 employees	3.03	23.94
Total	100	100

We begin our analysis of the incidence of noncompetes and firm size by providing cross tabulations of noncompete signing status and both multiunit and multi-state firms. Table 3.33 shows that 62.8% of the weighted sample work in a multi-unit firm, while 47.2% work in a firm with establishments or operations in another state. Of those in multi-unit firms, 12.8% sign noncompetes, while only 8.9% sign in single unit firms. Similarly, 14.1% of those in multi-state firms report signing noncompetes while only 8.9% of those in single state firms report signing noncompetes. Workers in multi-state or multi-unit firms are also almost 6 percentage points more likely to have heard about noncompetes.

Next we examine how the incidence of noncompetes varies by both establishment size and overall firm size. The respondents were asked separately to place their establishment size and firm size (if they indicated it was a multiple establishment firm) into pre-established size categories. They were later asked what proportion of workers in their establishment, in their occupation at their establishment, and in their firm across all establishments, sign

Table 3.33: Currently Signed vs Firm Size

	Multi-Unit Firm?		Multi-State Firm	
	Yes(%)	No(%)	Yes(%)	No(%)
Currently signed a noncompete?				
Yes	12.8	8.9	14.1	8.9
No	47.1	48.3	46.7	48.4
Do not want to say	0.5	0.1	0.7	0.1
Cannot remember	2.7	0.9	2.9	1.1
Don't know if ever signed	2.0	1.8	2.0	1.9
Never heard	34.9	40.1	33.7	39.6
Total	100	100	100	100
Proportion of N	62.8	37.2	47.2	52.8

noncompetes. Figures 3.3.13 and 3.3.14 plot both the projected incidence by whether or not the respondent has currently signed a noncompete, the average projected incidence from those who have and have not signed, and the actual incidences reported in the data..

Figure 3.3.13: Establishment Size vs Signing CNC

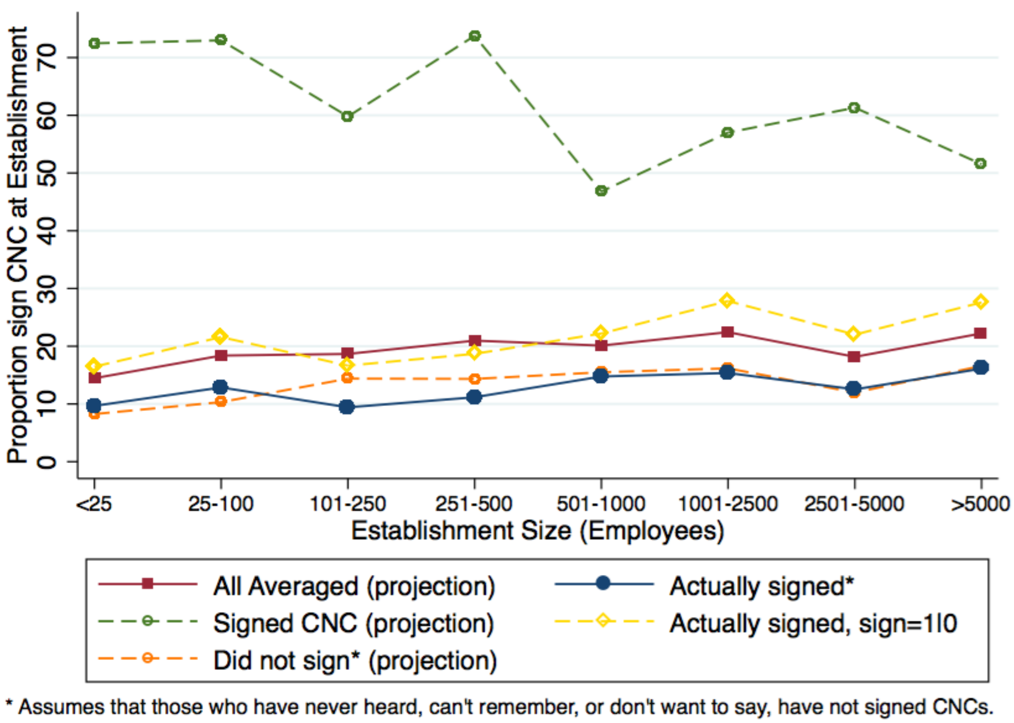
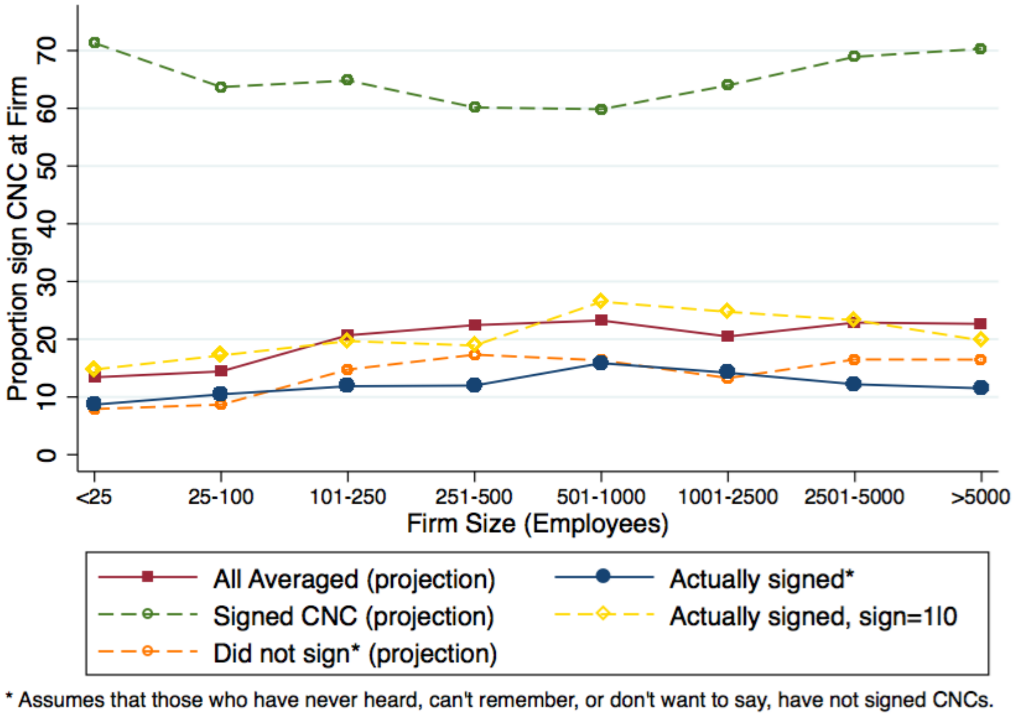


Figure 3.3.14: Firm Size vs Signing CNC

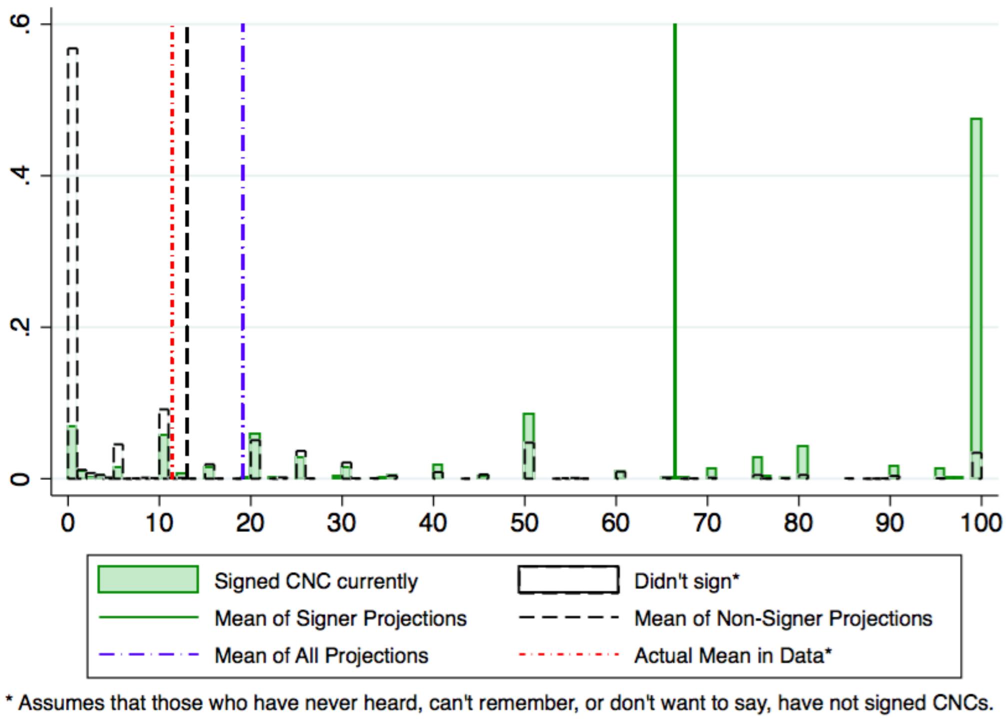


The data show a few interesting patterns. For both establishment and firm level, the difference in the incidence projections between those who sign noncompetes and those who do not is striking. For example, those who sign noncompetes project that the proportion of those who sign within their firm is between 60% and 70%, regardless of the firm's actual size. Alternatively, the incidences projected by those who currently did not sign a noncompete run between 8% for firms with less than 25 employees and 17% for firms with between 251 and 500 employees. The difference between these projections suggests that firms are not entirely selective in who they ask to sign noncompetes. Either the firms does not ask anybody to sign a noncompete, or the firm asks almost everybody to sign them.

Despite the scale of the graph, larger establishments and firms appear to use noncompetes slightly more frequently. The averaged projections shows that 13.4% of firms with less than 25 employees use noncompetes, compared to 22.6% for firms with over 5,000 employees. The corresponding numbers for establishment size are 16.1% and 22.6%.

The actual incidences bear out the same slight upward trend: 8.7% of those in firms with less than 25 employees sign noncompetes, whereas 11.5% sign them in firms with over 5,000 employees. The corresponding numbers for establishment size are 9.9% and 11.5%. Comparing the projected incidences across firms to the actual incidences observed in the data shows that again the projections are higher by between 4 and 11 percentage points.

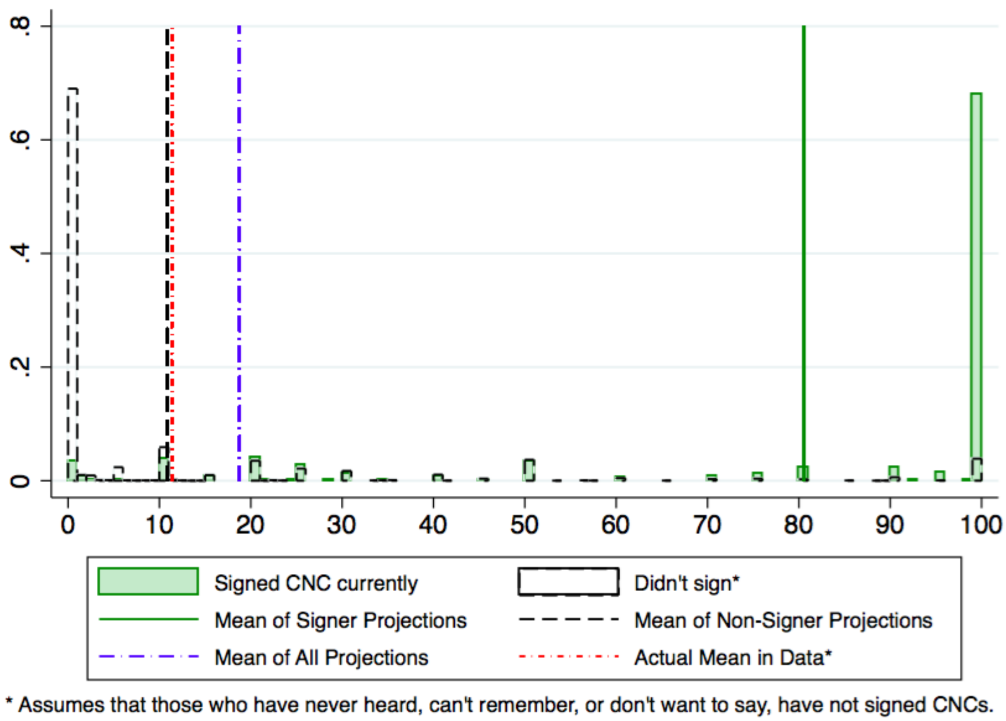
Figure 3.3.15: Firm Level Noncompete Incidence Projections



To examine the source of the differential between the projections and the actual incidence, Figure 3.3.15 shows the distribution of incidence projections within the firm by whether the worker has signed a noncompete. The histogram indicates the same pattern as before: most who do not sign project that nobody signs, while most who do sign project that everybody signs. In this instance, however, more than 40% of those who did not sign projected that a positive amount would have signed. Since the non-signers make up such a large proportion of the sample, these non-zero projections boost the average projection above the actual incidence in the sample. The same holds true for the establishment level projections.

Since the incidence of noncompetes may be hard to observe across the firm or even the establishment, we ask coworkers their projections for the incidence of noncompetes in their occupation at their establishment. Presumably, they know much more about the incidence of noncompetes among their coworkers in their same position than their firm in general. The results of the projections are plotted in Figure 3.3.16.

Figure 3.3.16: Firm Level Occupation Specific Noncompete Incidence Projections



Relative to the other projection distributions, the projections of the incidence of noncompetes among coworkers in the same occupation are the most polarized. Sixty six percent of noncompete signers report that 100% of their coworkers sign, while 70% of non-signers report that 100% of their coworkers do not sign. The other most frequent choices are 10%, 20% and 50%. We interpret the polarization in these projections to reflect actual knowledge of the incidence.

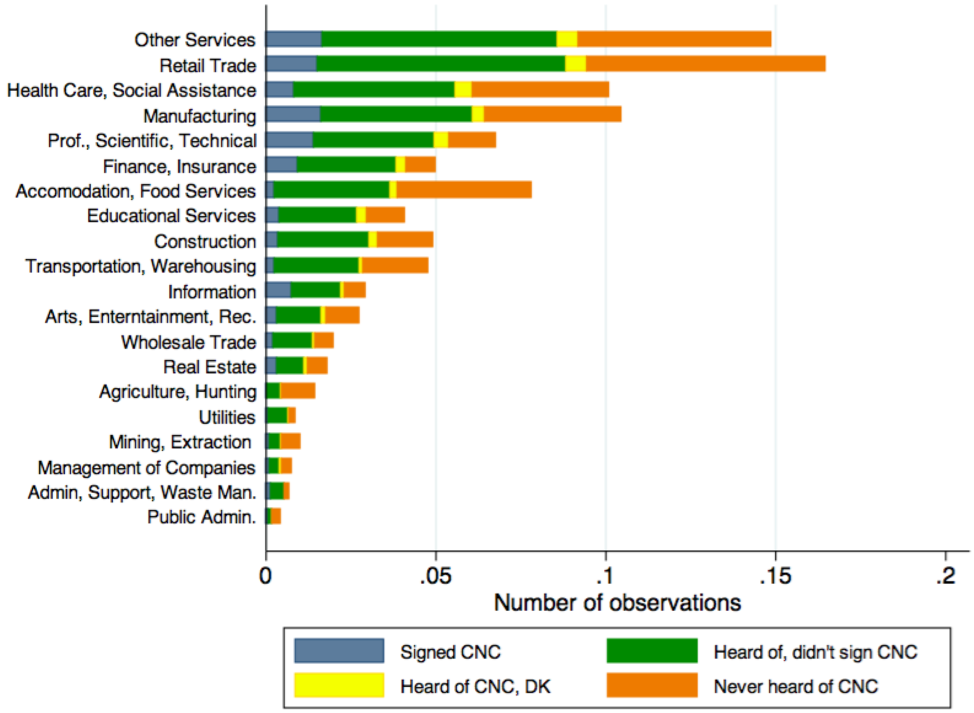
3.3.9 Incidence by Industry

We next turn to analyzing the incidence of noncompetes based on the industry of the respondent. We ascertain the industry of the respondent's current or most recent (if unemployed) job by asking the respondent to describe what their employer does or produces and to place their industry within the 2 digit NAICS codes. The results presented below correspond to the self-selected NAICS 2 digit industry, while a finer industry-level analysis is pending the coding of the data.

Figure 3.3.17 shows the industry distribution in the data by noncompete status. The most represented industries are other services, retail trade, healthcare and social assistance, and manufacturing. Utilities, mining and extraction, management of companies, support and waste management, and public administration are the least represented industries. Figure 3.3.18 shows the proportion of noncompete status by industry. The finance and insurance industry are least likely to be unaware of noncompetes while agriculture and hunting and public administration are the most likely to be unaware of noncompetes. In this weighted sample, information and professional, scientific, and technical services are the most likely to use noncompetes.

The exact proportion of noncompete signers is given in Table 3.34. The industries which exhibit the most frequent CNC usage are the information industry, with between 25.5% and 34.1% signing, and professional, scientific, and technical service companies, with between 20.9% and 28.6% of employees signing. Many of the industries the literature has focused on do show relatively high rates of noncompete utilization including, manufacturing with between 15.4% and 26.6%, and finance and insurance with between 19.1% and 24.9%. Among the more surprising industries with high incidence rates, between 12% and 20.3% of employees

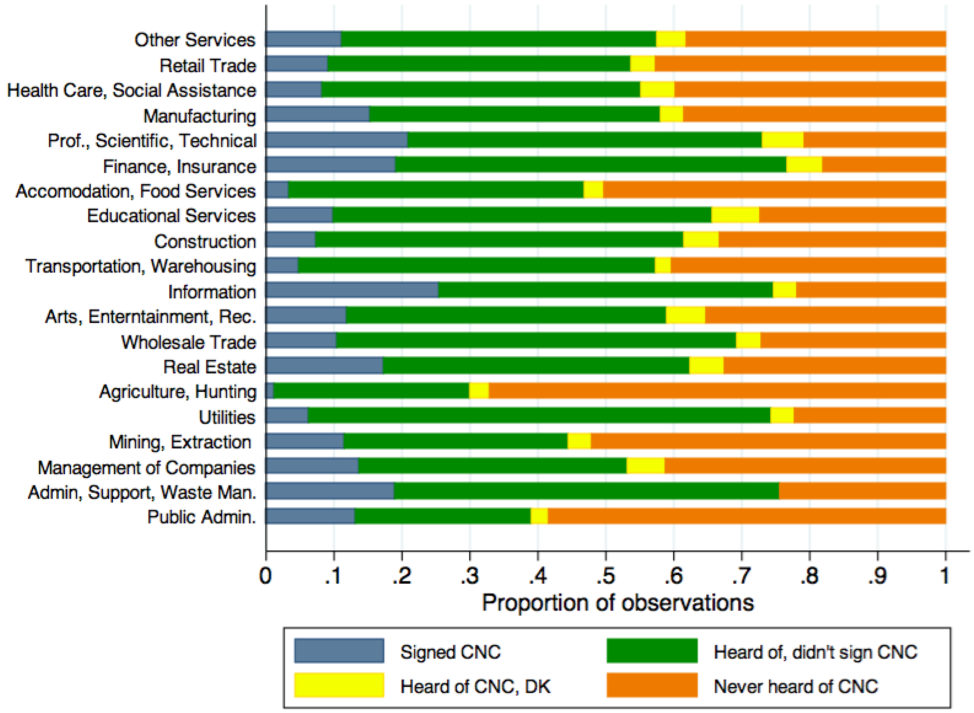
Figure 3.3.17: Industry Distribution by Signing CNC



in the arts, entertainment, and recreation industry sign noncompetes, between 17.4% and 27.9% sign in real estate, and between 10% and 15.2% sign in education services. Perhaps most surprising is the number of respondents who report signing a noncompete in a public administration position, between 13.2% and 33.9%. A quick examination of Figure 3.3.17 suggests that this public administration result is likely due to a tiny sample size, which comes about because publicly employed workers were intentionally filtered out of the survey.

While a respondent's report on whether he signed a noncompete is just one data point, we recognize that the respondent may have better information on who signs noncompetes in their industry. To corroborate the incidence of noncompetes in the data, we ask the respondents to report the incidence of noncompetes in their industry. The results are reported in Table 3.35 by whether or not the respondent has currently signed a noncompete, ever signed a noncompete, heard of noncompetes, or all averaged together. The table shows the same

Figure 3.3.18: Proportion of Industry Distribution by Signing CNC



trend as in the occupation projects: those who have currently signed think that others in their industry are also more likely to sign while those who have never signed bring down the average. The percentage in column (1) should be seen as an upper bound, since those who have signed are likely to assume that others sign, while the percentages in column (4) are likely to be a lower bound since the inclusion of those who have never heard of noncompetes does not incorporate the fact that some of them may have in fact signed one. We prefer the conservative estimates of column (4). The industry with the highest incidence of noncompetes is, surprisingly, mining and extraction with 39.6%, followed by Information with 33.1%. The industries with the lowest incidence of noncompetes are public administration (12.8%) and agriculture and hunting (13.6%).

Comparing column (4) of Table 3.35 to column (1) of Table 3.34 shows that for each industry the projections of noncompete incidence are greater than the observed incidence. To examine

Table 3.34: % Signed by Industry

Industry	% Signed, all others not sign (1)	% Signed if sign==1|0 (2)
Accomodation, Food Services	3.4	7.2
Admin, Support, Waste Man.	19	25.1
Agriculture, Hunting	1.3	4.3
Arts, Enterntainment, Rec.	12	20.3
Construction	7.4	12.0
Educational Services	10	15.2
Finance, Insurance	19.1	24.9
Health Care, Social Assistance	8.3	15.1
Information	25.5	34.1
Management of Companies	13.8	25.9
Manufacturing	15.4	26.6
Mining, Extraction	11.5	25.8
Other Services	11.2	19.4
Prof., Scientific, Technical	20.9	28.6
Public Admin.	13.2	33.9
Real Estate	17.4	27.9
Retail Trade	9.2	17.1
Transportation, Warehousing	4.9	8.6
Utilities	6.3	8.5
Wholesale Trade	10.5	15.2
Total	11.4	19.3

the source of the difference, Figure 3.3.19 presents the histogram of projections by whether or not the respondent currently signed a noncompete. The source of the difference is clearly from over 60% of non-signers indicating that some positive percentage of workers do sign in their industry. Their responses tend to cluster at 10, 20, 30, and 50, which boosts up the average of the industry. This upward boost is countered by the fact that 20% of those who sign noncompetes report that 50% of employees in their industry sign them. Because signers make up only 11% of this sample, the downward pressure is dominated by the non-signers indicating non-zero incidences within their industry.

Due to the importance of technological innovation, the noncompete debate has been focused on technology industries, comparing outcomes in Silicon Valley to outcomes in Route 128

Table 3.35: Industry Projections

	Currently Signed (1)	Ever Signed (2)	Heard (3)	All (4)
Accomodation, Food Services	46.6	24.7	17.9	15.5
Admin, Support, Waste Man.	45.5	33.2	22.9	19.8
Agriculture, Hunting	95.0	48.6	24.7	13.6
Arts, Enterntainment, Rec.	65.3	44.2	26.8	21.7
Construction	22.2	19.5	16.6	14.6
Educational Services	50.8	28.2	21.8	20.8
Finance, Insurance	51.0	38.9	28.3	26.7
Health Care, Social Assistance	52.1	40.4	24.9	21.8
Information	65.5	51.0	38.8	33.1
Management of Companies	43.3	34.3	31.1	29.3
Manufacturing	53.7	43.6	29.2	26.9
Mining, Extraction	50.2	39.2	31.8	39.6
Other Services	52.9	36.6	24.4	19.2
Prof., Scientific, Technical	51.0	40.4	31.8	30.9
Public Admin.	27.0	19.0	16.2	12.8
Real Estate	80.6	45.4	33.1	26.0
Retail Trade	56.8	36.5	22.2	18.5
Transportation, Warehousing	39.4	24.6	19.4	14.7
Utilities	60.6	35.4	23.7	21.7
Wholesale Trade	46.0	34.5	23.6	18.4
Total	52.7	37.7	25.5	21.8

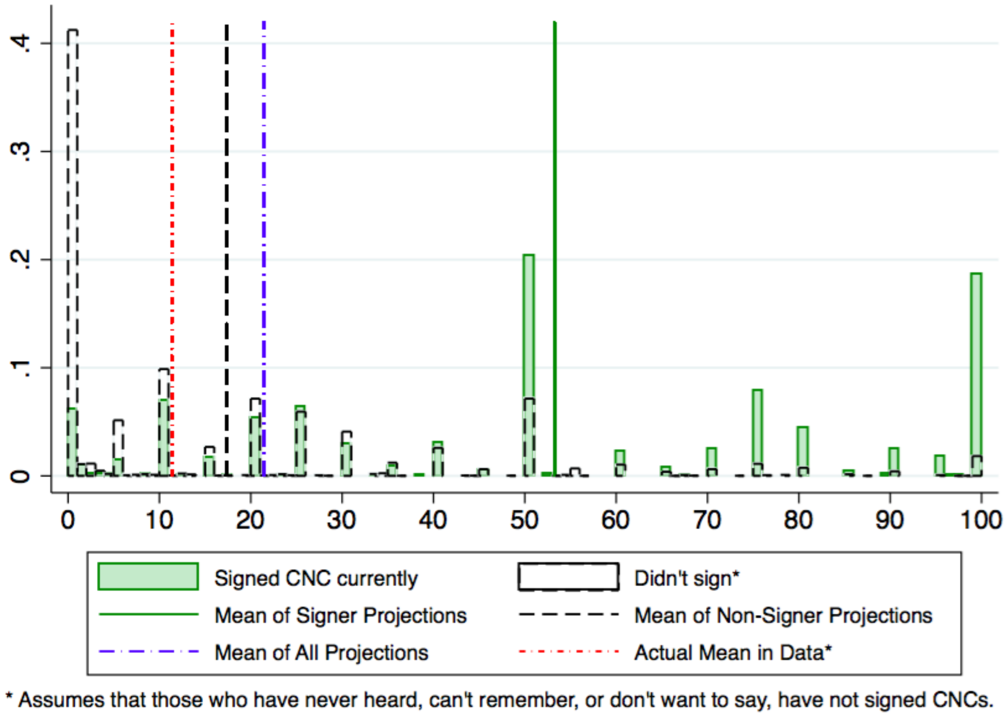
Note: These numbers represent averages of percentages from respondents within the industry row and column heading.

(Gilson 1999, Hyde 2003). While the tech industry is rather important, the particular focus on tech obscures the fact that noncompetes are being used in everything other industry in relatively equal measure.

3.3.10 Incidence by Industry Poaching Rates

Since noncompetes are used to prevent movement between competitors, this section tracks how likely a noncompete is corresponding to the frequency of poaching in the industry. Poaching was gauged by three questions asking how often the respondent's employer poaches,

Figure 3.3.19: Industry Noncompete Incidence Projections



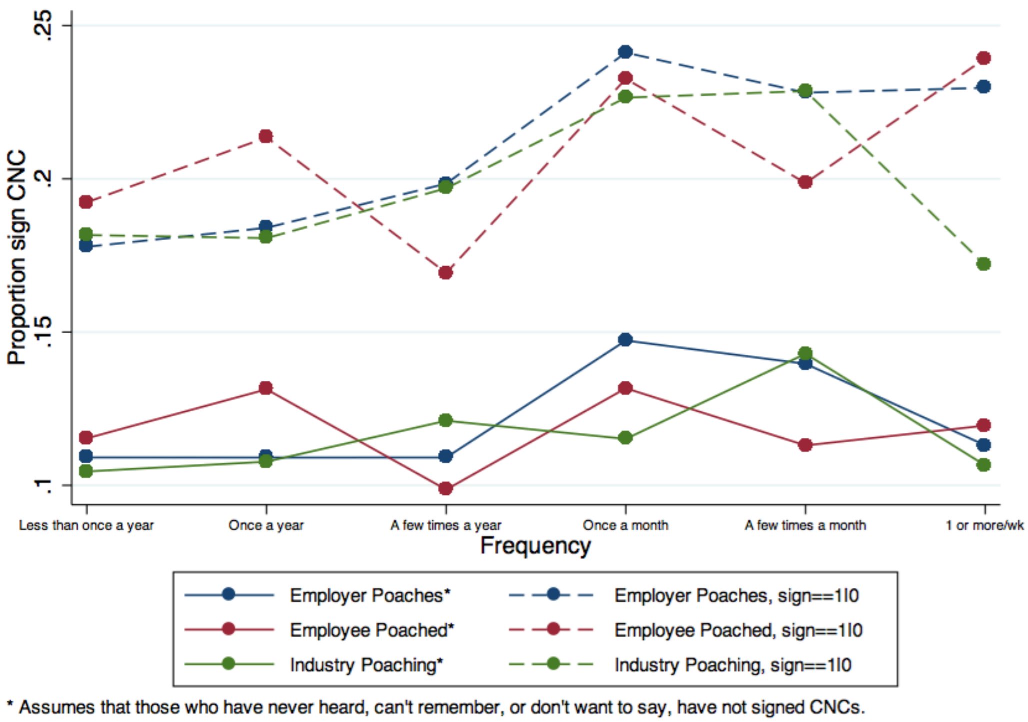
how often employees leave the employer for a competitor, and how frequently employees move between competitors in the industry in general. Table 3.36 shows summary statistics for the three poaching measures. About half of the sample report that poaching occurs less than or equal to once a year. About 10% of the sample reports being in an industry in high poaching industry where poaching occurs a few times a month or more.

Figure 3.3.20 plots the proportion of noncompete signers by their poaching frequency. Each of the poaching measures show roughly the same trend: Noncompete incidence does not strongly covary with poaching frequency. For example, in industries where employers poach other employees less than once a year, the incidence of noncompetes is 10.9%, while in industries where employees poach other employees once a week of more, the incidence is 11.3%. The numbers are similar for employees leaving for a competitor and industry poaching rates in general.

Table 3.36: Poaching Rate Summary Statistics

	Employer Poaches (%)	Employee Poached (%)	Industry Poaching (%)
Frequency			
Less than once a year	45	40.1	39.6
Once a year	10.9	13.3	12.6
A few times a year	28.6	29.7	28.5
Once a month	6.3	6.8	8.1
A few times a month	6.2	7.3	7.7
Once a week or more	2.9	2.8	3.5
Total	100	100	100

Figure 3.3.20: Poaching Rates vs Signing CNC



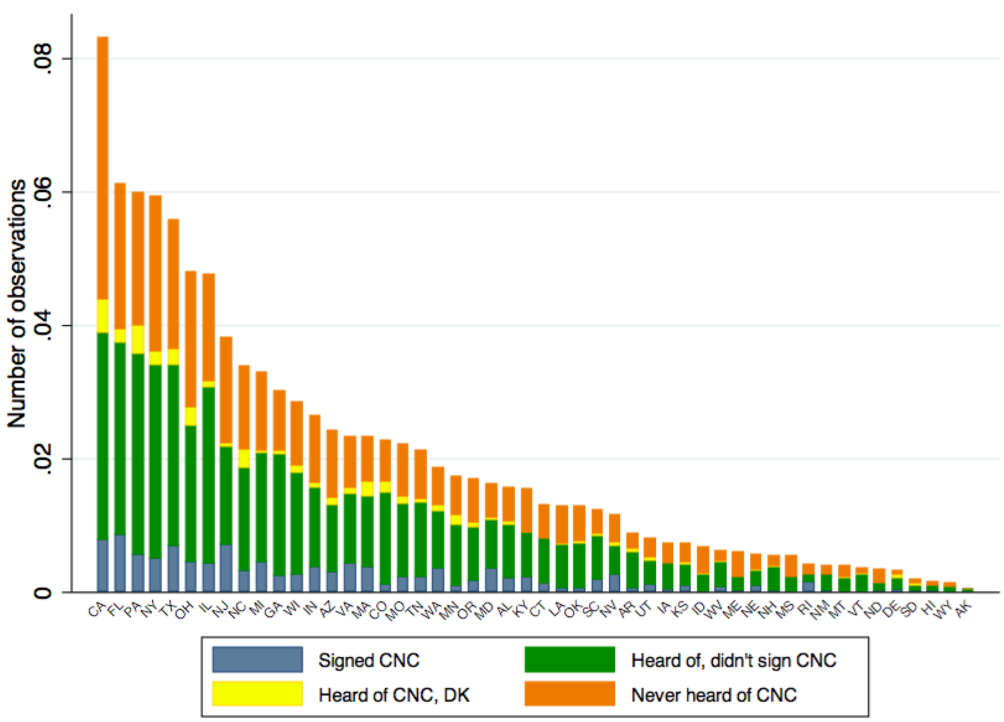
If noncompetes are truly meant to protect valuable information from leaking to competitors, then their incidence should be higher in industries characterized by high frequency poaching. The fact that the data does not indicate this suggests that firms have other, more dominant motives for using noncompetes.

3.3.11 Incidence by Noncompete Enforcement

States vary substantially in their noncompete enforcement policies (Bishara 2011, Garmaise 2011, Malsberger 2011). To the extent that firms may actually want to take advantage of their state's noncompete enforcement policy, higher enforcement policies should be correlated with the incidence of noncompete utilization.

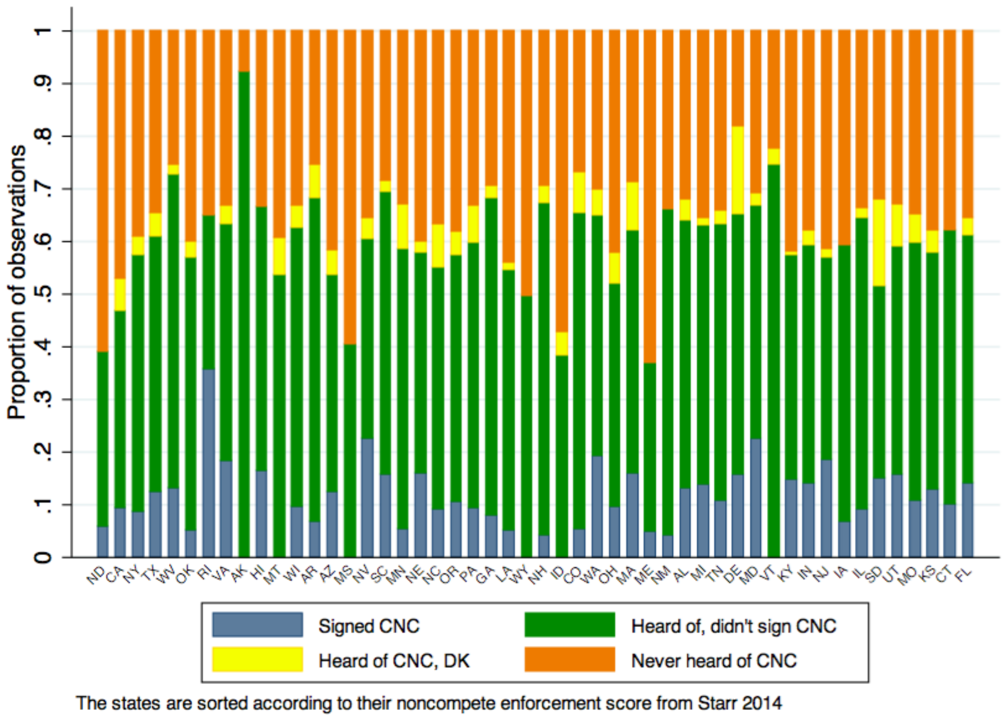
The distribution of states in the data is given by Figure 3.3.21. California, New York, Pennsylvania, and Florida represent the highest proportion of the sample. The corresponding table of proportions signing noncompetes is given in Figure 3.3.22, where states are sorted by their noncompete enforcement level. The figure shows that the proportion of people who have never heard of noncompetes is not impacted by state enforcement policy.

Figure 3.3.21: State Distribution by Signing CNC



The proportions signing from Figure 3.3.22 are plotted in Figure 3.3.23, where the x-axis

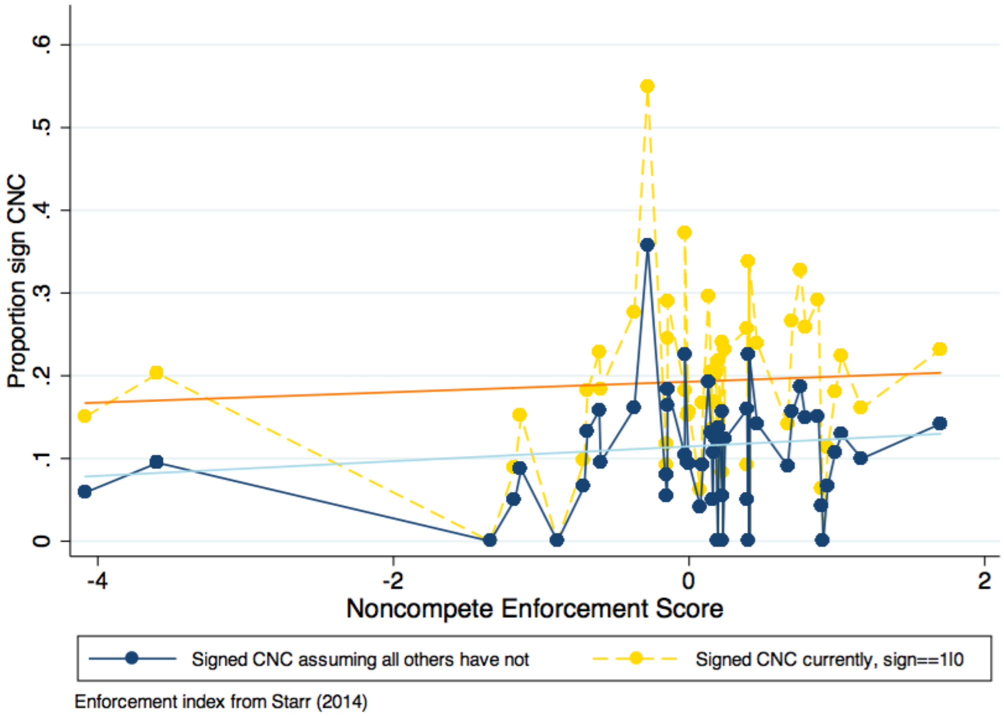
Figure 3.3.22: Proportion of State Signing CNC



reflects the noncompete enforcement score of the state from Starr (2014). The evidence supporting a positive relationship between overall incidence and state policy is relatively weak. The correlation between the incidence of noncompete enforcement in a state and the state's enforcement policy is 0.25. Of course there could within state differences by occupation and industry, but overall incidence only rises slightly with state enforcement.

A last important point in this section is that at least 11% of those in California and at least 13% of those in North Dakota report signing noncompetes. These states are particularly interesting because of their refusal to enforce noncompetes. The data show that the lack of enforcement in those states does not appear to deter firms from requiring their works to sign them.

Figure 3.3.23: State Level Noncompete Enforcement vs Signing CNC



3.4 Multivariate Analysis

Having looked at the bivariate relationship between the incidence of noncompetes and many firm level and employee level characteristics, we turn to a multivariate analysis. Table 3.37 reports the results from a linear probability model in which the dependent variable is a dummy equal to 1 if the respondent reports signing a noncompete, and 0 otherwise. Note that because those who cannot remember or have not heard of noncompetes are assumed to have not signed, the estimates may be biased. Due to space constraints the table only presents the variables of greatest interest, though often other controls were used. The results are all weighted using the propensity score transformation discussed in Section 3.2.1, and the errors are clustered at the state level.

In column (1), the only covariates are dummies for the highest completed education level.

Table 3.37: Multivariate Analysis

Linear Probability Model: Signed a Noncompete?						
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Log Earnings		0.007***	0.006**	0.004*	0.004*	0.003
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
CNC Enforcement					0.011***	0.010***
					(0.003)	(0.003)
Private non-profit				-0.068***	-0.069***	-0.067***
				(0.011)	(0.011)	(0.014)
Public healthcare				-0.024	-0.027	-0.010
				(0.020)	(0.020)	(0.027)
Bachelors Degree	0.080*	0.045	0.041	0.042	0.043	0.028
	(0.047)	(0.048)	(0.047)	(0.047)	(0.048)	(0.047)
Masters Degree	0.079*	0.040	0.033	0.041	0.041	0.022
	(0.044)	(0.045)	(0.044)	(0.044)	(0.046)	(0.044)
Professional Degree	0.154***	0.090	0.090	0.086	0.087	0.110**
	(0.055)	(0.055)	(0.054)	(0.054)	(0.054)	(0.054)
Works with clients (WC)		0.025*	0.028**	0.031**	0.029**	0.026*
		(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Has client list/info (CI)		0.035*	0.036*	0.036*	0.034	0.035
		(0.020)	(0.020)	(0.021)	(0.021)	(0.021)
Knows trade secrets (TS)		0.163***	0.163***	0.155***	0.155***	0.138***
		(0.037)	(0.038)	(0.038)	(0.038)	(0.037)
WC, CI		0.031**	0.035**	0.040***	0.038**	0.036**
		(0.014)	(0.014)	(0.014)	(0.014)	(0.015)
WC, TS		0.127***	0.129***	0.125***	0.126***	0.126***
		(0.034)	(0.034)	(0.034)	(0.034)	(0.035)
CI, TS		0.167***	0.168***	0.163***	0.163***	0.138***
		(0.042)	(0.043)	(0.043)	(0.041)	(0.044)
WC, TS, CI		0.195***	0.200***	0.196***	0.194***	0.175***
		(0.031)	(0.030)	(0.031)	(0.031)	(0.029)
501-1000 employees			0.071***	0.069***	0.069***	0.058**
			(0.025)	(0.025)	(0.025)	(0.024)
1001-2500 employees			0.052**	0.047**	0.047**	0.042**
			(0.020)	(0.020)	(0.021)	(0.019)
2501-5000 employees			0.039**	0.036*	0.034	0.028
			(0.019)	(0.020)	(0.020)	(0.020)
5000+ employees			0.032*	0.026	0.027	0.021
			(0.017)	(0.017)	(0.017)	(0.018)
Observations	100.6 m	100.6 m	100.6 m	100.6 m	100.6 m	100.6 m
R-squared	0.020	0.073	0.077	0.082	0.084	0.111
Occ, Industry FE	No	No	No	No	No	Yes

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses, clustered at the state level. The dependent variable is a dummy for signing a noncompete. 'WC' stands for works directly with clients, 'TS' stands for knows trade secrets, 'CI' stands for has access to client lists or client-specific information. The set of controls and omitted categories are described in Section 3.4.

The omitted category is having never completed high school. The estimates show exactly what we would have surmised from the education plots. Those who receive bachelors and masters degrees are 8 percentage points more likely to have signed a noncompete than those who have not finished high school, while those with a professional degree are 15 percentage points more likely. Dummies for an associates degree, doctoral degree, and less than a college degree are used but the coefficients are omitted from the table. The relationship between signing a noncompete and obtaining a bachelors and masters degree loses significance as more covariates are added, as can be seen in columns (2)-(6).

In addition to the education dummies, column (2) includes log of annual earnings and whether the worker possesses any protectable business interest of the firm. The estimates suggest that a 1% increase in log earnings increases the probability of signing a noncompete by 0.7 percentage points. The business interests results are striking. Relative to a worker who does not work with clients, does not have access to client lists or client-specific information, and does not know any trade secrets, workers who report knowing trade secrets are at least 12 percentage points more likely to have signed a noncompete. Workers who do not know any trade secrets but work with clients or have access to client lists are about 3 percentage points more likely to have signed a noncompete. These results hold regardless of the controls.

Column (3) has the same set of controls except that it adds dummies for firm size, where the omitted category is firms with less than 25 employees (25-100, 100-250, 250-500 employees are not shown in the results table). The results show that relative to firms with less than 25 employees, employees in firms with 501-1000 employees are about 7 percentage points more likely to sign noncompetes, while employees in firms with between 1001 and 2500 employees are about 5 percentage points more likely to sign. The results for larger firms are smaller around 3 percentage points and tend to be statistically insignificant in smaller samples.

Column (4) includes the same set of controls as (3) and includes dummies for the class of the worker, hours worked per week, and tenure. The omitted category for the class of the worker is private for profit sector. The results for the class of the worker show that private non-profit workers are about 7 percentage points less likely to sign noncompetes, while public healthcare employees are about 2 percentage points less likely to sign, but the difference is statistically insignificant. The results for tenure and hours worked per week (not shown in table) show that a 1 unit increase increases the probability of signing a noncompete by 0.1 percentage points, but the estimate is statistically insignificant.

Column (5) adds state level noncompete enforcement from Starr (2014), number of years of experience in the occupation, and demographics including gender, and race. Aside from the the level of noncompete enforcement, the estimates of the demographics and experience are all small and statistically insignificant (not shown in table). A one unit increase in state-level noncompete enforcement, however, increases the probability of signing a noncompete by 1 percentage point. This estimate suggests that workers in Florida are 5.3 percentage points more likely to sign noncompetes than workers in California.

In addition to the controls from column (5), column (6) includes age of the worker and separate occupation and industry fixed effects at the 2 digit level (coefficients for age, occupation and industry not shown). The addition of occupation and industry fixed effects reduces the statistical significance of log earnings, but increases the size of the professional degree dummy, causing it to regain statistical significance. Aside from these changes, all of the main results noted above still go through with these additional controls.

3.5 Discussion

Legal scholars have long held that noncompetes are ubiquitous (Stone 2002), but only in the last few years have we begun to study their use and impacts. Previous work has studied the use of noncompetes for only a select group of occupations: CEOs (Bishara et al. 2013, Garmaise 2011), physicians (Lavetti et al. 2013), and engineers (Marx 2011). This paper expands our knowledge of the incidence of noncompetes by considering the incidence of noncompetes in a broad array of occupations and industries, and further examines how noncompete incidence varies with other firm level and employee level variables.

About 63% of the weighted sample reports knowing of noncompetes, while 40% of those report having ever signed. Of the 40% who have ever signed, 45.8% had currently signed, which represents 17.9% of those who had heard of noncompetes and 11.3% of the overall sample. The incidence of noncompetes is higher in higher earning and higher skilled occupations and industries, but even for those earning less $40k or those without a college education the incidence of noncompetes is still high. Noncompetes are not related to the poaching frequency of the industry or the expected duration of employement. Workers who know trade secrets are at least 12 percentage points more likely to have signed a noncompete. Evidence from the multivariate analysis suggests that private for-profit firms are 7 percentage points more likely to sign non competes than private non-profits, while those with a professional degree are 11 percentage points more likely to sign a noncompete than those who have not finished high school.

Given the pervasiveness of these contracts, more work needs to be done on the contents and potential effects of signing these contracts to understand how they are impacting workers, firms, and the economy as a whole.

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