Consider This: Training, Wages, and the Enforceability of Covenants Not to Compete*

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Abstract

This study examines the effect of noncompete enforceability on training and wages. An increase from non-enforcement to mean enforceability is associated with a 14% increase in training, which tends to be firm-sponsored and designed to upgrade or teach new skills. In contrast to theoretical expectations, the results show no evidence of a relationship between noncompete enforceability and self-sponsored training. Despite the increases in training, an increase from non-enforcement to mean enforceability is associated with a 4% decrease in hourly wages. One noncompete policy that does not reduce training but is associated with higher wages throughout the distribution is the requirement that firms provide workers with consideration beyond continued employment in exchange for signing noncompetes.

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1 Introduction

Recent White House and US Treasury reports link the decline in economic dynamism (Davis and Haltiwanger, 2014), post-recession wage stagnation (Krueger, 2017; Furman, 2016), and underinvestment in training to a little known feature of employment contracts: covenants not to compete ('noncompetes'), which prohibit employees from joining or starting a competing firm upon departure (WhiteHouse, 2016; Treasury, 2016). Recent estimates suggest that nearly 40% of US Labor Force participants have signed a noncompete at some point in their career (Starr et al., 2017a). By increasing moving costs to competitors, enforceable noncompetes shield the firm from competitor-based wage competition and subsequently provide incentives for the firm to invest in employee human capital. While extant research has found a negative relationship between noncompete enforceability and mobility,[1] and many have theorized about the relationship between noncompetes and human capital investment,[2] little work has empirically examined the extent to which noncompete enforceability encourages human capital investment, whether the returns to such investments accrue to the employee, and which components of noncompete enforceability drive these relationships.[3] In this study I examine these gaps in our understanding directly.

Theoretical models typically posit an ambiguous relationship between enforceable noncompetes and *net* human capital investment since noncompetes reduce the return to self-sponsored investment but increase the returns to firm-sponsored investment (Garmaise, 2009; Ghosh and Shankar, 2015). Such models also typically predict that noncompetes will raise worker welfare (e.g., wages), because the choice to enter into a noncompete is voluntary and subject to negotiation (Callahan, 1985; Rubin and Shedd, 1981). However, historical accounts suggests that noncompetes were not even mandatory subjects of bargaining for

[1] See, for example, Stuart and Sorenson (2003); Marx (2011); Marx et al. (2015); Starr et al. (2017b); Marx et al. (2009); Marx (2011); Garmaise (2009); Lavetti et al. (2014).

[2] See, for example, Rubin and Shedd (1981); Posner et al. (2004); Meccheri (2009); Garmaise (2009); Ghosh and Shankar (2015)

[3] Notable exceptions include Garmaise (2009), which examines mobility and earnings of executives but does not examine the human capital investment mechanism directly.

unions (NLRB, 1992),[4] and recent evidence shows that employees rarely negotiate, are frequently asked to sign *after* they have accepted the job, and do not typically have another employment opportunity when they are asked to sign (Starr et al., 2017a).[5] Recognizing the potential for workers to find themselves unwittingly bound by a noncompete,[6] some states have passed 'consideration' laws, which tie the enforceability of the noncompete to the employer providing higher wages, a bona fide promotion, early notification of the contract, or some other form of consideration (e.g., a bonus or training). Such 'consideration' policies reduce the circumstances under which a noncompete is enforceable and provide the worker with some sort of compensation for giving up his post-employment mobility.

In this paper, I empirically examine the relationship between noncompete enforceability, training, and wages, focusing both on who pays for training and whether consideration policies exhibit any differential effects. To develop these measures of noncompete enforceability, I use factor analysis to weigh two 'consideration' dimensions and five 'non-consideration' dimensions of noncompete enforceability recently quantified by Bishara (2011). With these new indices, I employ a difference-in-differences identification strategy that exploits cross-sectional variation in the enforceability of covenants not to compete and occupational differences in the propensity to sign a noncompete.

The results suggest that if a non-enforcing state adopted average enforceability levels, then the incidence of training would increase by 14%. The positive relationship between enforceability and training is strongest when the training content is meant to upgrade skills and when it is firm-sponsored. In contrast to the theoretical expectations from the existing literature (Lobel and Amir, 2013; Garmaise, 2009), I find no evidence of a relationship between self-sponsored training and enforceability. Despite the increase in training, average

[4] Only just recently is this precedent being reviewed by courts. See Gurrieri (2016).

[5] The interested reader is referred to the detailed discussion presented in Starr et al. (2017a) regarding the implementation and negotiation over noncompetes.

[6] See for example, the Reddit thread on how a new CEO forced existing employees to sign a 3-year, nationwide noncompete with no additional consideration: `https://www.reddit.com/r/personalfinance/comments/65p21w/hrrecruiters_of_reddit_im_a_27_year_old_being/?limit=500`. Or, how noncompetes signed by workers when they were young or who needed the job have come back to hurt them (Dougherty, 2017).

hourly wages are lower in higher enforceability states: an increase from non-enforcement to mean enforceability is associated with a 4% decrease in wages. This wage effect is driven primarily by the fact that in high enforceability states individuals are less likely to appear in the right half of the wage distribution.

Disaggregating enforceability into separate consideration-specific and non-consideration indices reveals that estimates from the aggregate index mask substantial differential effects. In particular, the negative wage estimates are driven by state policies that do not require any additional consideration in exchange for offering a noncompete, while the positive training effects are driven by the non-consideration dimensions of enforceability. These findings are consistent with a model in which consideration laws transfer part of the surplus to the worker but do not affect the marginal benefit or cost of training.

These results are robust to a variety of different measures of noncompete enforceability, different measures of noncompete-exposure, and to the inclusion of a variety of potentially confounding variables. Diagnostic tests suggests that selection on unobservables must be quite strong to overturn the results.

This body of results contributes to a number of related literatures. In the literature on noncompetes, there is a growing ambivalence towards the enforcement of these contracts as a result of recent evidence showing it dampens mobility, entrepreneurship, and innovation.[7] Few studies, however, have empirically examined to what extent firms and workers actually benefit from the protection offered by enforceability, and no studies, to my knowledge, have examined the differential effect of individual noncompete policies.[8] Indeed, proponents of noncompetes argue that their voluntary nature implies that workers would exhibit reduced mobility and entrepreneurship, but that they would nevertheless be better off as a result,

[7]For the mobility results, see Marx et al. (2009); Garmaise (2009); Fallick et al. (2006); Marx et al. (2015). For the entrepreneurship results, see Stuart and Sorenson (2003); Samila and Sorenson (2011); Starr et al. (2017b). For the innovation results, see Samila and Sorenson (2011); Garmaise (2009).

[8]Among those that examine the benefits of noncompetes, Lavetti et al. (2014) find that physicians who sign noncompetes tend to earn 11% more because they are allocated more clients. Similarly, Conti (2014) finds that noncompete enforceability is associated with riskier firm R&D investments, while Younge and Marx (2013) find that Tobin's q increased by 9.75% after noncompetes became enforceable in Michigan. Garmaise (2009) finds a negative relationship between capital investment and enforceability.

either through training, increased wages, or some other benefit. I find that firms in higher enforceability states do provide more training to their workers but that the workers do not experience the returns to such training; rather, they experience wage losses.

While the wage findings echo the results in Garmaise (2009), I find no evidence for his proposed mechanism – reduced self-sponsored investment. The results here align with a more monopsonistic view of the labor market (Manning, 2003), whereby the enforceability of noncompetes reduces the elasticity of labor supply and puts downward pressure on wages. Nevertheless, for policymakers concerned about the distribution of the surplus, the adoption of consideration policies improves wages for workers throughout the wage distribution without dampening the incentives to invest in training.

These results also contribute to our understanding of the role of labor market frictions generally,[9] and within-industry mobility frictions specifically,[10] in determining training and wage patterns. These results suggest that within-industry frictions can indeed incentivize investment, but that such frictions may also prevent workers from receiving the returns to such investments. The results also point to a relationship between the legal environment and heterogeneity in management practices, with subsequent implications for productivity and performance differentials (Shaw, 2004; Bloom and Reenen, 2011; Younge and Marx, 2013; Younge et al., 2014).

The rest of the paper is organized as follows: Section 2 describes the existing theories relating noncompetes to mobility, training, and wages. Section 3 describes the measurement of noncompete enforceability and introduces the data and the identification strategy. Section 4 provides the results and robustness checks, Section 5 discusses the results, and Section 6

[9] For example, recent work in the training literature has been concerned with identifying labor market imperfections that create a wedge between the internal and external value of the worker (Acemoglu and Pischke, 1999), including 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), moving costs (Katz and Ziderman, 1990; Benson, 2013), and commitment to training (Dustmann and Schönberg, 2012). A related literature looks at similar market frictions that prevent the free flow of labor (Naidu, 2010; Naidu and Yuchtman, 2013).

[10] See, for example, the recent work on occupational licensing (Kleiner and Krueger, 2013), trade secret law (Png, 2012), and the doctrine of inevitable disclosure (Hsu et al., 2015; Png, 2012)

concludes.

2 Theory and Assumptions

While the contribution of the present study is primarily empirical, there is a significant body of theoretical work that describes the relationship between the enforceability of noncompetes, training, and mobility. Building from Becker (1962), the initial work by Rubin and Shedd (1981) argued that, in perfectly competitive labor markets, an employee moves to her most productive job, pays for all general training, shares in the quasi-rents of specific training, and never leaves; as a result, there is no need for a noncompete. However, if employees are liquidity-constrained such that they will be unable to pay for the value of any training or sensitive information that is shared with them, then enforceable noncompetes solve a hold-up problem by preventing the employee from appropriating the value of investments for which he did not pay, thus providing the proper incentives for firms to invest in training or the creation of information in the first place (Posner et al., 2004).

Subsequent theoretical work has examined the relationship between noncompetes and various types of training. Meccheri (2009) extends the negotiation framework of MacLeod and Malcomson (1993) to examine the firm's incentive to invest in general versus firm-specific training. He argues that noncompetes increase the return to general investments relative to firm-specific investments because the noncompete improves the firm's bargaining power over surplus sharing for general investments, but not firm-specific investments. Meccheri (2009) links these findings to Acemoglu and Pischke (1999) by suggesting that noncompetes create a wedge between the internal and external wage structure, resulting in wage compression that incentivizes the firm to provide general training.

Other recent work highlights the contrasting incentives of firms and individuals to invest in human capital as a result of noncompetes (Ghosh and Shankar, 2015; Lobel and Amir, 2013). Such studies note that, while noncompetes increase the incentives of the firm to invest in employee human capital, individuals have less incentive to invest in themselves since they cannot capture the returns in the external market. For example, though he does not explicitly analyze any training data, Garmaise (2009) argues that executives earn less in higher enforceability states is because they invest less in themselves.

In general, there are two significant shortcomings of these theories. The first is that, until recently, none of the assumptions underlying these models, have been tested. That is, most models assume fully informed, rational agents who will only enter into a noncompete agreement if they will be better off in expectation. Recent evidence suggests that this may not always be the case because of incomplete information, lack of alternative options, and a lack of negotiation. Starr et al. (2017a) finds roughly 1/3% of workers do not even know if they are bound by a noncompete, that less than 10% of noncompete signers actually negotiate over their noncompete,[11] that roughly 1/3 of the time the noncompete is requested *after* the employee has already agreed to the terms of the job, and that only 30% of employees have another offer at the time they are asked to sign. In perhaps the most surprising case, noncompetes have only recently become mandatory subjects of union bargaining: a 1992 National Labor Relations Board memo argued that a covenant not to compete was a nonmandatory subject of negotiation because it did *"not have material or significant effect on terms and conditions of employment because it became operable only after employee voluntarily or involuntarily left employment with employer."* (NLRB, 1992; Gurrieri, 2016). Lastly, Marx (2011) finds that over 90% of electronics engineers agree to sign noncompetes when asked. Taken together, these statistics cast doubt on the fully informed bargaining models in the literature. They suggest that the reality of the labor market contains many more frictions and perhaps behavioral biases than were previously envisioned, perhaps lining up more accurately with models of monopsony power (Manning, 2011) than of contracting. Regardless, the available evidence suggests that for many workers a noncompete is a take-it-or-leave-it proposition, for which they may or may not be compensated.

[11] Legal scholars have long been concerned about the potential lack of negotiation over these contracts (Arnow-Richman, 2001, 2006).

The second shortcoming of these studies is that none of the theoretical predictions regarding the relationship between noncompete enforceability and training have been, to my knowledge, tested empirically. In the following sections, I seek to examine empirically the relationship between noncompete enforceability, training, and wages. In particular, I focus on whether enforceability is associated with firm or self-sponsored training, and, given the limited observed negotiation in other studies, the potential for state laws tying the enforceability of the noncompete to the receipt of additional consideration to have a differential effect on training and wages.

3 Empirical Approach

3.1 Quantifying Noncompete Enforceability

While noncompetes are virtually unenforceable in California and North Dakota, most states will enforce them by implementing their own versions of the 'reasonableness doctrine,' which balances the protection necessary for the firm with the injury to the worker and society.[12] Among enforcing states, there is unanimous agreement that a necessary condition for the enforceability of a noncompete is that the worker possesses some kind of valuable information in which the firm has made a significant investment and which it seeks to protect, such as trade secrets, client lists, or other confidential information.[13]

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

[13]Some states, such as Florida and Kentucky, include extraordinary general skills training in this list of protectable interests, but traditionally it has been omitted (Blake, 1960). Regardless of whether general training is itself a protectable interest, however, the training 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 were 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.

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

Even after courts identify whether the worker possesses a 'legitimate business interest,' significant variation remains in how states perceive reasonableness or respond to the unreasonableness of various other dimensions of a 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. However, Wisconsin, which uses the so-called 'red-pencil' doctrine, will throw out the entire contract if it is deemed overbroad along any dimension. States also have different enforceability protocols for whether continued employment is sufficient consideration for the enforcement of the noncompete: 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 the firms do not notify the worker in advance, the firm must provide the worker with a 'bona fide advancement' within the firm in order for the noncompete to be enforceable. Malsberger et al. (2012) tracks these and other dimensions of enforceability in his volumes *Covenants Not to Compete: A State-by-State Survey* .

Three attempts have been made to quantify the enforceability of noncompetes (Bishara, 2011; Garmaise, 2009; Stuart and Sorenson, 2003), but they have done so in a rather ad-hoc way without explicitly stating the object being measured. One natural metric would capture the probability that a randomly chosen employee's noncompete would be enforced in court if the employee left for a competitor and his parent firm sued. To capture this probability, one would have to know (1) under what situations a state would enforce a noncompete, and (2) how frequently those circumstances occur in the noncompete-signing population. All existing indices capture (1) in various ways, but ignore (2). Stuart and Sorenson (2003) take the simplest approach, creating a simple enforceability dummy. The Garmaise (2009) index measures 12 dimensions of enforceability with a binary score and adds up the scores for each state, assuming that each dimension has equal weight. Bishara (2011) assigns each

that if the worker leaves too soon he must pay back damages to the firm (Von Bergen and Mawer, 2007).

state a score between 0 to 10 on seven dimensions of noncompete enforceability for 2009 and 1991 and aggregates the individual dimensions using subjectively chosen weights.[14] I improve upon Bishara's weighting scheme by using confirmatory factor analysis on his seven scores to generate weights for each dimension, which may better approximate the underlying importance of the various dimensions of enforceability.[15] Due to the highly correlated nature of the individual dimensions of enforceability, however, all weighting schemes that give non-negative weights to each dimension result in highly correlated aggregate indices. Confirmatory factor analysis as a reweighting tool is therefore a modest improvement.

Factor analysis postulates that each dimension of noncompete enforceability depends linearly upon latent enforceability. Defining x_{is} as observed enforceability dimension i for state s and $Enforceability_s$ as latent enforceability, the model is defined by the set of equations

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

where ϵ_{is} is random noise.[16] Under the normalization that $\lambda_1 = 1$, the correlation matrix of the observed enforceability dimensions identifies the other λ_i terms because $corr(x_i, x_j) = \lambda_i \lambda_j$. Given estimates of the λ_i terms, one can back out an estimate of the enforceability index. Regressing this estimate of the enforceability index on the dimensions of enforceability gives the weights.[17] The enforceability index is normalized to have a mean of zero and a standard deviation of one in a sample in which each state is given equal weight. Table 1 reports the mean, standard deviation, and weight of each dimension of enforceability for

[14] A complete explanation of the Bishara (2011) scoring method is available in Appendix B.

[15] A better index would incorporate the distribution of characteristics relevant for enforceability into the index itself. Such data, to my knowledge, is not available. An alternative is to use the method by Lubotsky and Wittenberg (2006), which shows 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.

[16] 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 $s \neq k$.

[17] See Kolenikov (2009) for details. See Black and Smith (2006) for an example of using factor analysis to generate an index of college quality.

1991 and 2009 from Bishara (2011) and the resulting weights from the factor analysis.

The factor analysis generated weights correspond surprisingly well with Bishara's subjectively chosen weights, putting slightly more weight on the extent of protectable interests within the state, and on consideration at the inception and after the inception of employment. Table B1 in Appendix B shows the exact scores for each state. California and North Dakota have the lowest scores, while Florida and Connecticut have the highest. Overall, the variation across states is large, while the index shows very little variation over time: The correlation between the enforceability scores in 1991 and 2009 is 0.94, which reflects the fact that, despite the recent legislative interest in noncompetes (Treasury, 2016), few states changed their policies between 1991 and 2009.[18]

3.2 Data

The data for this study comes from the topical module from Wave 2 of the Survey of Income and Program Participation (SIPP) panels from 1996, 2001, 2004, and 2008.[19] The SIPP is a longitudinal survey that interviews respondents once every four months for three or four years. As the training questions are asked once per individual, in Wave 2, I pool all of the cross-sections together to gain power. The SIPP tracks up to two occupations for each individual; 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.[20]

[18] Enforceability is not correlated with a state's political leanings (Lavetti et al., 2014) and does not appear to be clustered geographically (See the map in Appendix B.3.)

[19] 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 (Loewenstein and Spletzer, 1997), and the PSID is 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 across the enforceability spectrum.

[20] Occupation codes are updated to 2007 two-digit Standard Occupational Classification (SOC) codes and industry codes are updated to 2007 two-digit NAICS codes.

The SIPP contains training data reflecting answers to the following 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?"* For the 21% of individuals who respond "yes" to this question, the SIPP asks follow-up questions on the number of such training events in the last year, as well as questions about the most recent training event including where it occurred, what the training covered, and who paid for it.[21] Table 3 shows descriptive statistics for these training variables among the population of individuals who report receiving training.[22]

3.3 Identification Strategy

In order to isolate the cross-sectional heterogeneity in noncompete enforceability from other state-level factors, I compare how the within-state-year differences between occupations where noncompetes are used frequently and noncompetes are used infrequently change as noncompete enforceability increases. The low-use occupations thus act as a psuedo-control group, unaffected, or, at least less affected, by noncompete enforceability because they sign noncompetes less frequently. Using the reported incidence of noncompetes from Starr et al. (2017a) across occupations, as reported in Table 2, I divide occupations into high-use and low-use based on whether the occupation has an incidence of noncompetes greater than the national average of 18.1%.

Table 3 presents summary statistics for key variables by noncompete-use status. Workers in high-noncompete-use occupations are very different from those in low-use occupations. High-use occupations experience 14 percentage points more training than low-use occupa-

[21]The SIPP panels date back to 1983 and were substantially redesigned in 1996, especially the training questions. Before the 1996 redesign, the main training question was 'Has [the respondent] *ever* received training designed to help find a job, improve job skills or learn a new job?' Despite the fact that everyone has received training to improve job skills at some point in their life, the proportion responding yes was just 27%. Given confusion over what responses to this question meant, the SIPP redesign in 1996 changed the questions to reflect training in the last year that was designed to improve skills. Given the poor survey questions and ambiguity around the answers in the older SIPP panels, I focus only on the SIPP panels with the re-designed training questions.

[22]The data do not directly contain information on whether training is general or firm-specific. The fact that most training is meant to upgrade existing skills, teach basic skills, or teach new skills suggests that the training is general in nature.

tions. They are also more likely to have bachelor's and graduate degrees, more likely to be white, less likely to be unionized, have longer tenures and earn $11 more per hour.

With this psuedo-difference-in-differences strategy, the simplest empirical specification would include noncompete enforceability, a high-noncompete-use occupation dummy, and their interaction. To increase the precision of the model, the main specification subsumes the high-use dummy with occupation-by-industry-by-year dummies, and state-by-year fixed effects subsume the main effect of enforceability. The state-by-year fixed effects account for all time-varying state-level variables, while the occupation-by-industry-by-year dummies ensure that the effect of noncompete enforceability is identified by comparing individuals in the same jobs in the same year. The full specification is:

$$Y_{iojst} = \beta_0 + \beta_1 Enforceability_s * \mathbf{1}(High\text{-}Use_o) + \gamma X_{ist} + \Omega_{o,j,t} + \theta_{s,t} + \epsilon_{iojst} \tag{2}$$

In equation (2), Y_{iojst} refers to wage, training, and mobility measures for worker i in occupation o, industry j, state s, in year t. State by year fixed effects are represented by $\theta_{s,t}$ and occupation-by-industry-by-year fixed effects are given by $\Omega_{o,j,t}$. Individual controls are given by X_{ist}, which include hours worked, a quadratic in age, and indicators for working in a metro area, bachelors degree, graduate degree, male, white, and whether the worker is unionized. High-use occupations are denoted $High\text{-}Use_o$, and $Enforceability_s$ is noncompete enforceability level of state s in 1991.[23] The standard errors are clustered at the state level to account for state-level correlations in the disturbances (Moulton, 1990; Bertrand et al., 2004). In equation (2), the coefficient of interest is β_1, which captures how the within-state-year difference between high- and low-use occupations changes as noncompete enforceability increases.

This estimate is likely to be an underestimate of the true causal effect of noncompete enforceability because low-use occupations also sign noncompetes (Starr et al., 2017a), and

[23] I use the 1991 enforceability scores because they occur before the collection of the SIPP data, but since the correlation between the 1991 and 2009 scores is so high, the results are robust to either measure.

thus the difference between high- and low-use occupations will attenuate the true effect. To address this I use the continuous measure as a robustness check and find consistent results.

As is common in this literature (Marx et al., 2009; Samila and Sorenson, 2011; Stuart and Sorenson, 2003; Garmaise, 2009), whether a worker has signed a noncompete is not contained in the data. Therefore, one way to interpret a coefficient like β_1 from equation (2) is as an intent-to-treat effect. The state with a high intensity of enforceability is offering a treatment, but firms can choose to opt out of treatment by not using noncompetes. While identifying the effects of enforceability on those who do and do not sign noncompetes are important parameters, the intent-to-treat effect is the relevant parameter for state judiciaries and legislatures to consider since they choose the intensity of enforceability but cannot force firms to use noncompetes.

4 Results

4.1 Training Results

Table 4 reports the results from estimating equation (2) with various training dependent variables. In Panel A, the dependent variable is simply a dummy for reporting the receipt of skill upgrading training in the last year. Columns (1)-(4) show the breakdown of the effect of noncompete enforceability when adding individual controls, occupation-by-industry-by-year fixed effects, and state-by-year fixed effects. Column (1) of Panel A shows that a one standard deviation increase in noncompete enforceability is associated with a 0.77 percentage point increase in the probability of receiving training for low-use occupations, and an additional 0.67 percentage point increase in the probability of receiving training for high-noncompete-use occupations. Including individual controls, occupation by industry by year fixed effects, and state by year year fixed effects, the point estimate on the interaction of enforceability and high-use is 0.77 percentage points. To grasp the size of this coefficient, suppose that a non-enforcing state (score of roughly -4) adopted the enforceability policies of an average enforcing state (enforceability score of zero). These results suggest that such a change in policy would increase training by 3.08 percentage points (4*0.77), which is an 14.7% increase in training (3.08/21) relative to the mean likelihood of receiving training.

Figure 1 examines the likelihood of participating in multiple training events as a result of greater noncompete enforceability. In particular, the figure plots the coefficient on the interaction between noncompete enforceability and the high-use occupation indicator in a series of fully specified models (column (4) of Table 4) in which the dependent variable is an indicator for receiving at least one, two, three, ..., ten training events in the last year. The figure shows that the association between noncompete enforceability and the likelihood of receiving at least a given number of training events is strongest for the first few training events and positive and statistically significant up until the sixth training event, after which the effects are still positive but statistically indistinguishable from zero.

Panel B of Table 4 examines who paid for the most recent training, where it occurred, and who performed it. In columns (1) and (2), the dependent variable is equal to one if the most recent training event was firm-sponsored (1) or self-sponsored (2) and zero otherwise. The results show that the positive correlation between noncompete enforceability and training observed in Panel A is driven almost entirely by firm-sponsored training. The relationship between noncompete enforceability and self-sponsored training is practically zero. In the second half of Panel B, I perform a similar exercise in which the dependent variable is equal to one if the training is onsite and taught by a coworker (3), onsite but taught by an outsider (4), or offsite (5). The results show that the observed relationship between noncompete enforceability and training is coming primarily from training that is offsite (0.58 percentage points) or onsite but taught by an outsider (0.25 percentage points). Relative to simple on-the-job training taught by a co-worker, these investments are likely to be more costly.

Panel C of Table 4 considers the content of the most recent training. Training content is categorized into the following non-mutually exclusive categories: basic skills, new skills, upgrade existing skills, and company policies. Table 3 gives summary statistics of these outcomes by high and low-noncompete-use status. More than two-thirds of the 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 in what the most recent training covers. Panel C reports results from the main specification using indicators for content received as the dependent variable. The results show that noncompete enforceability is positively and statistically significantly associated with all types of training, though the largest effects are observed for upgrading skills (0.49 percentage points) and new skills (0.42 percentage points).

Taken together, the results provided here suggest that there is a strong positive relationship between noncompete enforceability and the firm's willingness to invest in multiple training events that tend to be offsite or outsider taught, and are primarily meant to upgrade skills and teach new skills. There is no evidence of a negative relationship between noncompete enforceability and self-sponsored training.

4.2 Who pays for training?

Before providing evidence on the impact of noncompete enforceability on wages, I confirm work by Barron et al. (1999) and others that, in the SIPP data, those who receive training do not take an observable wage cut to pay for it. In Table 5, the receipt of training is associated with 11% higher hourly wages after controlling for occupation, industry, and individual-level controls. The correlation is identical for workers in their first year of tenure. Using the log of the number of training events instead of a dummy for training reveals similar results. These coefficients may be biased upward if high-skill individuals sort into high-training jobs. Thus while one cannot be completely confident that individuals are not taking wage cuts to pay for training, the results here and from prior studies suggest that it is firms, not workers, who are paying for the observed training.

4.3 Mobility and Wages

Next, I corroborate the findings of the prior literature that noncompete enforceability is associated with reduced employee mobility. Column (1) of Table 6 uses tenure (lack of prior mobility) of the respondent as the dependent variable in the fully-specified model, and shows that a one standard deviation increase in noncompete enforceability is associated with an increase in tenure of 0.14 years. If a non-enforcing state adopted mean enforceability policies, this estimate suggests that mean tenure would increase by 0.56 years.

Because noncompete enforceability acts a shield from competitors and because employees hardly ever negotiate over noncompetes, the observed positive relationship between noncompete enforceability and training may not lead to increased wages for the employee. In Table 6, I re-estimate (2) using as a dependent variable log hourly wages and a dummy variable for having earnings in at least the 3rd, 6th, and 10th deciles among the high-use wage distribution (the deciles are calculated within each survey year). Column (2) shows that, on average, a one standard deviation increase in noncompete enforceability is associated with a roughly 1% decrease in hourly wages. If a non-enforcing state adopted the mean level of enforceability, then wages would fall by 4%.

Looking at the effect of noncompete enforceability on the distribution of wages indicates that the decrease in hourly wages is coming from employees at the upper end of the wage distribution. Figure 2 plots the coefficient on the interaction between noncompete enforceability and high-use occupations in a series of regressions in which the dependent variable is an indicator for having earnings in at least the 1st, 2nd, ..., 10th decile among the high-use occupations. The figure shows that noncompete enforceability is not associated with any differential effect on the probability of having earnings greater than the third, fourth, and fifth deciles, but that it is negatively associated with the likelihood of having earnings in at least the 6th, 7th, 8th, 9th, or 10th decile. Column (5) of Table 6 shows that a one standard deviation increase in enforceability reduces the probability of having earnings within the 10th decile by 1.08 percentage points.

4.4 The differential effect of 'consideration' laws

The measure of noncompete enforceability aggregates numerous underlying dimensions of enforceability, masking any potential differential effects of consideration and non-consideration laws. In this section, I divide the enforceability index into these separate components.

To provide a better understanding of consideration laws, I provide below the exact description of the consideration questions from Malsberger et al. (2012), which were scored from 0 (low enforceability) to 10 (high enforceability) by Bishara (2011):

> Consideration at inception: *Does the signing of a covenant not to compete at the inception of the employment relationship provide sufficient consideration to support the covenant?*
>
> Consideration post inception: *Will continued employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun? 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?*

High scores from Bishara (2011) on these two questions reflect that noncompetes are enforceable when no additional consideration beyond continued employment is provided. Low scores reflect that, in order for a noncompete to be enforceable, the employee must receive some additional consideration, which typically takes the form of a promotion, a wage increase, or additional training. For example Gomulkiewicz (2015) describes that in *Labriola v. Pollard Group, Inc.*, "the employer paid no additional compensation for the [5 year] non-compete" and that the "[Washington state] Court held that on-going employment is not sufficient consideration for a noncompete signed after the hiring date nor is on-the-job training when the employee comes to the employer with experience and training."[24]

To disaggregate the aggregate measure of enforceability into consideration (i.e., the two questions above) and non-consideration (i.e., the other five questions in Bishara (2011)) components, I utilize the weights in Table 1 to aggregate the two consideration dimensions

[24] See Labriola v. Pollard Group, Inc., 100 P.3d 791, 792 (Wash. 2004).

into one 'consideration' index and the five non-consideration dimensions into a separate 'non-consideration' index. Both indices are then normalized to have a mean of zero and a standard deviation of one in a sample in which each state has a weight of one. I reverse-code the raw 'consideration' index by multiplying it by negative one, such that higher scores reflect that the state requires some sort of additional consideration.

Using these consideration and non-consideration measures of enforceability, Table 7 reports the results from the main specification for training and wages. Column (1) shows that the relationship between noncompete enforceability and training is driven by the non-consideration dimensions of enforceability: notably, the coefficient on non-consideration dimensions is roughly 50% larger than for the overall index. This occurs because the adoption of consideration policies reduces overall enforceability but is itself associated with additional (but statistically insignificant) training.

To examine the effects of these consideration and non-consideration indices of noncompete enforceability on the distribution of training, Figure 3 plots the coefficients on both consideration and non-consideration measures in a series of regressions in which the dependent variable is the receipt of 1, 2, ... 10 training events. As in column (1) of Table 7, Figure 3 shows that the increase in the likelihood of receiving training for the first, second, third,..., event is due to the non-consideration component of enforceability. The consideration component is positively related to receiving the first few training events but is nearly zero for all subsequent training events.

Regarding wages, Column (2) of Table 7 shows that a one standard deviation increase in consideration increases the average hourly wage by 1.1%, while a one standard deviation increase in non-consideration dimensions of enforceability is associated with a 0.03% decrease in wages. In other words, the negative wage effect identified in Column (2) of Table 6 is driven primarily by states that do not require any additional consideration in exchange for agreeing to a noncompete. Columns (4) and (5) of Table show that the positive consideration effect is driven primarily by those at the upper end of the wage distribution. Figure 4 presents the coefficients on the consideration and non-consideration measures in a series of regressions in which the dependent variable is an indicator equal to one if the individual has earnings in at least the 1st, 2nd, ..., 10th decile of the high-use wage distribution. The point estimates show that increased consideration is associated with higher wages throughout the wage distribution, though in most cases the effects are not statistically distinguishable from zero. By contrast, increases in non-consideration dimensions of enforceability are associated with lower earnings for those in the very right tail of the earnings distribution.

4.5 Robustness Checks

A causal interpretation of these results requires that $\mathbf{E}[Enforceability_s * \mathbf{1}(HighUse)_o * \epsilon_{iojst}|X_{ist}, \Omega_{o,j,t}, \theta_{s,t}] = 0$. The two primary concerns underlying whether this equation holds is measurement error in the enforceability index or the measure of the use of noncompetes, and potential omitted variables that change the wages or training of high-noncompete-use occupations in high enforceability states.[25] To address the measurement concerns, I examine the robustness of the results to the continuous measure of noncompete use and different measures of enforceability. To address the potential omitted variable concerns, I include a variety of potentially confounding controls and ultimately use the method in Oster (2017) to examine how sensitive the estimates would be to selection on unobservables.

In particular, one might be concerned that high-enforceability states are systematically different in unobserved ways that might affect wages and training. For example, California, one of the non-enforcing states, was the first to adopt exceptions to at-will employment, while Florida, the highest enforceability state, has yet to adopt any (Autor et al., 2006). Recall that all specifications include state by year fixed effects, such that the enforceability effects are identified based on comparisons between high- and low-use occupations within a state-year. As a result, such fixed effects will pick up all state-year variables as long as

[25] Reverse causality is not an issue here as the enforceability measures from 1991 occur well before the data are collected. Furthermore, most states have not changed their policies over time – California, for example, adopted its ban in 1872 (Gilson, 1999).

they have a common effect on low- and high-use occupations. What this approach does not address is the possibility that high-enforceability states might adopt other policies that affect training and wages for only high- or only low-noncompete-use occupations.

To address the possibility of such omitted variables, I examine the sensitivity of the results to the inclusion of numerous state-year variables, including dummies for the three exceptions to at-will employment (Autor et al., 2006), and dummies for being a right to work state – all interacted with the high-noncompete-use indicator. The results, presented in Panel B of Table 8, show that the main the results are unchanged.

A second concern is that high-enforceability states may be more likely to adopt policies that affect industries wherein high-noncompete-use occupations cluster – for example, California might treat its technology industry differently than Florida does. To address the possibility that high-enforceability states might treat certain high skilled industries differentially, I saturate the model even further with state-by-industry by-year fixed effects (as opposed to state-by-year fixed effects). The results, presented in Panel A of Table 8 show that the main results are robust to such saturation: noncompete enforceability is associated with more training, driven by the non-consideration dimensions of enforceability, and lower wages, driven by the lack of adoption of consideration laws.

A number of other selection concerns remain: perhaps firms that employ high-noncompete-use occupations are fundamentally different in high enforceability states, with regards to the propensity for training and payment practices. This could be the case if, for example, firms sort to such locations over time. To address this, and any other potential omitted variable, I employ the diagnostic test developed in Oster (2017), which extends the methods in Altonji et al. (2005) to test how strong selection on unobservables must be in order to drive the estimated treatment effects to zero.[26] Oster's method produces a parameter, δ, which

[26]The intuition behind the method is that as confounding controls are added to the model, if the coefficient of interest stays roughly the same size *and* the R-squared rises significantly, then there is significantly less unobserved variation that could overturn the results, and thus we should be relatively confident in the directionality of the point estimates. If, by contrast, the coefficient falls dramatically as controls are added, or the R-squared does not change much, then we would be less confident in the directionality of the estimate.

captures how strong selection on unobservables would have to be, relative to selection on observables, to drive the estimated treatment effects to zero. A value of $\delta > 1$ implies that selection on unobservables would have to be stronger than selection on observables. Oster suggests that if one can control for the first order variables of interest, then $\delta > 1$ is a natural cutoff to ascertain the robustness of the results. Following Oster's guidelines, I set the maximum R-squared to 30% higher than the R-squared from the most saturated model. As a baseline set of controls, I include an indicator for a high-noncompete-use occupation, the baseline set of individual controls, and state-by-year fixed effects. The advanced set of controls includes state-by-year-by-industry fixed effects and the interacted state-level controls described earlier.

The results of these most saturated regressions and the Oster diagnostic statistics are shown in brackets in Panel C of Table 8. The results are largely similar in this most saturated model as before. The δ term for the main training interaction in Column (1) is 1.3, suggesting that selection on unobservables would have to be 30% stronger than the selection on observables in order to reduce the estimated effect to zero. As before, the positive training effect is driven by non-consideration dimensions, which pick up a δ term of 0.55, suggesting that if selection on unobservables is roughly half as strong as selection observables it could drive the estimated treatment effect to zero. The wage results in Column (3) and (4) show a pattern that is consistent with the previous results. The δ term on the enforceability interaction in Column (3) is 2.24, while the δ term on the consideration interaction is 0.78. Both high values suggest that selection on unobservables would have to be quite strong to overturn the directionality of these results.

A last set of concerns relate relate to the robustness of the measure of noncompete enforceability and the measure of noncompete incidence. In Panel A of Table 9, I replace the dichotomous noncompete variable with the continuous incidence measure. The results are markedly similar to before: In higher enforceability states, a greater incidence of noncompetes in the occupation is associated with more training and lower wages. As before, the training differential is driven by the non-consideration dimensions of enforceability, though the coefficient on the consideration dimension is positive and statistically significant as well. The negative wage effect is similarly driven by the consideration dimensions.[27]

I also replicate the results with a different set of enforceability measures.[28] One may be concerned that the measure of enforceability has significant outliers because California and North Dakota are really the only the non-enforcing states. To address this concern, I rid the index of any cardinal differences in the enforceability scores and instead use an ordinal ranking of the states according to their enforceability. Panel A of Table 9 reports the results of the main specifications using rankings from 1 to 50, in which higher ranks refer to greater enforceability. The main effects of noncompete enforceability point in the same direction, but they are weaker and lose statistical significance at canonical levels (column (1) and (3)). Nevertheless, the consideration and non-consideration effects show the same pattern as before.[29]

I also replicate the results using the noncompete enforceability measure in Garmaise (2009). The interaction of high-use indicator with the Garmaise measure of enforceability has a coefficient in the training regression of 0.0063 (compared to 0.0077 in Column (4) of Table 4) with a p-value of 0.095, while the point estimate in the wage regression is -0.0115 (compared to -0.0099 in Column (2) of Table 6) with a p-value of 0.005.[30]

[27]In unreported results, I divide workers into high and low litigation occupations based on the extent to which a given occupation is found in noncompete-based court proceedings, using two surveys of noncompete cases (LaVan, 2000; Whitmore, 1990). The results are similar along all dimensions, and are available upon request.

[28]I omit the results from using the Bishara (2011) measure because it is so highly correlated with the present measures (0.97) that it provides very similar results.

[29]The marginal effects of the enforceability measures in ranks across the distribution of wages, for both overall enforceability and separate consideration and non-consideration measures, are provided in Figures A1 to A4.

[30]Full results are available upon request.

5 Discussion and Limitations

The evidence presented above suggests the following four relationships: Increased levels of aggregate noncompete enforceability are related to (1) an increased likelihood of receiving firm-sponsored, offsite and outsider-taught, skill-upgrading training, but do not change the likelihood that the last training event was self-sponsored, and (2) a decrease in average hourly wages. Disaggregating noncompete enforceability into separate consideration and non-consideration measures shows that the training results are driven by the (3) greater non-consideration components of enforceability, while the negative wage effects are a driven by (4) states that enforce noncompetes when no additional consideration beyond continued employment is provided.

These findings provide empirical support for the the prediction of the theoretical literature that noncompete enforceability encourages firms to invest in training (Meccheri, 2009; Posner et al., 2004; Rubin and Shedd, 1981), but they also contrast with the hypothesized relationship between noncompete enforceability and self-sponsored training (Lobel and Amir, 2013). For example, Garmaise (2009) also finds that executives earn less in high-enforceability states and argues that the reason is reduced executive self-investment. The results in the present study suggest that self-investment plays a very minor, if any, role. If reductions in self-sponsored training are not driving the negative wage effects, then the negative wage results call into question exactly why workers would agree to such provisions in the first place.

The recent facts described in Starr et al. (2017a) suggest that the contracting process may not be as competitive as theoretical models suppose, and that employers can wield substantial power by, for example, delaying the implementation of the noncompete until after the worker has turned down other job offers. Thus some workers may end up being bound by a noncompete without receiving any of the benefits that the contracting literature would suggest they receive (Sterk, 1993; Callahan, 1985). One policy, overlooked by previous studies that only examine one dimension of enforceability (Stuart and Sorenson, 2003; Samila and Sorenson, 2011; Garmaise, 2009), which counteracts these negative wage effects without dampening the investment incentives is the requirement that firms provide some sort of consideration in exchange for an enforceable noncompete – whether it is a bonus, additional training, a promotion, or even early notification of the noncompete.

These findings contribute to several literatures. They contribute to training literature and the literature on labor market frictions (Loewenstein and Spletzer, 1997; Acemoglu and Pischke, 2003; Barron et al., 1999; Manning, 2003; Naidu, 2010; Naidu and Yuchtman, 2013) by showing that a labor market friction between competitors induces firms to provide additional training and that state policies can contribute to the distribution of that surplus. These findings also contribute to the literature on productivity and management (Bloom and Reenen, 2011; Shaw, 2004) by showing that firms can utilize such mobility reducing policies to improve the skills (and productivity) of their of their workforce.

Lastly, this work contributes to ongoing debates in the noncompete literature in two ways (WhiteHouse, 2016; Treasury, 2016): Regarding the discussion around firm-sponsored versus self-sponsored training, this paper provides the first evidence that self-sponsored training is unrelated to enforceability, but firm-sponsored training is. One potential reason for this finding relates to information: Individuals may be unaware of their state policies, but firms are much more likely to be informed. Second, regarding the tension between noncompete enforceability, employee mobility, and training, the existing logic is such that increasing enforceability comes as a double edged sword in that it reduces and redirects employee mobility (Marx et al., 2009; Marx, 2011; Marx et al., 2015) but is necessary to ensure firm-sponsored investments (Rubin and Shedd, 1981; Posner et al., 2004). By breaking noncompete enforceability into separate subcomponents, this study reveals that the adoption of policies that tie the enforceability of the noncompete to the receipt of additional consideration serves to reduce noncompete enforceability in a manner that compensates the individual and avoids dampening the incentives to invest in human capital.

This study nevertheless faces numerous limitations. Most notably, the SIPP does not possess information on who signs noncompetes. Thus it is impossible to identify the effect of enforceability on those who do and do not sign. This is a clear avenue for future work. Second, as noncompete policies have been largely stable between 1996 and 2008,[31] this study uses cross-sectional variation in the enforceability of noncompetes to identify its effects. Relatedly, it is unclear if firms sort into high- and low- enforceability states based on whether they are high-paying or high-training firms. While such sorting may underlie the effects observed here, longitudinal data following a major change in enforceability would be required to assess the extent to which these policies determine the location of firms. If the growing policy debate results in a cascade of policy changes, then exploiting such changes, utilizing data on who signs noncompetes, and tracking the movement of workers and firms over time would provide a promising avenue for future research.

6 Conclusion

The development of skills (Heckman et al., 1999), wage growth (Furman, 2016), and the movement of workers (Decker et al., 2015; Topel and Ward, 1992) are crucial for economic well-being. The enforceability of covenants not to compete is a legal labor market friction that reduces wage competition in the labor market and restricts the flow of workers across competitors, but increases firms' incentives to invest in their workers. Due to claims that California's ban on noncompetes caused the tremendous growth of Silicon Valley, and recent research finding negative impacts of noncompete enforceability on employee mobility and new venture creation, such restrictions have been the recent focus of significant legislative scrutiny (Treasury, 2016; WhiteHouse, 2016).

Utilizing cross-sectional variation in state noncompete laws, this study finds that if a non-enforcing state adopted mean enforceability policies, then the incidence of training would rise by 14%, but wages would fall by 4%. Estimates from the aggregate measure of noncompete

[31] The only change that I am aware is the reversal and re-reversal of noncompete law in Louisiana between 2001 and 2003, which falls between survey years of the SIPP (Garmaise, 2009).

enforceability mask significant heterogeneity in the impact of laws that tie the enforceability of noncompetes to the receipt of additional consideration. Such 'consideration' policies reduce noncompete enforceability in such a way that wages are relatively increased but training is not reduced.

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Figures

Figure 1: Marginal Effect of Noncompete Enforceability on Participating in at Least 1, 2, ... 10 Training Activities

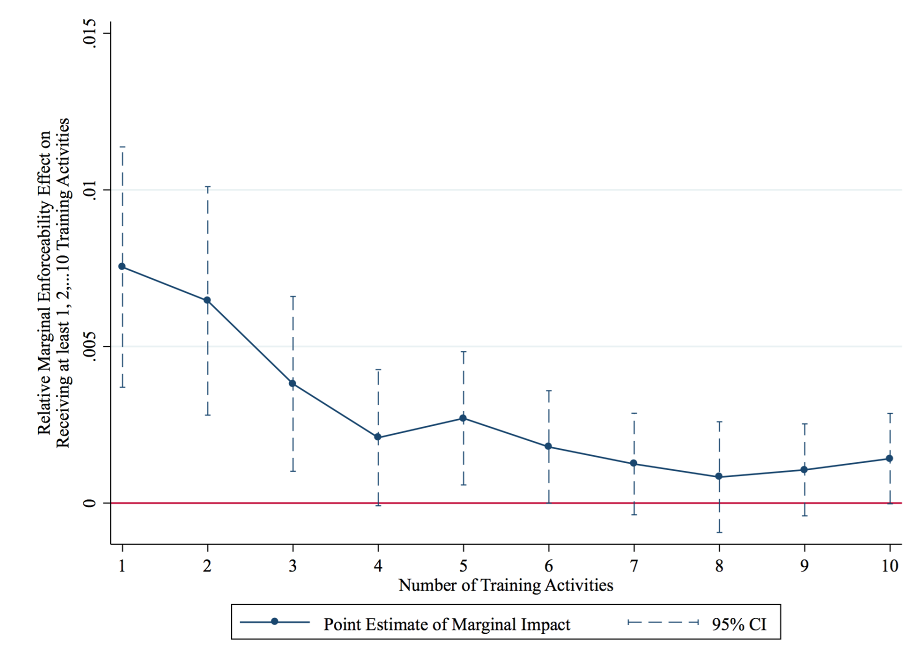


Figure 2: Marginal Effect of Noncompete Enforceability on having Earnings in at least the 1st, 2nd, ..., 10th Decile of the High-Use-Year Wage Distribution

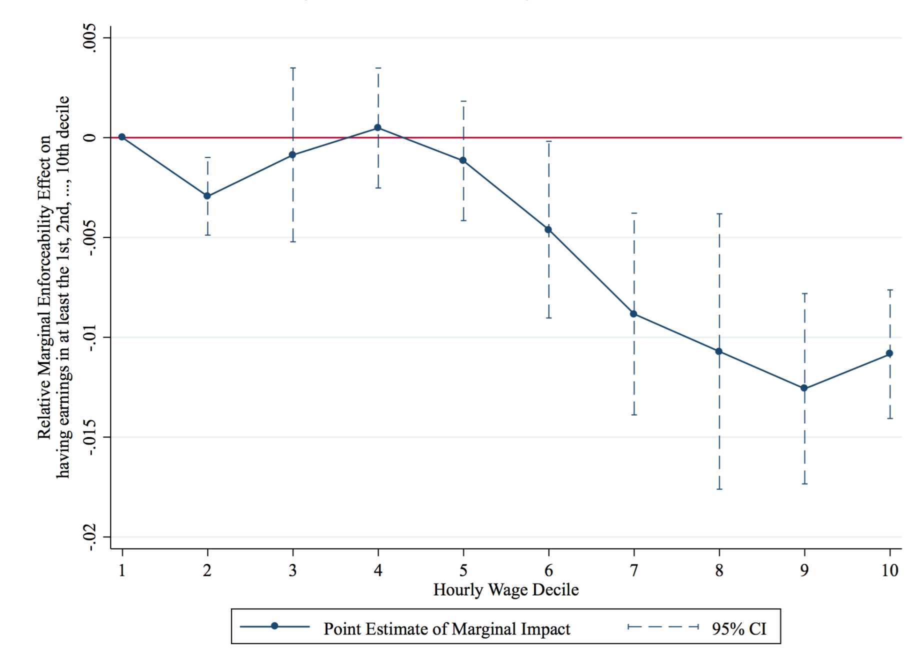


Figure 3: Marginal Effect of Consideration and Non-consideration Enforceability on Participating in at Least 1, 2, ... 10 Training Activities

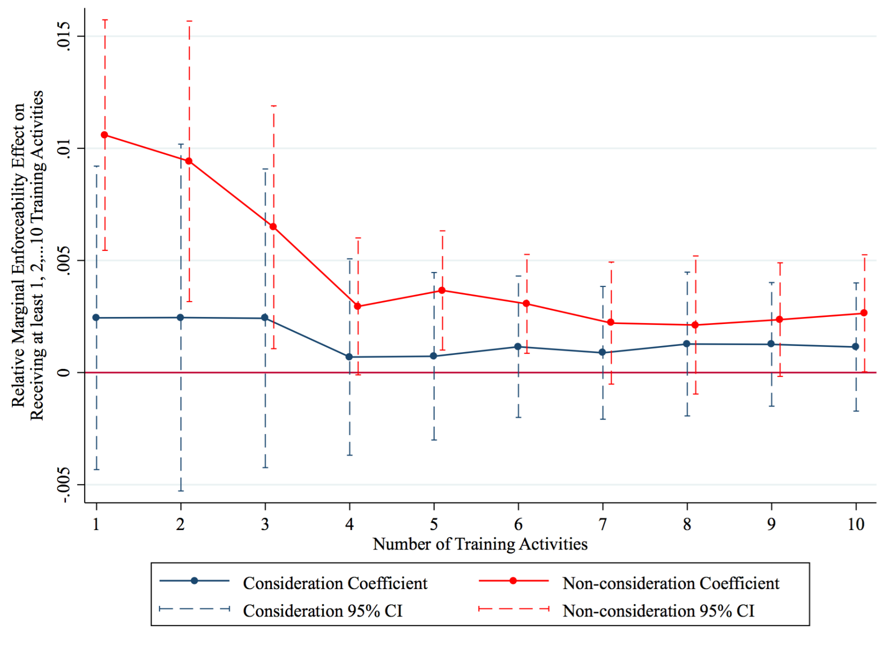
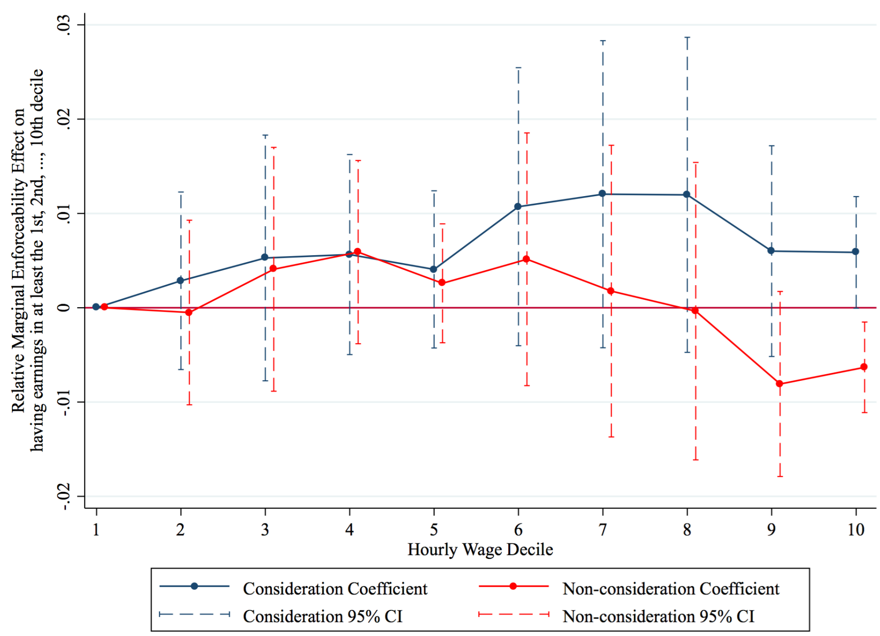


Figure 4: Marginal Effect of Consideration and Non-consideration Enforceability on having Earnings in at least the 1st, 2nd, ..., 10th Decile of the High-Use-Year Wage Distribution



Tables

Table 1: Noncompete Enforceability Index Weights

Question	1991 Mean	1991 SD	2009 Mean	2009 SD	Bishara Weight	Factor Analysis Weight
Statute of Enforceability	4.90	1.53	4.96	1.79	0.10	0.09
Protectable Interest	5.80	2.03	6.51	1.93	0.10	0.12
Plaintiff's Burden of Proof	5.36	2.06	5.59	1.93	0.10	0.10
Consideration At Inception	8.45	2.35	8.73	2.39	0.05	0.13
Consideration Post-Inception	7.04	2.78	7.15	2.86	0.05	0.08
Overbroad Contracts	5.59	3.17	5.83	2.91	0.05	0.04
Quit v. Fire	6.23	2.32	6.45	2.37	0.10	0.09
Constant						-4.23

Note: the index is normalized to have a mean of 0 and a standard deviation of 1 in a sample in which each state has a weight of 1.

Table 2: Incidence of Noncompetes by Occupation from Starr et al. (2017a)

Occupation	Incidence of Noncompetes
High-Use Occupations	
Architecture, Engeering	36%
Computer, Mathematical	35%
Management	30%
Healthcare Support	26%
Business, Finance	23%
Arts, Entertainment	22%
Life, Physical, and Social Science	21%
Physician, Technical	19%
Personal Care, Services	19%
Installation, Repair	18%
Low-Use Occupations	
Production Occupations	16%
Sales, Related	16%
Office, Support	14%
Transportation, Materials Moving	12%
Construction, Extraction	12%
Food Prep, Serving	11%
Grounds Maintenance	11%
Legal Occupations	10%
Farming, Fishing, and Forestry	6%
Overall	18.1%

Note: These estimates are directly reported from Figure A3 in the appendix of Starr et al. (2017a).

Table 3: Summary Statistics

	Low Use Occupations			High Use Occupations			Overall		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Panel A: Received training to improve skills?									
Received Training	0.16	0.36	44940	0.30	0.46	25434	0.21	0.41	70374
Panel B: How many training activities in the last year, conditional on receiving training?									
Number of Training Events	4.24	7.78	7048	4.55	7.60	7618	4.40	7.69	14666
Panel C: Who paid for the most recent training?									
Firm-sponsored	0.90	0.30	7048	0.89	0.31	7618	0.90	0.30	14666
Self-Sponsored	0.05	0.21	7048	0.07	0.25	7618	0.06	0.24	14666
Panel D: Where did the most recent firm-sponsored training event occur and who provided it?									
OTJ Co-worker Taught	0.49	0.50	7048	0.35	0.48	7618	0.42	0.49	14666
OTJ Outsider Taught	0.14	0.34	7048	0.16	0.37	7618	0.15	0.36	14666
Offsite	0.27	0.44	7048	0.36	0.48	7618	0.32	0.46	14666
Panel E: What was the firm-sponsored training content?									
Basic Skills	0.32	0.47	7048	0.25	0.43	7618	0.29	0.45	14666
New Skills	0.46	0.50	7048	0.43	0.49	7618	0.44	0.50	14666
Upgrading Skills	0.69	0.46	7048	0.74	0.44	7618	0.72	0.45	14666
Company Policies	0.26	0.44	7048	0.22	0.41	7618	0.24	0.43	14666
Panel F: Full Sample Characteristics									
Tenure (years)	6.46	7.0	44940	7.31	7.33	25434	6.76	7.19	70374
Age (years)	38.1	9.45	44940	39.05	8.97	25434	38.44	9.29	70374
Highest Degree: Bachelor's	0.11	0.31	44940	0.29	0.46	25434	0.18	0.38	70374
Highest Degree: Graduate	0.02	0.14	44940	0.12	0.32	25434	0.06	0.23	70374
Metro	0.78	0.42	44940	0.84	0.36	25434	0.80	0.40	70374
Male	0.55	0.50	44940	0.55	0.50	25434	0.55	0.50	70374
White	0.67	0.47	44940	0.77	0.42	25434	0.71	0.45	70374
Hours	39.86	9.86	44940	42.06	10.01	25434	40.65	9.97	70374
Unionized	0.12	0.32	44940	0.06	0.24	25434	0.10	0.30	70374
Hourly Wage	17.59	21.90	42071	28.29	42.62	24457	21.52	31.58	66528
Panel G: Measures of Noncompete Enforceability									
Overall Enforceability	-0.20	1.36	44940	-0.20	1.37	25434	-0.20	1.36	70374
Consideration Dimensions	0.22	1.26	44940	0.23	1.26	25434	0.22	1.26	70374
Non-consideration Dimensions	-0.22	1.24	44940	-0.21	1.25	25434	-0.21	1.25	70374

Note: Overall noncompete enforceability and the 'Consideration', and 'Non-consideration' variables use the weights developed in Table 1 and the scores from Bishara (2011) (two consideration dimensions for 'Consideration' and five remaining dimensions for 'Non-consideration'. The consideration measure is reverse coded such that higher scores reflect the adoption of additional consideration (which would be associated with reduced enforceability). Each measure is normalized to be mean zero, standard deviation of one in a sample in which each state is given equal weight.

Table 4: Training Results

Model: OLS	(1)	(2)	(3)	(4)	(5)
Panel A Dependent Variable: ***1****(Received training in last year?)*					
1(High-Use Occ)*Enforceability	0.0067***	0.0090***	0.0082***	0.0077***	
	(0.0020)	(0.0019)	(0.0019)	(0.0019)	
Enforceability	0.0077***	0.0021	0.0035**		
	(0.0019)	(0.0015)	(0.0016)		
1(High-Use Occ)	0.1443***	0.1082***			
	(0.0052)	(0.0048)			
R-squared	0.0297	0.0535	0.1024	0.1154	
Individual controls		X	X	X	
Occ-Ind-Year FE			X	X	
State-Year FE				X	
Panel B Dependent Variable: ***1****(Who paid? Where did the training occur? Who taught?)*					
	Who paid?		Where? Who taught?		
	Firm-Sponsored	Self-Sponsored	Onsite Co-worker	Onsite Outsider	Offsite
1(High-Use Occ)*Enforceability	0.0069***	0.0003	-0.0011	0.0025***	0.0058***
	(0.0021)	(0.0005)	(0.0010)	(0.0007)	(0.0011)
	.	.	.		
R-squared	0.1099	0.0461	0.0478	0.0379	0.0771
Individual controls	X	X	X	X	X
Occ-Ind-Year FE	X	X	X	X	X
State-Year FE	X	X	X	X	X
Panel C Dependent Variable: ***1****(What did the most recent training contain?)*					
	Basic	New	Upgrade	Policies	
1(High-Use Occ)*Enforceability	0.0033**	0.0042**	0.0049***	0.0020***	
	(0.0015)	(0.0016)	(0.0015)	(0.0006)	
	.	.			
R-squared	0.0385	0.0514	0.0917	0.0374	
Individual controls	X	X	X	X	
Occ-Ind-Year FE	X	X	X	X	
State-Year FE	X	X	X	X	
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. 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 omitted group is low-noncompete-use occupations. Individual controls consist of hours worked, a quadratic in age, and indicators for working in a metro area, bachelors degree, graduate school degree, male, white, and whether the worker is unionized.

Table 5: Is the Receipt of Training Associated with Lower Wages?

Dependent Variable: Log Hourly Wages				
Model: OLS	(1)	(2)	(3)	(4)
Sample	All	Tenure≤1	All	Tenure≤1
1(Received training)	0.1122***	0.1116***		
	(0.0080)	(0.0223)		
Log Number of Training Events			0.0673***	0.0749***
			(0.0044)	(0.0161)
Observations	66,528	7,628	66,528	7,628
R-squared	0.3965	0.3908	0.3960	0.3908
Individual Controls	X	X	X	X
Occupation-Industry-Year FE	X	X	X	X
State-Year FE	X	X	X	X

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses, clustered at the state level. The dependent variable is the log of hourly wages. Individual controls consist of hours worked, a quadratic in age, and indicators for working in a metro area, bachelors degree, graduate school degree, male, white, and whether the worker is unionized

Table 6: Mobility and Wage Results

Model: OLS	(1) Tenure	(2) Ln(Wage)	(3) **1**(Wage≥ 3rd Decile)	(4) **1**(Wage≥ 6th Decile)	(5) **1**(Wage≥ 10th Decile)
1(High-Use Occ)*Enforceability	0.1418***	-0.0099***	-0.0009	-0.0046**	-0.0108***
	(0.0368)	(0.0028)	(0.0022)	(0.0022)	(0.0016)
Observations	70,374	66,528	66,528	66,528	66,528
R-squared	0.2790	0.3909	0.2889	0.3476	0.1662
Individual controls	X	X	X	X	X
Occ-Ind-Year FE	X	X	X	X	X
State-Year FE	X	X	X	X	X

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses, clustered at the state level. Columns (2) through (4) use indicators for earning at least 10, 25, or 40 dollars per hour. The omitted group is low-noncompete incidence occupations. Individual controls consist of hours worked, a quadratic in age, and indicators for working in a metro area, bachelors degree, graduate school degree, male, white, and whether the worker is unionized.

Table 7: Consideration and Non-Consideration Components of Enforceability

Model: OLS	(1)	(2)	(3)	(4)	(5)
	1(Training)	Ln(Wage)	**1**(Wage≥ 3rd Decile)	**1**(Wage≥ 6th Decile)	**1**(Wage≥ 10th Decile)
1(High-Use Occ)*Consideration	0.0029	0.0111*	0.0053	0.0107	0.0059*
	(0.0034)	(0.0058)	(0.0065)	(0.0073)	(0.0029)
1(High-Use Occ)*Non-consideration	0.0112***	-0.0003	0.0041	0.0051	-0.0063**
	(0.0027)	(0.0045)	(0.0064)	(0.0067)	(0.0024)
Observations	70,374	66,528	66,528	66,528	66,528
R-squared	0.1155	0.3909	0.2889	0.3476	0.1662
Individual controls	X	X	X	X	X
Occ-Ind-Year FE	X	X	X	X	X
State-Year FE	X	X	X	X	X

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 indicated in the column heading. Individual controls consist of hours worked, a quadratic in age, and indicators for working in a metro area, bachelors degree, graduate school degree, male, white, and whether the worker is unionized.

Table 8: Selection on Unobservables

Model: OLS	(1)	(2)	(3)	(4)
	1(Training)	**1**(Training)	Ln(Wage)	Ln(Wage)
Panel A: Including additional, interacted state-level controls				
1(High-Use Occ)*Enforceability	0.0084*		-0.0130**	
	(0.0049)		(0.0049)	
1(High-Use Occ)*Consideration		0.0005		0.0117**
		(0.0039)		(0.0056)
1(High-Use Occ)*Non-consideration		0.0097***		-0.0031
		(0.0030)		(0.0052)
R-squared	0.1157	0.1157	0.3910	0.3910
Interacted State-Level controls	X	X	X	X
State-Year FE	X	X	X	X
Panel B: Using state-industry-year fixed effects				
1(High-Use Occ)*Enforceability	0.0065***		-0.0117***	
	(0.0020)		(0.0037)	
1(High-Use Occ)*Consideration		0.0016		0.0136**
		(0.0035)		(0.0066)
1(High-Use Occ)*Non-consideration		0.0086***		0.0001
		(0.0027)		(0.0053)
R-squared	0.1517	0.1518	0.4194	0.4194
Interacted State-Level controls				
State-Industry-Year FE	X	X	X	X
Panel C: Sensitivity to selection on unobservables				
1(High-Use Occ)*Enforceability	0.0078		-0.0138**	
	(0.0050)		(0.0055)	
	[1.3000]		[2.2368]	
1(High-Use Occ)*Consideration		-0.0006		0.0142**
		(0.0042)		(0.0068)
		[0.0541]		[0.7769]
1(High-Use Occ)*Non-consideration		0.0080**		-0.0016
		(0.0034)		(0.0061)
		[0.5536]		[0.0564]
R-squared	0.1519	0.1519	0.4195	0.4195
Interacted State-Level controls	X	X	X	X
State-Industry-Year FE	X	X	X	X

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses, clustered at the state level. All models include Occupation by industry by year fixed effects and the set of individual controls. The dependent variable is highlighted in the column heading. Panel A includes indicators for the three exceptions to at-will employment (Autor et al., 2006), and whether the state is a right-to-work state, and all such variables are interacted with the high-use occupation indicator. Panel B uses state by industry by year fixed effects, as opposed to just state by year fixed effects. Panel C includes all controls and the Oster (2017) δ term is presented in brackets. Per Oster's recommendation, the maximum R^2 is set to 30% higher than the observed R^2 in the most saturated model (those in this table). The baseline set of controls include state-year fixed effects, the individual controls, and a high-noncompete-use dummy.

Table 9: Different Measures of Noncompete Incidence and Enforceability

Model: OLS	(1)	(2)	(3)	(4)
	1(Training)	**1**(Training)	Ln(Wage)	Ln(Wage)
Panel A: Continuous Measure of Noncompete Incidence				
Incidence*Enforceability	0.0416***		-0.0732***	
	(0.0112)		(0.0161)	
Incidence*Consideration		0.0669***		0.1347***
		(0.0211)		(0.0413)
Incidence*Non-consideration		0.1089***		0.0475
		(0.0178)		(0.0355)
R-squared	0.1139	0.1140	0.3768	0.3769
Individual Controls	X	X	X	X
Occupation-Industry-Year FE	X	X	X	X
State-Year FE	X	X	X	X
Panel B: Using rank of enforceability instead of enforceability score				
1(High-Use Occ)*Rank(Enforceability)	0.0005		-0.0006	
	(0.0004)		(0.0005)	
1(High-Use Occ)*Rank(Consideration)		0.0001		0.0008**
		(0.0003)		(0.0003)
1(High-Use Occ)*Rank(Non-consideration)		0.0007***		0.0000
		(0.0002)		(0.0003)
R-squared	0.1154	0.1154	0.3908	0.3909
Individual Controls	X	X	X	X
Occupation-Industry-Year FE	X	X	X	X
State-Year FE	X	X	X	X

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses, clustered at the state level. Panel A uses a continuous measure of the use of noncompetes by occupation (as shown in Table 2), as opposed to the binary, high/low variable. Panel B uses the rank of the state within the enforceability spectrum instead of the actual enforceability score. All models include the original control variables and set of fixed effects (except Panel B, as noted).

Online Appendix

A Additional Figures

Figure A1: Marginal Effect of Rank(Noncompete Enforceability) on Participating in at Least 1, 2, ... 10 Training Activities

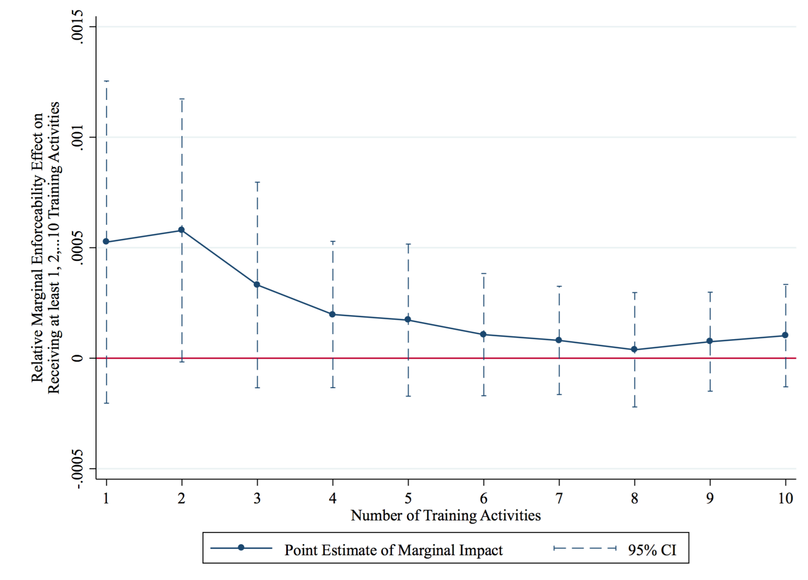


Figure A2: Marginal Effect of Rank(Noncompete Enforceability) on having Earnings in at least the 1st, 2nd, ..., 10th Decile of the High-Use-Year Wage Distribution

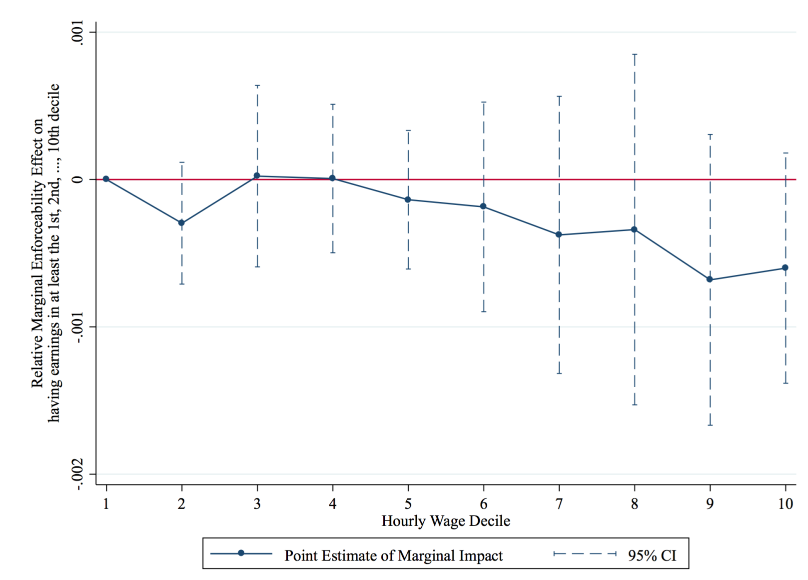


Figure A3: Marginal Effect of Rank(Consideration) and Rank(Non-consideration) Enforceability on Participating in at Least 1, 2, ... 10 Training Activities

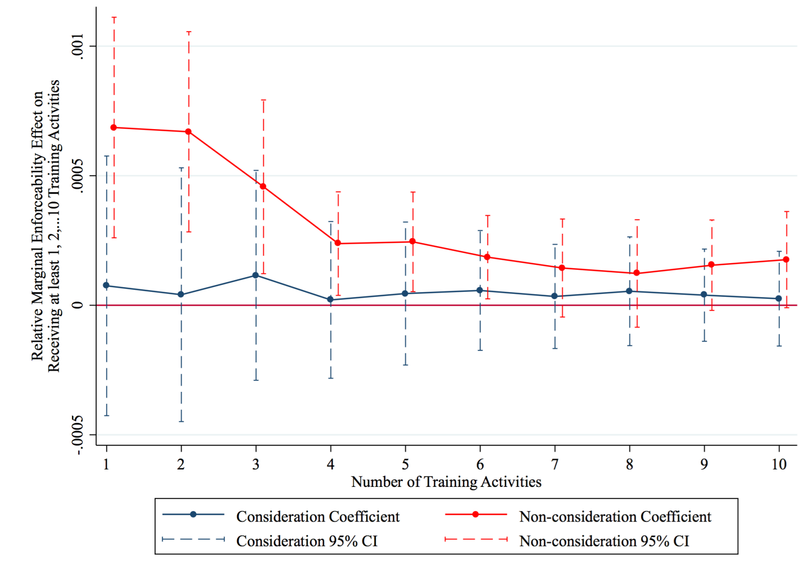
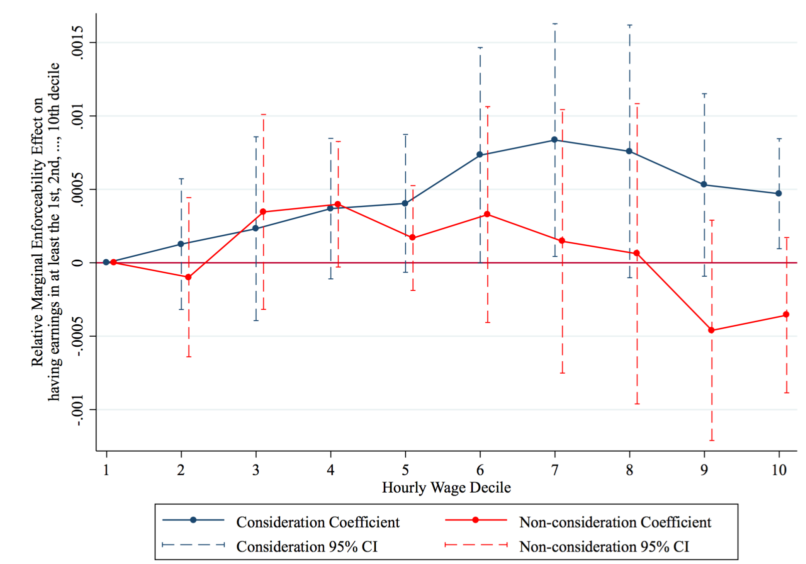


Figure A4: Marginal Effect of Rank(Consideration) and Rank(Non-consideration) Enforceability on having Earnings in at least the 1st, 2nd, ..., 10th Decile of the High-Use-Year Wage Distribution



B Enforceability Indices

B.1 Factor Analysis Index

Table B1: Factor Analysis Weighted Noncompete Index

State	1991	2009	State	1991	2009
AK	-1.33	-0.98	MS	-0.20	0.04
AL	0.36	0.36	MT	-0.63	-0.65
AR	-0.62	-0.58	NC	0.18	0.18
AZ	-0.16	0.15	ND	-4.23	-4.23
CA	-3.76	-3.79	NE	-0.13	-0.13
CO	0.38	0.38	NH	0.26	0.26
CT	0.62	1.26	NJ	0.47	0.90
DC	0.12	0.12	NM	0.74	0.74
DE	0.18	0.52	NV	-0.62	0.03
FL	1.15	1.60	NY	-0.73	-1.15
GA	0.45	0.02	OH	-0.18	0.08
HI	-0.83	-0.17	OK	-0.80	-0.94
IA	0.19	1.01	OR	0.14	0.14
ID	-0.01	0.77	PA	-0.14	0.14
IL	0.55	0.95	RI	-0.67	-0.33
IN	0.70	0.70	SC	-0.20	-0.27
KS	0.69	1.21	SD	0.37	1.02
KY	0.61	0.85	TN	0.22	0.45
LA	-0.70	0.50	TX	-0.04	-0.28
MA	0.87	0.48	UT	1.00	1.00
MD	0.15	0.60	VA	0.09	-0.29
ME	0.06	0.41	VT	0.30	0.60
MI	0.07	0.46	WA	0.64	0.34
MN	-0.07	-0.07	WI	0.16	-0.09
MO	0.93	1.08	WV	-0.80	-0.80
			WY	-0.65	0.23

This table presents the 1991 and 2009 noncompete enforceability scores for each state, where the weights for the seven dimensions of enforceability are determined via factor analysis, as shown in Table 1, and the scores are from Bishara (2011).

B.2 Bishara 2011 Index

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

Source: Bishara (2011).

B.3 Map of Noncompete Enforceability in 1991

Figure B1: Geography of Noncompete Enforceability in 1991