Training the Enemy?

Firm-Sponsored Training and the Enforcement of Covenants Not to Compete[*]

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Abstract

This paper theoretically and empirically examines the impact of noncompete enforcement on firm-sponsored training. Theoretically, the impact depends upon how training is determined, whether by firms competing for workers by offering wage-training contracts or by firms making unilateral training decisions. In addition to providing direct evidence on the validity of the underlying assumptions of these models, I empirically test the predictions of these models by examining the impact of enforcement on training for inexperienced workers and workers with longer tenures. The empirical analysis shows that a one standard deviation increase in noncompete enforcement leads to an increase in firm-sponsored training of at least 3 percent for occupations that are likely to experience noncompete litigation. Among these occupations, the effect is strongest in the high earning, high skill occupations. The empirical tests of the candidate training models show that the marginal impact of noncompete enforcement on training is strongly positive for inexperienced workers, and rises monotonically between 3 and 8 percent in the first 20 years of tenure, implying that the unilateral firm choice model of training is operative. In states that require additional consideration in order for noncompetes to be enforceable, workers receive more training, however, suggesting that these state policies substitute to some extent for the lack of individual contracting behavior.

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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 ('noncompetes'), which prevent the worker from joining or starting a competing firm for a fixed amount of time post separation. Noncompetes are ubiquitous today, with at least 25% of the US Labor Force having ever signed one, and at least 12% currently under one (Starr et al., 2014b).[1] Despite their prevalence and 600 year history, state courts vary significantly in the circumstances under which they will enforce noncompetes (Blake, 1960; Bishara, 2011). For example, some states have a per se prohibition on enforcing noncompetes, while other states enforce them even if the worker is fired.

This paper theoretically and empirically investigates whether firms will invest more in their workers if their workers's noncompetes are more likely to be enforced. Theoretically, the assumption which generates the often presumed positive relationship between noncompete enforcement and training is that training is not contractible, leading firms to make unilateral training decisions. Under this model, noncompete enforcement reduces the positive externality to training by limiting the chance the worker will leave, allowing the firm to capture more of the returns to training. I develop a competing model which shows that when training is chosen in equilibrium as a result of firms competing for workers by offering wage-training contracts, an increase in noncompete enforcement can increase or decrease firm-sponsored training. If the worker expects to move to a competitor in which his training is more valuable, then higher noncompete 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. If the worker is relatively well matched in his current firm, however, an increase in noncompete enforcement only reduces the chance that the worker will make an inefficient quit, thereby increasing the value to training at the current firm, causing the worker to select a contract with more training. Under the contracrible model, the aggregate impact of noncompete enforcement on training, averaging across well-matched and poorly matched workers, is likely to be positive because those who care less about their mobility – the well matched – are more likely to agree to sign noncompetes in the first place.

Although the two models make the same aggregate predictions, distinguishing the operative training model is nevertheless possible. The fact that the contractible model predicts that the impact of noncompete enforcement on training is negative for poorly matched workers, while the unilateral firm choice ('not-

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[1]Starr et al. (2014b) show that there is remarkable heterogeneity in who signs noncompetes: At least one in three workers earning over $100k have signed one, while 10% of those earning less than $40k have also signed.

contractible') model predicts a positive relationship, suggests that by examining inexperienced workers who are likely to be poorly matched (Topel and Ward, 1992) we can test the predictions of the models directly. In addition to this test, under the assumption that training observed later in tenure is not contractible by its very nature of being far off in the future, examining the impact of noncompete enforcement on training later in tenure provides a direct test of the not-contractible model. Lastly, to more directly examine the underlying assumptions of the models, I incorporate direct empirical evidence from Starr et al. (2014a) and Starr et al. (2014b), which shows that 90% of noncompetes are not negotiated over, and that most training is neither contracted nor negotiated.

The empirical relationship between noncompete enforcement and observed firm-sponsored training has never before been examined because of the difficulties involved in accurately quantifying the various dimensions of enforcement. To begin the empirical analysis, I create an improved measure of enforcement which weighs seven 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 noncompete cases (LaVan, 2000; Whitmore, 1990).[2] The estimates represent an underestimate of the causal, intent-to-treat effect of state noncompete enforcement.[3]

The results show that a one standard deviation increase in a state's enforcement level increases the probability that the average high litigation occupation receives firm-sponsored training by 3% relative to low litigation occupations.[4] This estimate suggests that if lowest enforcing state, California, were to adopt the highest enforcing state's laws, Florida, then high litigation occupations in California would receive at least a 16% increase in the likelihood of receiving firm-sponsored 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, business and financial occupations, computer and mathematical occupations, health practitioners, and engineers, though personal care and services occupations are also strongly impacted by enforcement. Policies that exploit this heterogeneity, such 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.

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

[3]Since the data does not contain information on which workers actually signed noncompetes, the estimates are intent-to-treat.

[4]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.

The training effects also 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. An alternative explanation is that in high enforcing states it is difficult to attract experienced workers who are also bound by enforceable noncompetes and as a result firms tend to hire less experienced workers.

The results of two tests of the training models described above confirm the direct evidence provided by Starr et al. (2014a) and Starr et al. (2014b), suggesting that the operative training model is the unilateral firm choice, not-contractible, model. The results of the first test show that for inexperienced workers the impact of noncompete enforcement is strongly positive, which coincides with the predictions of the not-contractible model. The results of the second test show that the effect of enforcement on training rises monotonically between 3% and 8% in each of the first 20 years of tenure. 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 also suggests that the relevant model of training in that stage of tenure is also the 'not-contractible' model. Given the appropriate training model is the unilateral firm choice model, there is a clear role for noncompete enforcement because it improves training outcomes unambiguously by reducing the tendency of firms to underinvest in training.

While the bulk of evidence validates the unilateral training model, breaking the noncompete enforcement index into its individual components reveals that the contractible model still holds some sway. In particular, the results show that state policies which require firms to provide workers with additional consideration, such as additional training, wages, or other benefits, in order for their noncompetes to be enforceable, function to both reduce the enforcement of noncompetes and increase training outcomes. These laws can be conceived of generally as the state negotiating on behalf of its workers, who are generally not negotiating over their training (Starr et al., 2014a) or noncompetes (Starr et al., 2014b). Hence, these laws serve to reduce the inefficiencies from both labor misallocation and firm-sponsored training, thereby avoiding the otherwise inevitable tradeoff between increased investment and reduced mobility.

This paper makes important contributions to both the on-the-job training literature and the nascent empirical literature on the welfare effects of noncompetes. With regards to the training literature (Acemoglu and Pischke, 1999), this article provides evidence that noncompete enforcement represents a labor market friction that compresses the wage structure and induces firms to provide more training. By providing evidence that the unilateral firm choice model is the appropriate model of training, this paper underscores the recent growth in our understanding of the workings of monopsony power in the labor market (Manning, 2003). The results about the lack of worker bargaining over training, and the subsequent potential for the state to fill in the negotiation gap show that the classic training models are still important

([Becker](#), [1962](#)), but open many questions about why workers are not negotiating and whether or not it is efficient for states to pass laws which effectively negotiate on their behalf.

With regards to the literature on the efficacy of noncompetes, 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](#), [2009](#); [Lavetti et al.](#), [2014](#)) 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.[5] My results contribute to this line of inquiry by estimating an important parameter necessary to understand the overall welfare effects of noncompete enforcement: for some high skill and personal service occupations, firms are indeed responding to the increased protection of their confidential information by providing more training to their employees. Increased training need not be brought about by higher enforcement, however, since state courts can actually reduce their enforcement policies and increase training by enforcing noncompetes only when workers are provided additional consideration in exchange for signing.

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

2 Noncompetes and Noncompete Enforcement

2.1 The Incidence of Noncompetes

While claims of the ubiquity of noncompetes are common, until recently there has been very little systematic evidence on the incidence of noncompetes.[6] Figure I from [Starr et al.](#) ([2014b](#)) shows the breakdown of the incidence of noncompetes by occupation. The data show that individuals in higher-skill, knowledge-intensive occupations are the most likely to sign: engineering and architecture (30.1%), computer and mathematical (27.8%), business and financial (23.1%), and managers (22.7%). Yet even low-skill occupations such as office support (8.7%), installation and repair (10.5%), production (11.0%), and personal care and services (11.8%) involve significant noncompete activity.

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[5][Lavetti et al.](#) ([2014](#)) find that physicians who sign noncompetes tend to earn 11% more because they are allocated more clients.

[6]Previous studies consider a few high skill occupations, showing that that about 80% of CEOs sign noncompetes ([Bishara et al.](#), [2012](#); [Garmaise](#), [2009](#)), 45% of physicians ([Lavetti et al.](#), [2014](#)), 40% of engineers ([Marx](#), [2011](#)), and 70% of entrepreneurs with venture capital contracts ([Kaplan and Strömberg](#), [2003](#)). [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 noncompetes. The authors did not investigate which occupations within the firm were asked to sign noncompetes.

2.2 Quantifying Noncompete Enforcement

In this section I describe the process from noncompete signing to noncompete enforcement and in particular how enforcement policies depend upon the characteristics of that process. 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. Upon violating the terms of the noncompete, 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 noncompete. Noncompetes are considered common law and are decided by judges based on state statutes or case law precedents.[7] In 2012 there were 742 reported, litigated noncompete cases (Beck, 2013). This number is an underestimate of the vastness of the impact of noncompetes, however, because most cases settle out of court, and many workers may take career detours to explicitly avoid potential litigation (Marx, 2011).

While some states, such as California and North Dakota, refuse to enforce noncompetes, most states will enforce them by implementing their own version of the 'reasonableness doctrine,' which balances the protection necessary for the firm with the injury to the worker and society.[8] Among enforcing states there is unanimous agreement that a necessary condition for the enforcement of a noncompete 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, 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 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 noncompete and the firm has the same increased incentives to invest in the worker as if training was itself a protectable interest. 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.[9]

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[7]Interjurisdictional issues regarding noncompete 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.

[8]See Blake (1960) for an in-depth review of the history of noncompete enforcement.

[9]There exists a debate in the legal literature about whether general training should be a protectable interest. The

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 noncompete 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 noncompetes 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 noncompete 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 noncompete two weeks before employment commences. If they firms do not notify the worker in advance, the firm must provide the worker with additional benefits ex post in order for the noncompete to be enforceable. Colorado is particularly unique in that it will only enforce noncompetes for workers in upper management.[10]

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 noncompete enforcement for 2009 and 1991.[11] 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[12] increase as the number of state-level regressors increases[13] and (2) if each dimension of enforcement is considered a measurement error ridden proxy for latent noncompete enforcement intensity, then combining the measures into a single index reduces attenuation bias.[14] 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.

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 noncompete (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).

[10]See generally Malsberger et al. (2012) and earlier editions.

[11]A complete explanation of Bishara (2011) scoring method is available in Appendix C.

[12]See Goldberger (1991).

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

[14]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.

Conceuptually, factor analysis postulates that each particular dimension of enforcement depends linearly upon latent enforcement intensity. Defining x_{is} as observed enforcement dimension i for state s and $Enfc_s$ as latent enforcement intensity, the model is defined by the set of equations

$$x_{is} = \lambda_i Enfc_s + \epsilon_{is} \quad \text{for } i = 1, 2...7,$$

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 of the observed enforcement dimensions identifies the other λ_i terms because $corr(x_i, x_j) = \lambda_i \lambda_j$. Given estimates of the λ_i terms, we can back out an estimate of the enforcement index. Regressing this estimate of the enforcement index on the dimensions of enforcement gives the weights.[15] 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 I 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.

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 dimensions with the most weight are whether a state has a statute of enforceability, what 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 noncompetes are enforceable if the worker only receives continued employment in exchange for signing.

Using the 2009 weights, Figure II shows the noncompete enforcement score for each state. 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.[16] Enforcement intensity is not correlated with a state's political leanings (Lavetti et al., 2014) and does not appear to be clustered geographically.[17]

While noncompete 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 changed significantly between the ends of the time horizon. The only reversal occurred in Louisiana, which had an initial reversal in

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[15]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.

[16]There are two reasons why there might be differences between the 1991 and 2009 scores: (1) New cases or statutes caused changes in state laws; (2) 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.

[17]See the map in Appendix D.1.

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 further 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 Lubotsky-Wittenberg method.

3 Relevant Training and Noncompete Enforcement 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 (Autor, 2001) and workers do not take commensurate wage cuts (Barron et al., 1999; Loewenstein and Spletzer, 1999, 1998) to pay for it.[18] Acemoglu and Pischke (1999) show that wage compression, when increases in training cause larger increases in productivity than in wages, incentivizes firms to invest in general on-the-job training. They demonstrate that many plausible market failures including general and specific complementarities in production, minimum wage laws, adverse selection, and search frictions generate wage compression and thus encourage firms to provide training. Work studying why firms pay for training have focused on specific market failures which lead to monopsony power for the firm, such as technological complementarities (Acemoglu, 1998), minimum wages (Acemoglu and Pischke, 2003), the "thinness" of labor markets (Wolter et al., 2013), asymmetric information (Autor, 2001; Stevens, 1994), search frictions (Moen and Rosén, 2004), and moving costs (Katz and Ziderman, 1990; Benson, 2013).[19]

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 (Acemoglu and Pischke, 1999). It is widely argued that 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). As a result, the enforcement of noncompetes may have the bizarre effect of creating efficient investment incentives while further distorting employee mobility choices.

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[18]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.

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

To my knowledge there has been no theory examining the impacts of noncompete *enforcement* on training, though the relationship between firm-sponsored training and noncompetes was first noted by Rubin and Shedd (1981). Rubin and Shedd argue that while noncompetes 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 sharing sensitive, confidential information. In this situation, the firm would want the worker to sign a noncompete agreement to prevent the worker from appropriating the value of the training for which he did not pay. 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. With this logic, Rubin and Shedd recommend the enforcement of noncompetes that are reasonable in scope.

The theory developed in Section 4 is most similar to Posner et al. (2004), who also present a model of noncompetes that explores the tension between human capital investment and employee mobility.[20] They consider three contract breach remedies: 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, noncompete enforcement represents a hybrid between specific enforcement for movements within the scope of the noncompete and zero liquidated damages for movement outside its scope. Their suggestion to courts is that noncompetes 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 noncompete and alternative breach remedies, their model makes two assumptions which are crucial for their results: (1) They assume that investments in the worker are most productive in his initial firm; (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 higher noncompete enforcement increases firm-sponsored investment. The model presented in Section 4 considers the role of these important assumptions.

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[20]Meccheri (2009) studies a related model focusing on the provision of general and firm-specific training, but does not consider any mobility issues. See Leuven (2005) for a survey of classic private sector training models.

4 A Theory of Noncompete Enforcement, Training, and Mobility

4.1 Model Setup

The theory developed in this section formalizes the tension between noncompete enforcement, general human capital investment and worker mobility, seeking both to understand the assumptions underlying a relationship between noncompete enforcement and firm-sponsored general skills training and to characterize the optimal noncompete enforcement level. While reported skills are typically general (Loewenstein and Spletzer, 1999), I consider skills general in the sense that they are transferable across firms in some form, whether it is within industries (Neal, 1995; Parent, 2000), occupations (Kambourov and Manovskii, 2009), or tasks (Gibbons and Waldman, 2004; Gathmann and Schönberg, 2010). The theory is based on the full-competition and constrained regimes laid out in Acemoglu and Pischke (1999), which I refer to as 'contractible' and 'not-contractible' regimes, respectively. These two training regimes contrast the incentives of firms and workers to invest in general skills training. In the full-competition, or 'contractible,' regime, identical firms compete to hire a 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 constrained, or 'not-contractible,' regime, workers are not allowed to contribute to training and firms unilaterally choose training to maximize profits, competing for workers on training period wages only.[21] In either regime, the worker's second period wage is determined in a Nash bargain with worker bargaining weight β, based on the worker's expected outside option from quitting, $\mathbf{E}[v(T)]$.

The worker's output in the hiring and training stage is normalized to zero, while the worker produces $y(T)$ in the second period if the worker stays with the firm. The cost of training, $c(T)$, is paid in the first period. The cost and production functions satisfy the standard conditions.[22]

In the poaching stage, the now trained worker meets another firm.[23] 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 $[0, \bar{a}]$.[24] If the worker decides to stay, then he earns $w(T)$ and produces $y(T)$ at the initial firm.

Before the worker is hired, the firm will decide whether to offer the worker a noncompete and the worker will decide whether or not to sign it. If the worker signs it and eventually joins the poaching firm,

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[21]This assumption is validated by Loewenstein and Spletzer (1998) who find that employers often pay for training without commesurate wage cuts. Futhermore, 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.

[22]$y(0) = 0$, $y'(T) > 0$, $y'(0) = \infty$, $y''(T) < 0$ and $c'(T) > 0$ if $T > 0$, $c'(0) = 0$, and $c''(T) > 0$

[23]Or equivalently, mulls starting his own business

[24]The assumption that the worker earns his full marginal product simplifies the math but is not necessary for the results. Nash bargaining will yield the same results.

then the worker's noncompete is enforced with probability $\lambda \in [0, 1]$.[25] The worker's expected wage from quitting is $(1 - \lambda)ay(T)$, where the worker is assumed to earn nothing if his noncompete is enforced.[26] In this specification, not signing a noncompete can be thought of as $\lambda = 0$: that is, not signing a noncompete is equivalent to having signed a noncompete which is entirely unenforceable.[27]

4.2   Solving the Model

I solve the model via backwards induction, starting with the worker's quit decision. In the poaching phase, the worker meets a new firm at which he will have a wage equal to his productivity, $ay(T)$. The worker quits if his expected pay at the competitor firm exceeds his bargained wage at the incumbent firm:

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

The worker's post-training wages, $w(T)$, are determined in a Nash bargain after the worker is hired but before he meets a firm in the poaching stage. At this point, the worker has an expectation of the value of his training in the new firm, $\mathbf{E}[a]y(T)$. 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 gives him his outside option plus his share, β, of the surplus: $w(T) = \mathbf{E}[v(T)] + \beta(y(T) - \mathbf{E}[v(T)])$, which simplifies to:

$$w(T) = y(T)\left[\beta + (1 - \beta)(1 - \lambda)\mathbf{E}[a]\right] \tag{2}$$

Condition (2) shows that noncompete 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:

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

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[25]By focusing on enforcement, I am abstracting from the firm's choice to sue the worker over the noncompete. This choice is one of convenience. However, it does not matter if the firm actually sues the worker, because it is the threat of enforcement (known from prior cases or statutes) that determines quit and training decisions.

[26]There are two ways to think about noncompete enforcement in this context: (1) as mentioned above, λ is the probability that the worker's noncompete 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.

[27]In practice, this may not be true if individuals who sign noncompetes in very low enforcing states, such as California, perceive them to be enforceable. I consider individual perceptions of enforcement in Section 4.6

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.[28]

Substituting the wage from (2) back into the quit equation from (1) gives the quit decision as a function of exogenous variables:

$$a > \hat{a}(\lambda) \equiv \frac{\beta}{1-\lambda} + (1-\beta)\mathbf{E}[a] \tag{3}$$

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

$$P(a > \hat{a}(\lambda)) = 1 - G(\hat{a}(\lambda)) \tag{4}$$

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

4.2.1 Case 1: Training is Contractible

In this case, firms compete for the worker by offering wage contracts of the form $\{W, T\}$ and ask the worker to sign a noncompete. 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:

$$\max_{T,W} \quad U(W,T) \quad = \quad W + G(\hat{a})w(T) + (1 - G(\hat{a}))(1 - \lambda)\mathbf{E}[a|a > \hat{a}]y(T)$$

$$s.t. \quad W \quad = \quad G(\hat{a})(y(T) - w(T)) - c(T)$$

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

$$\max_T U(T) = G(\hat{a})y(T) + (1 - G(\hat{a}))(1 - \lambda)\mathbf{E}[a|a > \hat{a}]y(T) - c(T) \tag{5}$$

The firm's indifference between zero expected profit wage-training contracts turns the worker's optimal contract choice problem into a problem of joint surplus maximization.[29] 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

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[28] 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: $\mathbf{E}[a] \leq \frac{1}{1-\lambda}$.

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

objective function by 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 (5) yields the first order condition for the optimal training level $T_c^*(\lambda)$ of the contract selected by the worker:

$$y'(T_c^*(\lambda))\left(G(\hat{a}) + (1 - \lambda) \int_{\hat{a}}^{\bar{a}} ag(a)da\right) = c'(T_c^*(\lambda)) \tag{6}$$

Whether increases in noncompete enforcement induce more training is unclear. Totally differentiating (6) with respect to noncompete enforcement and using Leibniz' rule gives:

$$\frac{\partial T_c^*(\lambda)}{\partial \lambda} = \frac{y'(T_c^*(\lambda))\left(g(\hat{a})\frac{\beta}{(1-\lambda)^2}(1 - (1-\lambda)\hat{a}) - \int_{\hat{a}}^{\bar{a}} ag(a)da\right)}{c''(T_c^*(\lambda)) - y''(T_c^*(\lambda))\left(G(\hat{a}) + (1 - \lambda) \int_{\hat{a}}^{\bar{a}} ag(a)da\right)} \tag{7}$$

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. To understand the numerator, note that both the first and second terms are both unambiguously positive and therefore that the sign of the derivative relies on whether the first or second term is greater. The first term reflects the marginal benefit of the worker if he stays, while the second term, $\int_{\hat{a}}^{\bar{a}} ag(a)da$, reflects the expected marginal productivity of the worker given that he quits. If the second term is larger, then training falls when noncompete enforcement rises. 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 increased potential enforcement of his noncompete he instead chooses a contract with less training and a wage increase.[30] Put differently, when the likelihood of an *efficient* quit is high, then increasing noncompete enforcement prevents the worker from moving, which reduces the benefit from investing in training. On the other hand, if the probability of an inefficient quit is high, increasing noncompete enforcement increases the probability that the worker stays at the current firm which thereby raises the value of training.

On aggregate, the impact of noncompete enforcement on training depends upon the composition of the pool of noncompete signers. If the pool is made up of primarily well-matched individuals with a high probability of an inefficien quit, then the impact will be high. However, if the pool is made up of poorly

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[30]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 noncompete enforcement may delay and possibly prevent the move to the more productive firm in the first place.

matched individuals, then the impact may be negative. We show below that under the contractible model, only workers who have a high valuation of mobility will turn down noncompetes, suggesting that the pool of noncompete signers will be dominated by well-matched individuals.

Under the assumption that younger workers are less well matched (Topel and Ward, 1992) to firms, the top of their relative marginal productivity distribution is higher than those of more experienced workers: $\bar{a}_{young} > \bar{a}_{old}$. From the numerator in (7), it is obvious that a higher $\bar{a}$ increases the chance of an efficient quit. As a result, for less experienced workers, increases in noncompete enforcement are likely to lead to decreases in training. I will employ this fact in the empirical section to test whether or not there is evidence for the contractible model of training.

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 noncompete enforcement level, and (2) noncompete 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:

$$W_c^*(\lambda) = G(\hat{a}(\lambda))y(T_c^*(\lambda))(1 - \beta)\Big(1 - (1 - \lambda)\mathbf{E}[a]\Big) - c(T_c^*(\lambda)) \tag{8}$$

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 noncompetes are not enforced, $\lambda = 0$. If there is perfect noncompete enforcement, $\lambda = 1$, then the worker is paid $W_c^* = y(T_c^*(1))(1 - \beta) - c(T_c^*(1))$.

From (8), there are three effects of increased noncompete 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 noncompete 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 noncompete enforcement reduce the optimal training level, then the firm will pay less for the training if the third effect dominates the other two.

To see a simple example in which more enforcement reduces the willingness of the firm to pay for

training, suppose 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 (7) that an increase in noncompete 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 noncompete enforcement is to reduce the amount of training chosen, which reduces the firm's contribution to training.

Credit constraints or minimum wage laws may hamper the worker's ability to pay for training. In this case, increases in noncompete enforcement increase the firm's willingness to pay for training as long as the increase in noncompete 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 noncompete enforcement increase the firm's second period monopsony power, leading it to pay more for training.

The Choice to Offer and Sign a Noncompete Under the Contractible Model

Since the firm earns expected zero profits either way, the firm is indifferent between offering a noncompete and not. If the worker is fully aware of the noncompete, he will accept it if

$$U(W_c^*(\lambda), T_c^*(\lambda)) > U(W_c^*(0), T_c^*(0))$$

Since the worker's utility in the contractible regime represents expected total surplus as shown in (5), a sufficient condition for choosing the noncompete is that increases in noncompete enforcement increase expected total surplus for enforcement in the range of $[0, \lambda]$. As shown by Proposition 5 in Section B.2.1, total surplus is maximized by $\lambda_c^* \geq 0$. Therefore, if $\lambda \leq \lambda_c^*$ then the worker will choose the noncompete. If $\lambda > \lambda_c^*$ then the worker will choose the noncompete if the difference between λ and λ_c^* is not too great. Intuitively, noncompetes increase total surplus when the worker has a high likelihood of leaving for a firm in which he is less productive but makes more money. In this situation, the noncompete increases the probability the worker stays in his most productive firm. If an efficient quit is sufficiently likely then the worker will choose to sign a noncompete only if the enforcement level is not too high.

This model provides support for the idea that there is selection into who signs noncompetes. Under this model, the population of workers who sign noncompetes is composed primarily of people who care little about their mobility because they believe that there are few other firms at which they would be more productive. As a result, the pool of noncompete signers is dominated by well-matched individuals.

Although noncompete enforcement has opposing impacts on workers depending on how well matched they are, the aggregate effect will be driven by those who are well matched since they make up the bulk of the noncompete signing population. In other words, the negative impact of enforcement on the poorly matched workers who still sign noncompetes will be dominated by the positive impact of enforcement on the well matched workers.

4.2.2 Case 2: Training is Not Contractible

This model examines the case in which the worker is not allowed to contribute to training and the firm unilaterally chooses training for its worker, competing for the worker only on first period wages.[31] Because an untrained worker's marginal product is assumed to be zero, competition induces first period wages of $W = 0$.[32]

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 (2) and solving for the optimal training level, $T^*_{nc}(\lambda)$, gives:

$$G(\hat{a})y'(T^*_{nc}(\lambda))(1 - \beta)(1 - (1 - \lambda)\mathbf{E}[a]) = c'(T^*_{nc}(\lambda)) \tag{9}$$

Using the implicit function theorem, the partial derivative of the optimal training choice with respect to noncompete 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, noncompete 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 training is contractible at the hiring stage, $T^*_c(\lambda) > T^*_{nc}(\lambda)$.*

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[31]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.

[32]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.

The proof is in Appendix section B.1, 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.

The Choice to Offer a Noncompete

Unlike the contractible case, in this case the firm has an incentive to offer a noncompete while the worker is indifferent. The indifference of the worker arises because of the assumption of competition on first period wages alone. The firm will offer a noncompete if it's profits are greater than from not offering it:

$$E[\pi(T_{nc}^*(\lambda), \lambda)] > E[\pi(T_{nc}^*(0), 0)]$$

Equilibrium profits are given by

$$E[\pi(T_{nc}^*(\lambda), \lambda)] = G(\hat{a}(\lambda))(1 - \beta)y(T_{nc}^*(\lambda))\left[1 - (1 - \lambda)\mathbf{E}[a]\right] - c(T_{nc}^*(\lambda))$$

Since increases in noncompete enforcement reduce the chance of a quit, reduce the wage the firm has to pay the worker, and increase training, increases in enforcement will lead to more profits unless the additional cost from increased training is overwhelmingly large. As a result, firms will always choose to use noncompetes.

4.3   Timing of the Noncompete and Blending the Models

The contractible model and not-contractible model make assumptions that the worker knows about the noncompete ahead of time and can properly consider it. Marx (2011) finds that most engineers who sign noncompetes do not know about them at the time of the offer. Indeed, the typical vignette is of a worker who accepts an offer without knowing about the noncompete in advance, then signs the noncompete on the first day while working through a pile of paper work. Incorporating this strategic timing into the contractible model, it is straightforward to show that noncompete 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 $T_{nc}^*(\lambda) \leq T_c^*(0)$ the firm will leave the training level at $T_c^*(0)$. Thus noncompete enforcement in this scenario can only have a non-

negative impact on training. This blend of the contractible and not-contractible training indeed may be representative of the training received by the typical worker.

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 noncompete 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 noncompete 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 noncompetes, would train by maximizing expected social surplus, and make the worker quit whenever he meets a more productive firm.

Consider next the choice of noncompete policy faced by state legislatures who take as given the mobility choices workers make and the training choices firms make. In this theoretical setup, there are two potential benefits to enforcing noncompetes: (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 noncompete enforcement is that it might prevent workers from moving to firms in which they are more productive. Optimal noncompete enforcement levels balance these costs and benefits. When training is not contractible in the hiring stage, enforcing noncompetes 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 noncompete enforcement is to prevent workers from quitting for less productive firms. This leads to a lower optimal level of noncompete enforcement relative to the not-contractible case.

As long as optimal noncompete 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.

4.5   The Role of Confidential Information

The primary rationale for enforcing noncompete 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 the incentive to invest. Failing to enforce noncompetes 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,[33] 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. These assumptions are incorporated explicitly into the model in Section A of the appendix.

Consider how allowing a worker to provide valuable information about his initial firm to a competitor firm will affect the constrained social planner's choices, the optimal noncompete enforcement 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 noncompetes or training workers differently, and therefore none of the constrained efficient choices would be changed.

In the contractible regime, since competition over contractible training internalizes the positive training and information externalities, the only effect of noncompete enforcement is to decrease the chance the worker will make an inefficient quit. Therefore, the resulting optimal noncompete enforcement level will increase with the amount of confidential information the worker is transporting.

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 the value of the worker's information 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 noncompete enforcement level (as long as $\lambda < 1$). The second effect

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[33]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.

also reduces the benefit to training because the worker is more likely to quit. In this scenario, the state legislature should choose an optimal enforcement level that is much higher, and increasing in the extent of confidential information, 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 noncompete enforcement increases when training is contractible, but increases more when training is not contractible, and (b) the chosen training level will be higher in the contractible case.*

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 know little about noncompete enforcement (Starr et al., 2015) but that firms are likely to be keen to remind them of their noncompete 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 noncompete 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 noncompete 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 noncompete 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 noncompetes 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 noncompete enforcement, $\frac{\partial T_c^*}{\partial \lambda_a} = 0$ and $\frac{\partial T_{nc}^*}{\partial \lambda_a} = 0$.*[34]

4.7 Theoretical Prescriptions for Courts and State Legislatures

While these models do not present a full welfare analysis of noncompetes, the takeaways relevant to courts are: (1) When training is contractible, increased enforcement of noncompete 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 noncompetes to be enforceable or adhere to them for some other reason, then actual enforcement policies are irrelevant. (5) 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 noncompete would encourage workers to bargain for training that they would not have otherwise requested, laws such as Oregon's which enforce only noncompetes for workers who are given two weeks notice may encourage the contractibility of training. Similarly, if workers are not negotiating over training, ex post consideration laws which require firms to provide additional benefits to workers in order for the noncompete to be enforceable serve to negotiate on behalf of the worker.

4.8 Which Model is the Operative Model of Training?

The theoretical models presented above make the same aggregate empirical prediction: increases in noncompete enforcement increase firm-sponsored training. In the not-contractible model, noncompete enforcement serves to reduce the chance of a quit, thereby reducing the positive externality to providing training and allowing firms to capture more of the returns. Under the contractible model, the pool of noncompete signers is dominated by individuals who are well matched, implying that on average increases in noncompete enforcement prevent inefficient quits, causing workers to select contracts with more training. Thus if we were to only look at the aggregate effect of noncompete enforcement on training, we would be unable to

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[34]The proof of this proposition is omitted.

discern which theoretical model is operative. In order to distinguish which model is operative, I consider two types of evidence: (1) direct evidence from survey data on the extent of contracted training, and (2) indirect evidence due to opposing predictions from the competing models.

Direct evidence on the assumptions of the two models comes from a recent large scale survey of 11,529 labor force participants (Starr et al., 2014a). In particular, the contractible model makes two assumptions: (1) firms compete over workers by offering wage-training contracts and (2) workers choose the best contract for them either by negotiating with their employer or by choosing a better contract at a different employer. For the first assumption, Starr et al. (2014a) show that only 20.4% of workers sign employment contracts in which the amount of training is specified, though 71.4% of employers promise some kind of training. Of course the 28.6% of employers who did not promise training may decide to provide some on their own accord. Of those who sign noncompetes, the proportions are slightly higher at 32.2% and 80.1% respectively. This evidence suggests that most training is not explicitly contracted upon, though it is possible that informal contracts are still negotiated.

The second assumption of the contractible model regards whether or not workers have choices over wage-training contracts and whether they negotiate over training. Starr et al. (2014a) show that 69.6% of workers have no alternative job opportunities when they were asked to sign their noncompete. For these workers, either they accept the contract or negotiate over it. For the workers who signed noncompetes with no alternative offers, only 9.6% report negotiating over training. For those who did have alternative job options when they were asked to sign a noncompete, only 21.7% negotiated over training. Although the propensity to negotiate is higher for those with alternative job offers, this evidence shows that most workers do not have alternative job options and do not bargain over training.

Workers may bargain over other aspects of the noncompete, however, such as work benefits or additional compensation. Table II, reproduced from Starr et al. (2014b) shows, however, that only 10.2% of those who sign noncompetes bargain over either the terms of the noncompete or in exchange for additional benefits. When asked why they didn't negotiate over their noncompete, 50.5% responded that they found the terms reasonable, 40.4% simply assumed that they could not bargain, and 22.9% said they were worried they would be fired if they attempted to negotiate. These numbers provide clear evidence that there are significant barriers to negotiation for many workers. Taken together, this direct evidence indicates that the assumptions underlying the contractible model are unlikely to hold for the majority of workers.

The lack of bargaining over noncompetes is surprising, given the Coasean perspective that has dominated the thinking in the legal field (Callahan, 1985; Posner et al., 2004). State noncompete policies may, to some extent, substitute for these individual negotiations. Specifically, noncompete enforcement policies which require additional consideration in the form of training, additional compensation, or other benefits

in order for the noncompete to be enforceable function to bargain on behalf of the worker. The difficulty with these policies, however, is defining what 'additional' means in court. Still, however, there is potential for these policies to fill in what might be considered a 'negotiation gap' due to high transaction costs or perceived potential tensions.

As indirect evidence, the theories above make opposing predictions for inexperienced workers. First, since the quality of the worker-firm match increases with labor force experience (Topel and Ward, 1992), less experienced workers are likely to be in relatively poor matches. As a result, for inexperienced workers the pool of poaching firms has a high proportion of better matches and thus they are more likely to make efficient quits. The contractible model predicts noncompete enforcement will reduce training for these workers, while the not-contractible model predicts the opposite.

A second test of the not-contractible model regards the impact of noncompete enforcement over tenure. By its very nature of being far off in the future, training 10 years after commencing employment is difficult to contract over. As a result, training observed later in tenure is unlikely to be contracted upon. This assumption is not new: Loewenstein and Spletzer (1997) show that firms do indeed provide more training later in tenure in order to avoid provide training to 'leavers.' Their analysis implicitly assumes that firms can unilaterally choose when to provide training. If training later in tenure is not contractible, then noncompete enforcement should have an unambiguously positive impact.

5   Empirical Analysis

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 primary benefit of the SIPP relative to other training data sets such as the Employment Opportunities Pilot Project, the Small Business Administration data (Barron et al., 1999), the NLSY, and the PSID is the that the number of respondents in each panel is about 40,000. This size difference is crucially important to the project because power issues demand a large enough number of workers who sign noncompetes. The SIPP is a longitudinal survey that interviews respondents once every four months for three to four years. Because noncompete 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 I can: 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. Overall, 18.7% respondents report receiving training paid for by the firm.

It is unclear whether a worker who reports receiving firm-sponsored training is referring to informal or formal training, and the SIPP does not make this distinction explicitly. The SIPP does, however, ask the location in which the training occurred and who provided the training. The tabulation shows that of those who report receiving firm-sponsored training, 46.43% report that it was on the job taught by somebody from the firm, 16.87% report that it was on the job taught by somebody outside the firm, 35.16% say it was away from the firm and 1.53% say 'other.' Given these numbers, it appears that up to half of the training could be informal (46.43%). However, given that only overall only 18.7% of the respondents report receiving training, informal training is likely underreported. As a result, if noncompete enforcement increases informal training, perhaps because it is not contractible by its nature, then my estimates will understate the actual effect of noncompete enforcement on *total* training.

In order to exploit the cross-sectional state level heterogeneity in noncompete enforcement, I compare training outcomes between occupations likely to see noncompete litigation (high litigation) and occupations unlikely to see such litigation (low litigation) using surveys of litigated, noncompete cases (LaVan, 2000; Whitmore, 1990). LaVan's 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.[35] 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 III. 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 noncompetes are unenforceable for lawyers (Stroud, 2001; Starr et al., 2014c).

An important feature of this identification strategy is that mistakes in the division of occupations into

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[35]It is unclear if this is a random sample.

low and high litigation groups biases my estimates towards zero. The logic is as follows: Suppose that managers are not actually affected by noncompete enforcement and should actually be put in the low litigation category. Then leaving managers in the high litigation category reduces the average impact of enforcement for the high litigation group, reducing the difference in impacts between the high and low litigation groups. Alternatively, if construction workers are affected by noncompete enforcement, then they should be in the high litigation group. But by leaving them in the low litigation group, this raises the average impact of enforcement for the low litigation group, thereby shrinking the difference with the high litigation group. This feature strongly suggests then that the DID estimates are likely to be underestimates.

Selection into low litigation can be determined by four possibilities: (1) Workers in these occupations do not actually sign noncompetes, thereby exempting them from potential litigation, (2) firms decide not to attempt to enforce noncompetes 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 noncompetes (Starr et al., 2014b) or firms choosing not to enforce because the occupation is a low value occupation. Summary statistics for key variables are presented in Table IV, and state, industry, and occupation distributions by high and low litigation status are shown in Figures III to V.

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 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. 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.

Figure III shows that the high litigation occupations are dominated by sales, production workers, and management, while the low litigation group is dominated by office support workers, transportation and materials moving workers, and construction workers. Figure IV shows that manufacturing and retail trade are the most represented industries and that most industries are fairly mixed in their employment of high litigation and low litigation. Figure V shows importantly that the within-state distribution of litigation types is balanced. This is important because the empirical strategy I employ relies on within state differences in training between high and low litigation occupations.

To get a sense of the unconditional relationship between noncompete enforcement and the within-state difference between firm-sponsored training received by high and low litigation occupation, Figure VI plots the average probability of receiving training in high and low litigation occupations within each state against noncompete enforcement intensity. The unconditional difference-in-differences estimate is the difference between the slopes. 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.

5.2 Identification

Due to the fact that noncompete 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 noncompete enforcement between occupations which appear frequently in noncompete 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 these workers may also sign noncompetes, but the likelihood of litigation is much lower for this group. Indeed, there is evidence that there is a positive relationship between training and noncompete 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:

$$T_{ijost} = \beta_0 + \beta_1 Enfc_{st} * HL_o + \beta_2 Enfc_{st} + \gamma X_{ijst} + \Omega_o + \theta_s + \phi_t + \epsilon_{ijost} \tag{10}$$

$$T_{ijost} = b_0 + \sum_{k=1}^{10} \alpha_k Enfc_{st} * Occ_{k,HL} + b_2 Enfc_{st} + \gamma X_{ijst} + \Omega_o + \theta_s + \phi_t + \nu_{ijost} \tag{11}$$

In equations (10) and (11), 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} is a set of individual, firm level, and interacted state-level controls, HL_0 is a dummy for high litigation occupations, and $Enfc_{st}$ is the noncompete 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 (10) and the ten α_k terms in equation (11). They capture an underestimate the causal, intention-to-treat effect of noncompete enforcement on high 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, 2012), 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.[36]

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 noncompete enforcement. In notation, the identifying assumption for equation (10) is

$$\mathbf{E}[Enfc_{st} \cdot HL_o \cdot \epsilon_{ijost} | X_{ijst}, \Omega_o, \theta_s, \phi_t] = 0 \tag{12}$$

The equivalent assumption for equation (11) follows a similar form. In the robustness section below, I show that the training effects are not driven by the weighting of the index, the linearity assumption, reverse causality, high training firms sorting to high enforcing states, and skill-related training being more likely in high enforcing states.

5.2.1 Intent to Treat vs. Treatment on the Treated

Unless noncompete enforcement is associated with other frictions in the labor market, noncompete enforcement only matters for workers who sign noncompete agreements. Unfortunately, whether a worker has signed a noncompete is not contained in the data. Therefore, the way to interpret a coefficient like β_1 from equation (10) 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 noncompetes. 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 noncompetes.

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[36]Notably, tenure and potential experience are both 'bad controls' in the sense that they are outcomes of noncompete enforcement as well. Specifications excluding these variables tend to slightly increase the effects.

6 Results

6.1 Baseline Results

The results from equation (10) are reported in column (4) of Table V.[37] The intent to treat effect in the full sample is 0.007. This implies that if a state were to increase its noncompete enforcement intensity by 1 standard deviation, high litigation 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.[38] The results are robust to using state-year fixed effects as well (estimates not shown).

The first column of Table VII shows the occupation-specific ITT estimates from equation (11). 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 worker-reported 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 affected by noncompete enforcement relative to low-litigation occupations.[39] Figures A2 to A3 in Section D.2 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 noncompete enforcement on training tends to be located in occupations that have higher earnings and higher schooling levels.

6.2 The Type and Content of Training

In this section I consider how noncompete enforcement affects the type of training (e.g., offsite, onsite) provided and the content of the training. The type of training is divided into four categories: (1) onsite taught by somebody from the firm, (2) onsight taught by somebody outside the firm, (3) offsite, and (4) other. Since the other category is so small (1.53% of those who receive training), I group them into the offsite bin. Summary statistics for the types of training are given in Table VI.

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[37]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.

[38]Tenure, potential experience, and firm size may be considered bad controls since greater noncompete 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.

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

Using a dummy for the type of training as the dependent variable in the same specification, I examine which type of training noncompete enforcement is likely to affect. The results are shown in Table VII. The results show that a one standard deviation increase in noncompete enforcement increases the probability of receiving offsite training by 0.5 percentage points. The increase for onsite training programs taught by outsiders is 0.2 percentage points, while the impact for coworker taught training onsite is estimated to be 0. Given the additional expense of offsite training and of bringing in outside teachers, these estimates suggest that noncompete enforcement provides valuable enough protection to merit a more costly investment in workers.

Since training could consist of many activities, I use the SIPP's follow up questions regarding the contents of the training to see whether noncompete enforcement encourages firms to pay for skill-upgrading training. Training content is categorized into the following non-mutually exclusive categories: basic skills, new skills, upgrade existing skills, and company policies. Summary statistics of these outcomes by high and low litigation status are given in Table VIII. 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.

To examine which type of training noncompete enforcement affects, I run the same regressions using indicators for content received as the dependent variable. The results in Table IX 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 1.1 percentage point impact on basic skills training is relatively large. With regards to the probability of learning new skills, noncompete enforcement appears to positively affect only business, financial, and computer and mathematical occupations.

6.3   Enforcement Impact by Potential Experience and Tenure

In this section I examine the impact of enforcement by the potential experience and tenure of the worker in order to distinguish between the two competing models of training. 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. With regards to potential experience, assuming that workers with little potential experience are more likely to make efficient quits than experienced workers, the contractible model predicts that noncompete enforcement will reduce

training for younger workers, while the not-contractible model predicts a positive impact of enforcement on training early on in a worker's career.

Figure VII shows the impact of noncompete enforcement by potential experience of the worker using the baseline specification. A one standard deviation increase in noncompete enforcement increases training for young workers with 0 to 10 years of potential experience by 1.7 percentage points (8%), for those with 10-16 years of potential experience by 0.8 percentage points (4%), and for those with 16-22 years of potential experience by 1.4 percentage points (7%). Beyond 22 years of potential experience, the point estimates fall markedly and become statistically indistinguishable from zero. These results provide clear evidence in favor of the not-contractible model. If the contractible model does hold some sway, then these estimates suggest the impact of the not-contractible model are underestimates.

Furthermore, under the assumption that training later in tenure is unlikely to be contracted upon, the impact of noncompete enforcement on training later in tenure should be unambiguously positive. To perform this test, I run the baseline specification for different bins of tenure levels. The results, displayed in Figure VIII,[40] show that the impact of noncompete enforcement on training rises between years 0-5 and years 15-20, before falling off. Because the impact of noncompete enforcement on training is stronger for workers later in tenure, these results strongly support the not-contractible model.[41]

These results across tenure are somewhat unexpected. In light of Loewenstein and Spletzer (1997), we might expect that most of the delayed formal training is being replaced by training up front since the firm does not have to worry about the worker leaving because of the enforceable noncompete. But this would suggest that the impact of enforcement on training would be higher in the first few years of tenure before falling off, which is the opposite of what the results show. One way to reconcile this finding is that it may be that the employee's noncompete becomes more important over time as the employee collects 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 else equal. Thus the employee's noncompete becomes a more important feature of the employee-firm relationship later in tenure.

In combination with direct evidence from Starr et al. (2014a), this indirect evidence on the impact of enforcement on training by the potential experience and tenure of the worker provides compelling support for the not-contractible model. Under this model, there is a role for noncompete enforcement in reducing

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[40]The corresponding numbers are shown in Table A2 in the appendix.

[41]While these tenure results may be subject to bias since tenure may be a bad control since those in higher enforcing states may be likely to 'stay' in their jobs longer, as shown in Table XI, high litigation workers and low litigation workers show no differential enforcement effects on tenure.

the positive externality of training and thus increasing incentives for firms to invest.

6.4   Consideration and the Role of Specific Dimensions of Enforcement

The enforcement index generated from factor analysis is useful because it provides relatively objective weights for the seven underlying dimensions of noncompete enforcement intensity, but it is less useful to courts and state legislatures that want to know to which dimension of enforcement training responds the most. In order to provide direct policy relevance, in this section I break up the enforcement index into its separate components to see which components of the index cause the increase in firm-sponsored training. The results appear in Table X.

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 variables.

Including each of the variables linearly, the results in column (8) show two interesting points.[42] First, the impact of easing the plaintiff's burden of proof increases training, as it did in the univariate specification. The second, more notable point regards the negative point estimate on consideration post inception. To understand what that variable represents, note that it is a state-level score from 0-10 (Bishara, 2011) on the following two questions from Malsberger et al. (2012):

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

High scores reflect the enforcement of a noncompete when only continued employment is provided, while lower scores reflect state policies that require firms to provide some other type of consideration, such as training, increased wages, or other benefits, in order for the noncompete to be enforceable. Thus the empirical results suggest that for workers who signed noncompetes after the employment relationship has begun, holding other dimensions of enforcement constant, state policies that enforce noncompetes only when these workers are provided additional benefits receive more training.

While the bulk of the evidence so far has favored the not-contractible model, these results suggest that there is a role for the contractible model as well. To see this, consider that most workers do not negotiate

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[42]Note that 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.

over their noncompetes. Without such negotiating, training is likely chosen unilaterally by the firm, resulting in training levels which are lower than if the worker had negotiated properly over the contract (Proposition 1). State policies that require additional consideration for the enforcement of noncompetes interfere with the firm's training choice by 'bargaining' on behalf of the worker. As a result, these policies improve training outcomes for workers while simultaneously reducing the likelihood of enforcement.

6.5   A Mechanism and Other Enforcement Predictions

Given the training effects documented above, I test for one mechanism and two predictions from the model. First, it could be that training is not the primary margin on which noncompete 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. Alternatively, in higher enforcing states it may be harder to hire experienced workers if they are bound by an enforceable noncompete. In order to explain the observed training effects there must be a negative relationship between noncompete enforcement and the starting experience of the worker. Instead of actual starting experience, I use starting potential experience as the dependent variable with the same estimation strategy from (10) and (11).[43]

Second, the model shows that higher noncompete 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.[44]

Third, the model shows that higher noncompete enforcement leads to wage compression, which encourages more training. Directly estimating whether noncompete enforcement 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 noncompete enforcement on wage compression. Despite these limitations, however, I can still test for evidence of post-training wage effects. Recall equation (2). If noncompete 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,

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[43] 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 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.

[44] 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.

and (2) they also receive the wage boost from the extra training they receive.[45] If noncompete enforcement causes lower wages, then the effect of noncompete enforcement through wage compression dominates. I examine this by regressing log hourly wages on noncompete enforcement using the same identification strategy as above.[46]

The results of these regressions are shown in the second through fourth columns of Table XI. The results for high litigation occupations are generally in the expected direction but only the starting potential experience margin reaches canonical levels of statistical significance for the high litigation group as a whole. 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 for tenure are mixed in their direction, but none of them reach statistical significance. This finding merits further consideration, however. In the training results, I argue above that the appropriate model for training is the non-contractible model in which the firm chooses the training. Thus the empirical strategy exploits the fact that firms train differentially based on the enforcement policy of their state and if workers are in occupations that have been litigated before. In this case, the quit decision is solely the worker's. If low litigation and high litigation workers perceive themselves to be equally affected by noncompete enforcement, then they will make the same quit decision and thus the difference-in-differences estimate will be close to zero, as observed in Table XI. Pooling the impact of enforcement on both high and low litigation workers (ie, not using state fixed effects and not having a differential effect for high litigation workers), shows that a standard deviation increase in enforcement increases tenure by 0.07 years (p-value 0.047). This estimate is purely cross-sectional, but taken at face value it implies that if California adopted Florida's laws then the average worker tenure would increase by about a 4 months.

The effects of noncompete enforcement on wages show that computer and mathematical occupations 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 noncompete enforcement on wages.

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[45]They also might receive additional compensation for signing an enforceable noncompete 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 noncompete (Marx 2011), this effect is expected to be small. Regressions, not shown here, confirm this is true.

[46]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.

6.6   Robustness

6.6.1   Threats to Identification

There are at least four threats to identification: (1) Reverse causality, (2) unobserved confounding treatments may make skill-related training more likely in higher enforcing states, (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, and (4) the enforcement impact may simply be picking up an increase in the incidence of noncompetes. 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 noncompetes 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 XIII. 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 noncompete 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[47] which are presumably less likely to be impacted by noncompete enforcement, to for-profit low litigation occupations (the same control group). Evidence from Starr et al. (2014b) shows that indeed noncompetes are much more likely in the private for-profit sector (13%) than the private not-for-profit sector (5%). The for-profit versus non-profit results are presented in column (1) of Table XII. Additionally, because lawyers are a high skill occupation which are unaffected by noncompetes per the American Bar Association's Model Rule 5.6 (Stroud, 2001; Starr et al., 2014c), 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 XII. 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

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[47]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. I also include all lawyers in the high litigation group here because they are a high skill group which are unaffected by noncompetes.

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.[48] 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 XII 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.

The last concern regards the interpretation of the main results. Because the data does not contain information on who signs noncompetes, the impact of enforcement could be driven by two margins. First, Starr et al. (2014b) show that the incidence of noncompetes rises with enforcement such that Florida has a 5 percentage point higher incidence than California. As a result, if noncompetes themselves are associated with an increase in training in the absence of enforcement, then the enforcement impact identified here may simply correspond to the increased incidence of noncompetes. Alternatively, it could be that those who sign noncompetes actually receive more training in higher enforcing states. Without data on who has signed a noncompete, there is no way to distinguish between these two interpretations.

6.6.2 Pretrends, Variations in the Enforcement Index, and Excluding Extreme States

In this section I examine to what extent the results are driven by the linear empirical specification, the particular enforcement index used, and outliers in the sample. I begin by considering the linearity assumption and the existence of 'pretrends'. Instead of assuming the effect of noncompete enforcement on training is linear, I allow the impact of enforcement to vary by the number of standard deviations away from the mean, which was normalized to zero. The estimates are shown in Figure IX. The estimates show that the linear assumption is a relatively good assumption. The impact by standard deviation is increasing and convex, but not too convex. As a result of the slight convexity, the linear approximation underestimates

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[48]Appendix D.4 shows the tradable versus non-tradable breakdown by industry.

the impact for the highest enforcement states and overestimates the impact for the lower enforcement states. Overall, however, the differences are minimal and as such provide strong support that the linearity assumption is a good one.

Additionally, Figure IX can inform us about 'pretrends'. Pretrends are normally thought of in a dynamic sense, whereupon differences in pretrends before treatment occurs can be explicitly analyzed. This dynamic assessment translates roughly into this setting, the difference being that there is not exactly a point along the enforcement dimension whereupon enforcement suddenly takes effect. To check for pretrends in this case, I examine to what extent states that are closest to the non-enforcement margin (those with enforcement measures 3 standard deviations below the mean) differ from the states who are slightly more enforcing. This amounts to checking whether or not the difference in training between those 3 standard deviations below the mean and those 1 standard deviation or no standard deviations below the mean is large and significant. Figure IX shows that impact of enforcement relative to the least enforcing states is neither large nor statistially significant for those 1 or 0 standard deviations below. Thus we can have some confidence that pretrends are not driving these results.

Next I test whether the results are robust to variations of the factor analysis enforcement index, using alternative enforcement indices and excluding states at the ends of the enforcement 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 (2009) 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 (2011) 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 XIII. They show the results are robust to whatever variation of the factor analysis weights I choose.

In *Panel B* of Table XIII, I consider how the baseline estimates change if instead I utilize the Bishara (2011) index from 1991 and 2009, the 1992 and 2001 indices developed by Garmaise (2009), or the method developed by Lubotsky and Wittenberg (2006) [49] The Lubotsky and Wittenberg (2006) method takes the linear regression of training on the seven individual enforcement dimensions 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,[50] and (2) the weights on the dimensions of enforcement are 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.[51] 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 (2) the arbitrary weights chosen by Garmaise may overemphasize the importance of various dimensions of noncompete 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.

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.[52] To address these concerns, I re-run the baseline specification without California, Florida, or both. The results are presented in *Panel C* of Table XIII. As expected from Figure VI, the results show that the common effect for high litigation occupations is robust to the exclusion of both California and Florida.

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[49]Garmaise (2009) considers various dimensions of noncompete enforcement for each state using the same Malsberger et al. (2012) text as Bishara (2011), though he assigns each dimension a binary score and simply adds them up. See Appendix C.2 for a complete description of the questions Garmaise (2009) considers.

[50]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 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.

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

[52]Excluding North Dakota does not affect the results because it includes only 79 observations, which constitutes 0.1% of the sample.

7 Conclusion and Policy Implications

The skills and proper allocation of workers are important for economic growth and productivity (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 at 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 noncompete enforcement on firm-sponsored training, I extend the classic on-the-job training models to incorporate noncompete enforcement and mobility. The models shows that the common assertion that higher noncompete enforcement increases firm-sponsored training outcomes relies upon the contractibility of training. When training is not contractible, noncompete enforcement serves to reduce the positive externality to training, incentivizing the firms to train more. When firms compete over workers by offering wage-training contracts, workers make the best choice for them, taking into account the potential for noncompetes to reduce their mobility should a better offer come along. In this model, individuals who are well matched are willing to sign a noncompete and also select contracts with more training in higher enforcing states. Those who are poorly matched are less likely to have signed noncompetes, but those that do choose contracts with less training in higher enforcing states because of the reduced value to their training. To distinguish whether the contractible training model or the not-contractible model is operative, I consider direct evidence examining the validity of the underlying assumptions, and test predictions of both models for inexperienced workers and for workers with longer tenures.

In order to empirically examine the impact of noncompete enforcement on training, I first improve upon the Bishara (2011) index of noncompete enforcement by generating weights to aggregate his scores using factor analysis. Comparing occupations likely to experience noncompete litigation to occupations less likely to experience such litigation, I find that a one standard deviation increase in the noncompete enforcement index increases training for high litigation occupations by 3% relative to low litigation occupations. This positive training effect is 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 affected, presumably because of their interactions with clients. As a result of these heterogeneous effects we suggest that laws which enforce noncompetes for certain affected types of workers, such as Colorado's law enforcing noncompetes only for upper-management employees, can exploit the training benefits without hurting workers in unaffected occupations.

Testing between the competing models, we find that the relative impact of noncompete enforcement on

training is increasing monotonically between 3% and 8% for each of the first 20 years of tenure, and is 7% for workers with less than 10 years of potential experience. These tenure and potential experience results coincide with the predictions of the not-contractible model. Since the not-contractible model is premised on a positive externality to training, these results suggest that there is a role for noncompete enforcement in improving training outcomes.

Disaggregating the enforcement index into its individual components indicates that there is still a role for the contractible model, however: courts or state legislatures requiring additional consideration for noncompetes signed after the inception of employment function to negotiate on behalf of the worker, simultaneously reducing the probability of enforcement and increasing training. Since workers are very unlikely to negotiate over their training (Starr et al., 2014a), these state level policies help to fill in the negotiating gap, though in a notably blunt way.

Additionally, the results show that the overall 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, perhaps because they are unwilling to bear their training costs. Alternatively, it could be that firms in high enforcing states have trouble hiring experienced workers because they are also bound by enforceable noncompetes.

These findings have important implications for both the debate over noncompete enforcement and the literature on firm-sponsored training. With regard to training, these results show that noncompete enforcement can be used as a tool to improve training outcomes, either by increasing enforcement via the unilateral firm choice training model or by requiring firms to provide consideration in exchange for enforcing noncompetes. The latter finding opens up new directions for future research on training: Why are individuals not negotiating for training? Is it efficient for the government to intervene in this way?

With regards to the enforcement debate, legal scholars have recently been advocating for lower enforcement (Hyde, 2003; Lobel, 2013) based on studies which focus on the negative impacts of noncompetes. But these positions are empirically premature because little is known about the ways in which firms and workers are benefitting from enforcement. While noncompetes have been shown to harm inventor, physician, and CEO mobility (Marx et al., 2009; Garmaise, 2009; Lavetti et al., 2014), and mitigate the impact of venture capital on startups (Samila and Sorenson, 2011), Lavetti et al. (2014) find that doctors who sign noncompetes earn more because they are entrusted with more clients. This paper finds that in higher enforcing states many occupations do receive more skill upgrading paid for by the firm, though it is also possible to reduce enforcement and improve training via consideration laws. These parameters are an important part of the welfare story, but they are not the whole story. Other parameters of interest which have yet to be estimated are the distortionary impact of noncompete enforcement on the allocation of

workers across firms and the necessity of these laws for innovation.

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Figures

Figure I: Proportion of Occupation Signing CNC
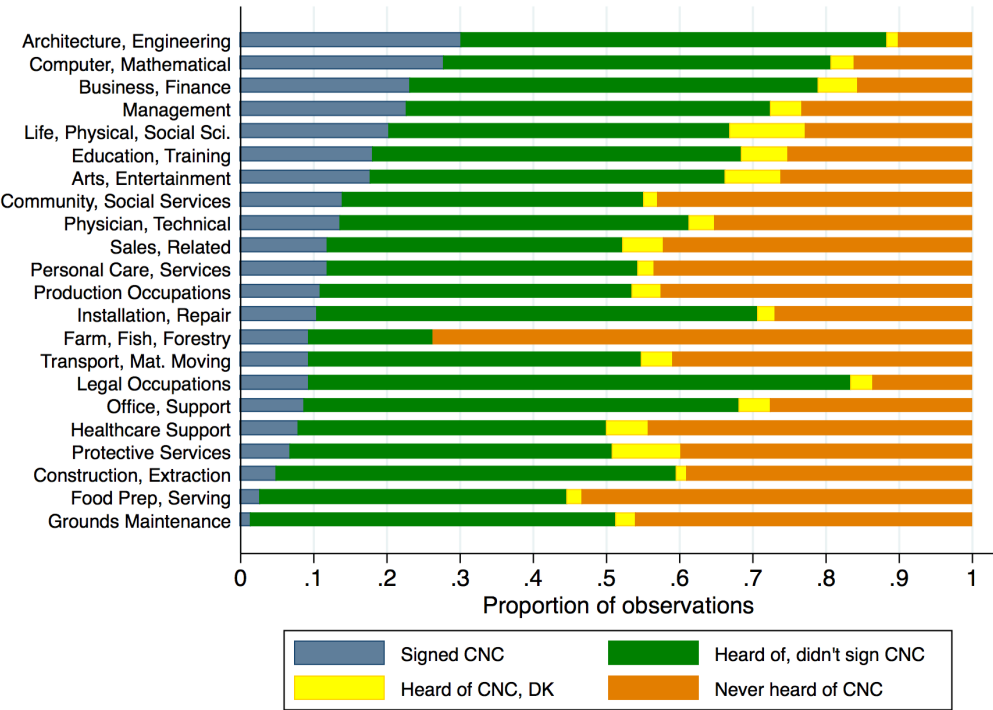
Note: Figure from Starr et al. (2014b). 'CNC' stands for covenant not to compete.'DK' refers to doesn't know if currently signed a noncompete.

Figure II: Factor Analysis Enforcement Index for 2009 and 1991
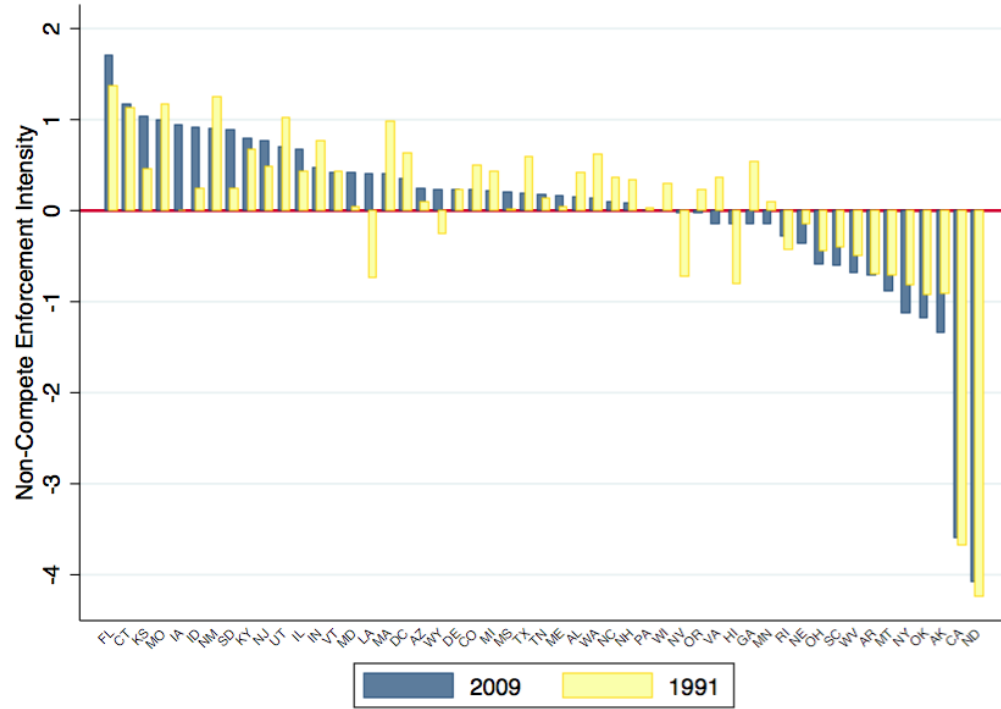
Figure III: Occupation Distribution by Litigation Type
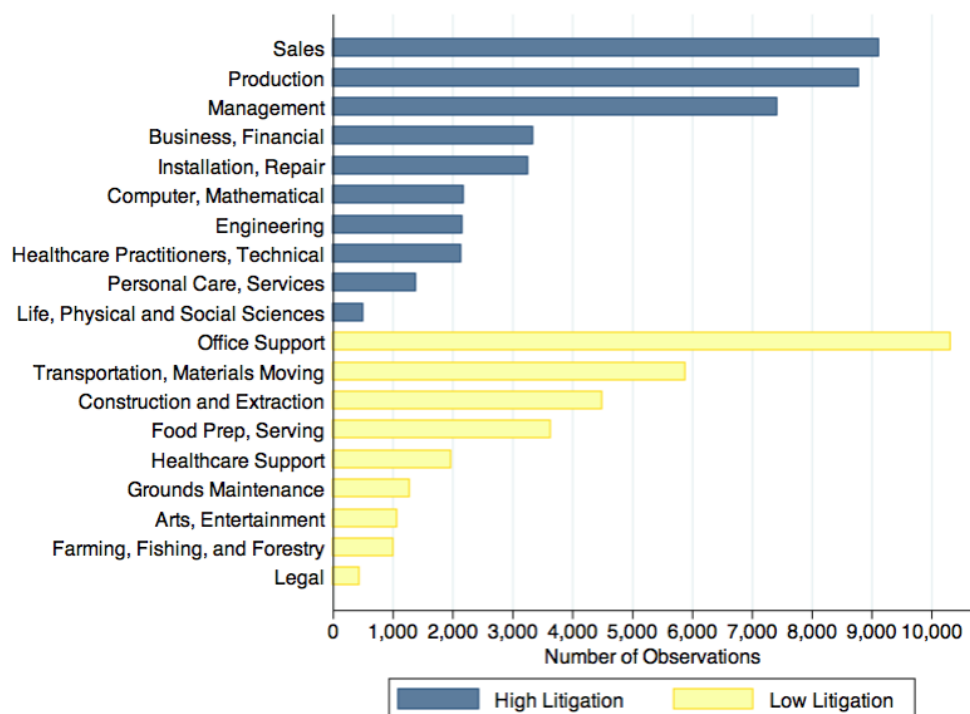
Figure IV: Industry Distribution by Litigation Type
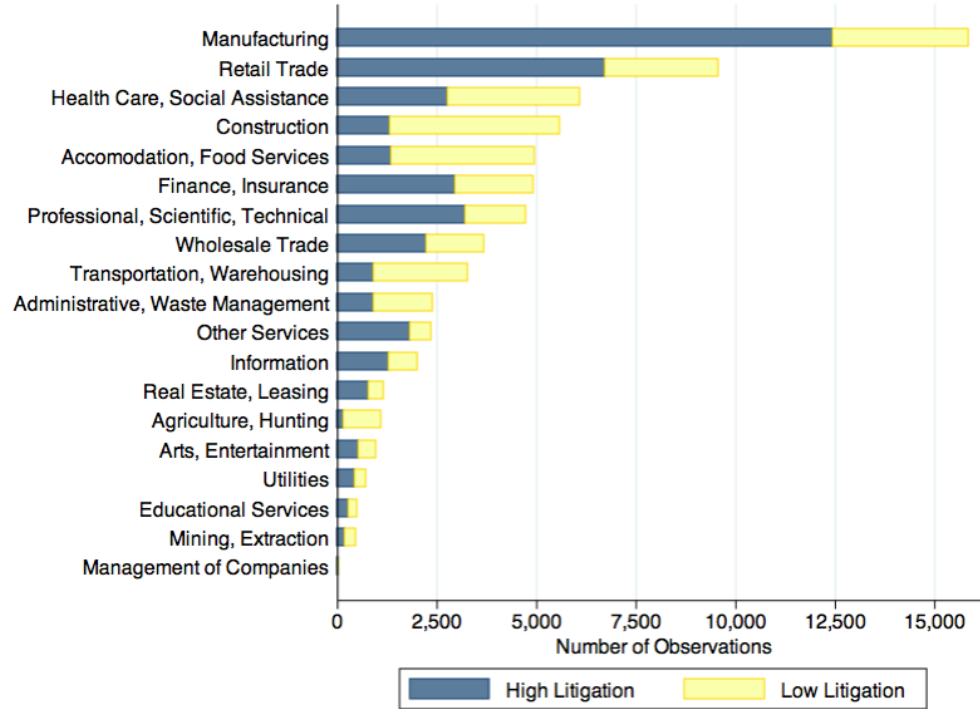
Figure V: State Distribution by Litigation Type
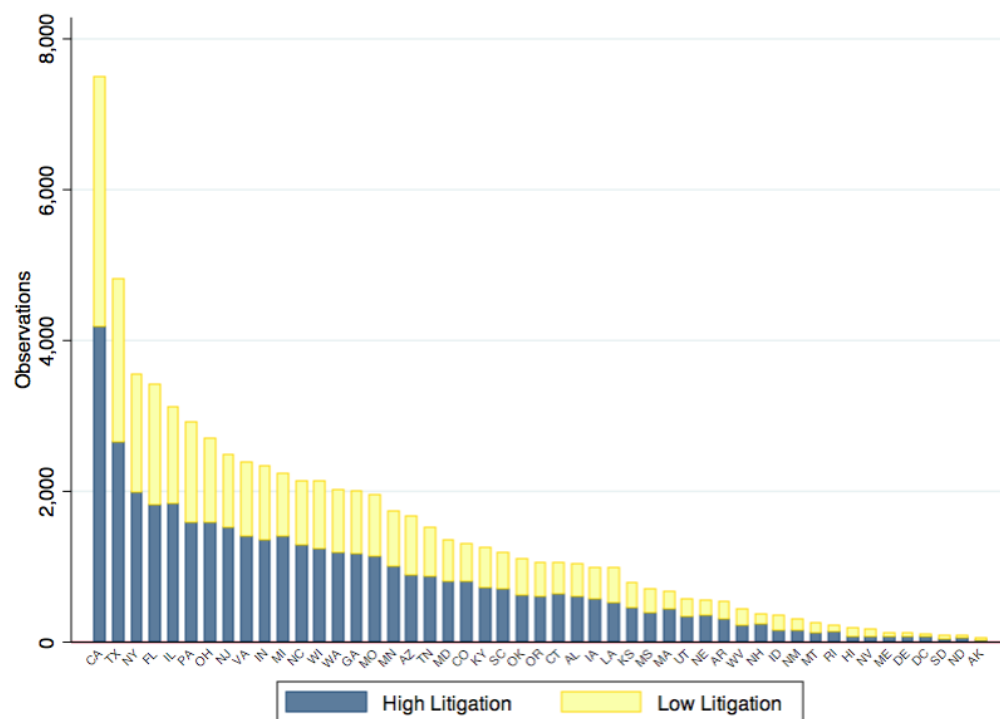
Figure VI: Within-State Training versus Noncompete Enforcement
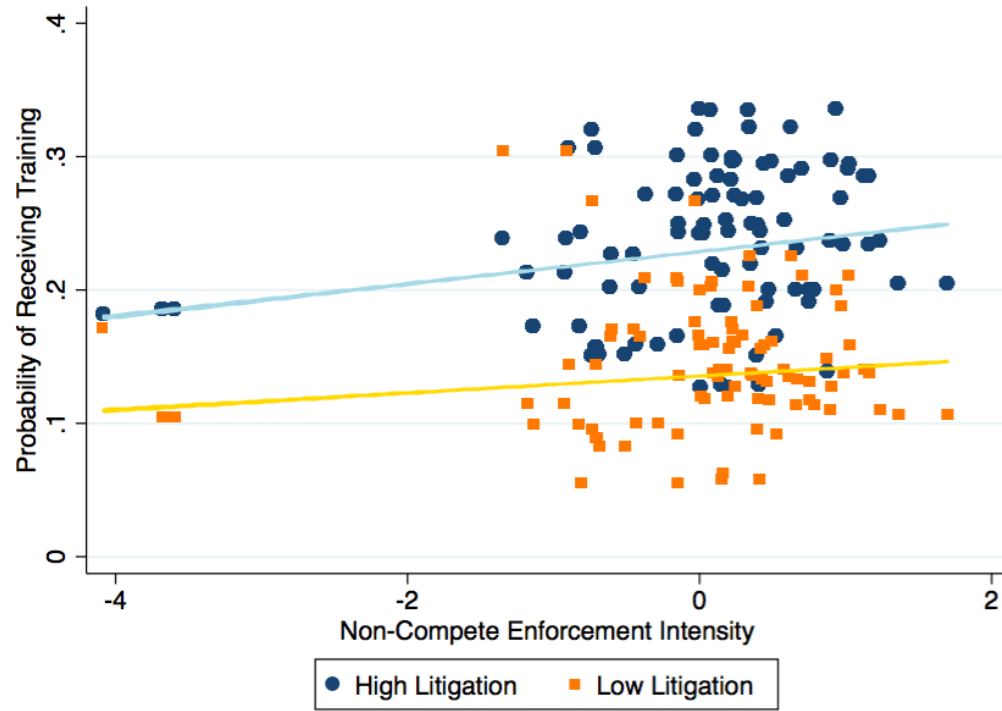
Figure VII: Marginal Effect of Noncompete Enforcement Across Potential Experience
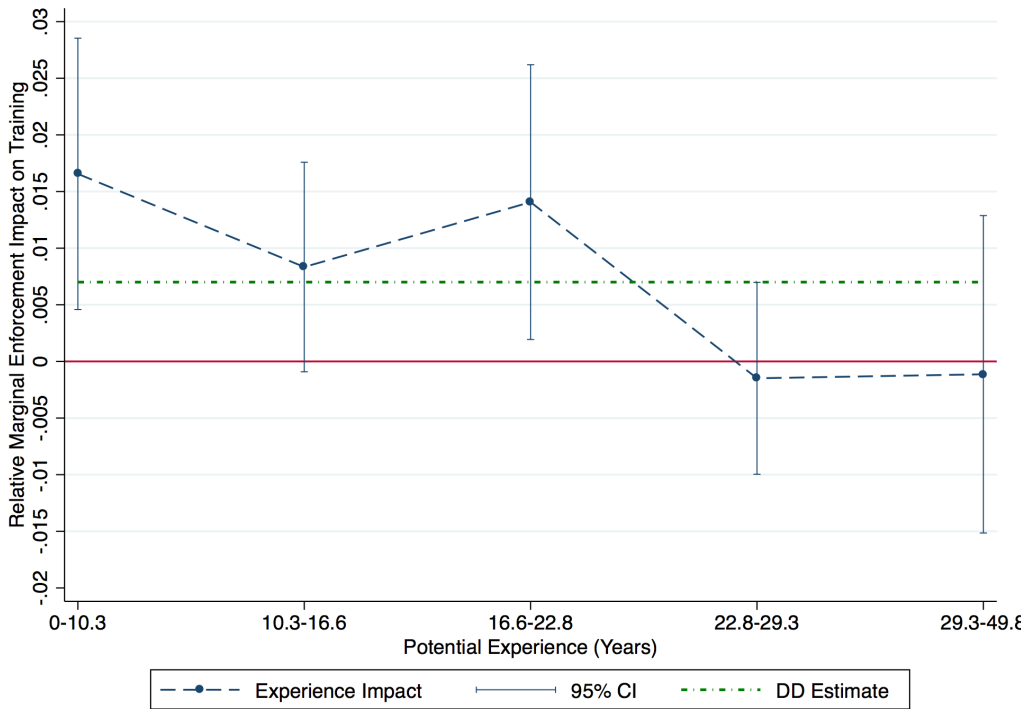
Figure VIII: Marginal Effect of Noncompete Enforcement Across Tenure
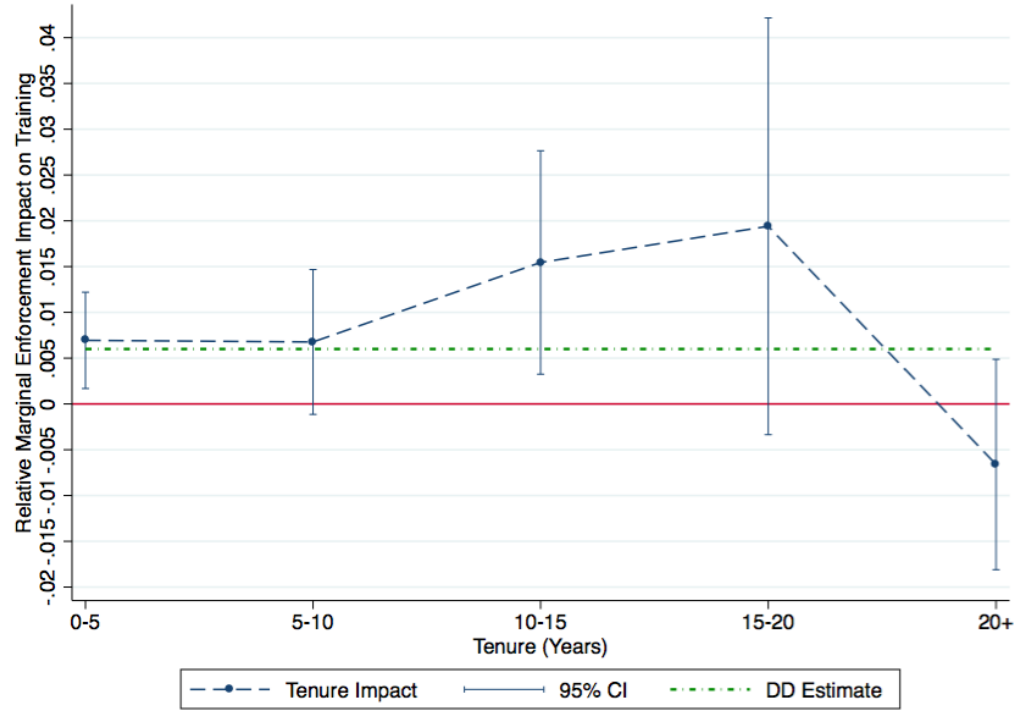
Figure IX: Enforcement Effects by Standard Deviations From the Mean
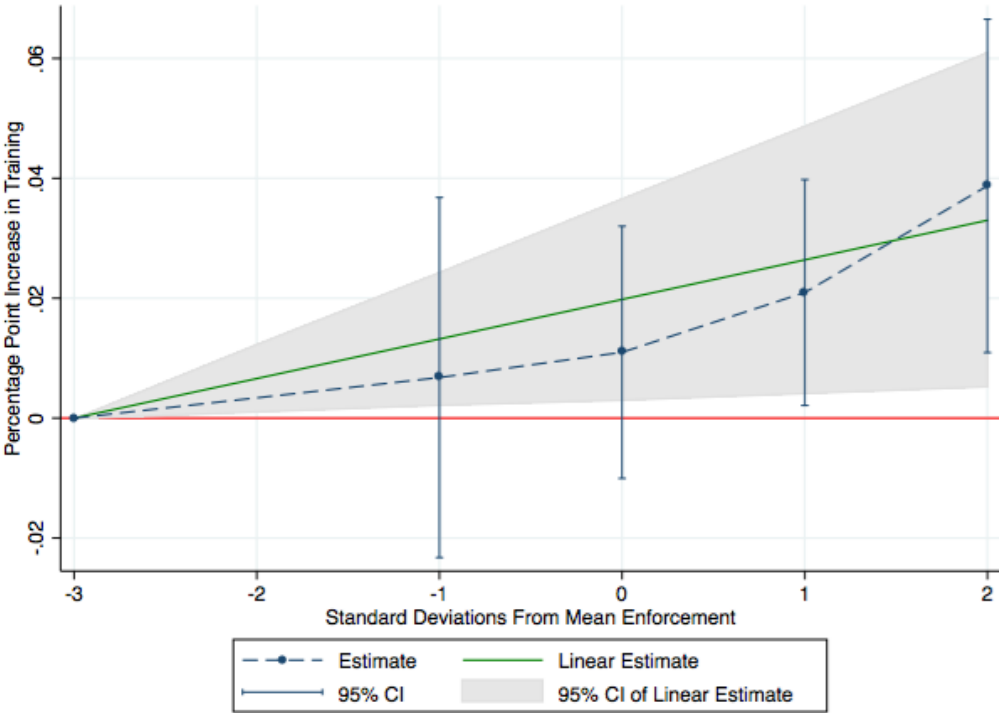
Tables

Table I: Factor Analysis Index

Question	1991			2009			
	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

Table II: Do Workers Bargain Over Their Noncompete?

	Have Outside Offers?		
	Yes (%)	No (%)	Total (%)
Bargain over Noncompete?			
Yes	19.93	5.97	10.22
No	80.07	94.03	89.78

If No, Why Not?	%
Was Reasonable	50.5
Assume Couldn't	40.4
Fired	22.9
Create Tension	20.2
Firm Wouldn't Sue	7.0
Court Wouldn't Enforce	5.3
Other	5.1

Note: This table is reproduced from Starr et al. (2014b)

Table III: 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 IV: 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.

Table V: 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.

Table VI: Summary Statistics of Firm-Sponsored Training Type

Variable	Low Litigation		High Litigiation	
	Mean	SD	Mean	SD
Onsite Taught By Coworker	0.08	0.26	0.10	0.30
Onsite Taught By Outsider	0.02	0.14	0.04	0.20
Offsite Training	0.04	0.20	0.09	0.29
Observations	30,094		40,280	

Table VII: Firm-Sponsored Training Type

		Onsight Taught By		
Intent-to-Treat Effect	Baseline	Coworker	Outsider	Offsite
High Litigation	0.007**	-0.000	0.002**	0.005***
	(0.003)	(0.002)	(0.001)	(0.002)
Observations	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. The set of controls are the same as the baseline specification discussed on page 26.

Table VIII: Summary Statistics of Firm-Sponsored Training Content

Variable	Low Litigation		High Litigiation	
	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	

Table IX: 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 26.

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 26.

Table XI: A Mechanism and Two Other Predictions				
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 43 on page 32 (for initial experience), 46 on page 33 (for log wages), and 44 on page 32 (for tenure). Tenure is in years and log earnings are log monthly earnings.

Table XII: 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 26.

Table XIII: 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 Noncompete Indices Robustness Check

Intent-to-Treat Effect	Baseline	Bishara Index		Garmaise Index		Lubotsky-Wittenberg	
		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*, I use the 1991 and 2009 Bishara aggregated index for those years (with his weights), and the same for Garmaise 1992 and 2001. The Lubotsky and Wittenberg (2006) method refers to using their procedure 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 26.