

# Noncompetes in the U.S. Labor Force<sup>\*†</sup>

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## Abstract

Using nationally representative survey data on 11,505 labor force participants, we examine the use, implementation, and effects of noncompete agreements. Nearly 1 in 5 labor force participants were bound by noncompetes in 2014, and nearly 40% had signed at least one in the past. Noncompetes are more likely to be found in high-skill, high-paying jobs, but they are also surprisingly common in low-skill, low-paying jobs. We document that less than 10% of employees negotiate over noncompetes, that roughly one-third of noncompetes are signed after accepting the job offer, and that nearly two-thirds of job applicants had no alternative job opportunities when they were asked to agree to a noncompete. Differences in the competitive circumstances under which noncompetes are signed are associated with starkly different outcomes for employees: those presented with a noncompete before they accept a job offer and those who have alternative employment options earn 19% higher wages, receive 14% more training, and are 13% more satisfied in their job than those not bound by noncompetes. However, those asked to sign after accepting an offer and/or without other employment options are 15% less satisfied in their job and experience no wage and training benefits. In contrast to the existing literature, we find little role for the enforceability of noncompetes in explaining their use and their association with wages and training.

**Keywords:** covenants not to compete, monopsony power, employment law, training, wages, job satisfaction

**JEL Codes:** J4, J6, K31, L41, M5

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<sup>†</sup>Results from early versions of this paper were discussed in the US Treasury report on noncompetes ([Treasury, 2016](#)), as well as the subsequent White House report ([WhiteHouse, 2016](#)).

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# 1 Introduction

*“...for 18 months after the Separation Date, Employee will not ...engage in or support the development, manufacture, marketing, or sale of any product or service that competes or is intended to compete with any product or service sold, offered, or otherwise provided by Amazon ...”*

- Amazon noncompete agreement, signed by seasonally employed warehouse “packers”

The potential importance of “monopsony power”—the firm’s ability to set wages—in limiting wage and employment growth has provoked much research examining frictions that reduce labor market competition (Ashenfelter et al., 2010; Manning, 2011; Boal and Ransom, 1997; Furman, 2016), including search and moving costs (Burdett and Mortensen, 1998; Fox, 2010), recruitment restrictions (Naidu, 2010), the “thinness” of labor markets (Ransom, 1993; Lazear, 2009), and employer collusion (Krueger and Ashenfelter, 2017). In this paper, we investigate the use and effects of a particularly anticompetitive management tool: covenants not to compete (“noncompetes”), which prohibit workers from joining or starting a competing firm within particular geographic and time boundaries.<sup>1</sup> Noncompetes have long been the subject of significant debate because the prohibition of within-industry mobility likely has pervasive effects on a variety of economic outcomes,<sup>2</sup> including employee mobility, entrepreneurship, wages, innovation, human capital investment, and firm profitability.<sup>3</sup>

The foundation of this longstanding debate can be traced to different fundamental assumptions regarding private contracting behavior in labor markets.<sup>4</sup> Pro-noncompete advocates argue that competition among sophisticated parties ensures that noncompetes are signed only when they are mutually beneficial (Callahan, 1985), and that noncompetes are necessary for firm-sponsored investment in valuable information or human capital when workers are credit constrained (Rubin

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<sup>1</sup>See Appendix C for two examples of actual noncompetes.

<sup>2</sup>See Treasury (2016) for an up-to-date discussion. Numerous states including Alabama, Arkansas, Hawaii, Idaho, Maryland, Massachusetts, Michigan, Missouri, New Mexico, New York, Oregon, Pennsylvania, Rhode Island, Utah, Washington, and Wisconsin have recently passed or proposed reforms to their noncompete laws. Federal noncompete legislation, the MOVE Act, was also proposed in June of 2015 to ban noncompetes for low-wage employees. See <http://faircompetitionlaw.com/the-changing-landscape-of-trade-secrets-laws-and-noncompete-laws/> for more details.

<sup>3</sup>Regarding employee mobility, see Fallick et al. (2006); Marx et al. (2009); Marx (2011). Regarding entrepreneurship, see Stuart and Sorenson (2003); Starr et al. (2017). Regarding wages, see Lavetti et al. (2014); Garmaise (2009); Starr (2016). Regarding innovation, see (Gilson, 1999; Samila and Sorenson, 2011; Conti, 2014; Belenzon and Schankerman, 2013). Regarding human capital investment, see (Rubin and Shedd, 1981; Starr, 2016). Regarding firm profitability, see Younge and Marx (2013).

<sup>4</sup>See Blake (1960) and Garrison and Wendt (2008) for a review of the history of noncompete law and the evolution of this debate.

and Shedd, 1981; Posner et al., 2004), or to supplement trade secret protections.<sup>5</sup> By contrast, those opposed to noncompetes counter that imperfect markets and inequities in bargaining power allow firms to impose noncompetes on unsuspecting workers without appropriate or fair consideration (Stone, 2002; Arnow-Richman, 2006), preventing them from later taking better jobs in their chosen field (Marx, 2011). Among the other arguments they make are that noncompetes also reduce self-sponsored training (Garmaise, 2009; Lobel and Amir, 2013) and suppress innovation due to reduced knowledge flows (Gilson, 1999; Hyde, 2003; Lobel, 2013).

Recent discoveries of sweeping noncompetes signed by temporarily employed Amazon packers (Woodman, 2015) and minimum wage sandwich makers (Jamieson, 2014) have bolstered the anti-noncompete position and fueled widespread speculation about the ubiquity and effects of noncompetes on the upward mobility of workers (Greenhouse, 2014). Yet while compelling logic or anecdotes that support both the pro- and anti-noncompete rationales are easy to find, little evidence exists to adjudicate between these positions because data on the use and prevalence of noncompetes across the U.S. labor force are themselves non-existent.<sup>6</sup> In what follows, we use data from a nationally representative survey that we developed and implemented to present the first comprehensive picture of noncompete activity in U.S. labor markets, unpack the various drivers behind their use and effects, and test the hypotheses that emerge from our analysis.

We begin by studying why employers may seek to enter into noncompetes, decomposing employer aims into the following discrete, profit-maximizing objectives: depressing wage growth, enhancing productivity through training and information sharing, reducing turnover costs, and curtailing product-market competition by preventing valuable information and skills from reaching competitors. Our analysis highlights how the initial conditions under which employers offer noncompetes influence whether employees are likely to benefit from the agreements. In theory,

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<sup>5</sup>Relative to other knowledge protections such as confidentiality agreements, nonsolicitation agreements, or trade secret protections, noncompetes are supposedly preferred by firms because their breach is readily observable (Garrison and Wendt, 2008).

<sup>6</sup>Currently available data on the use of noncompetes are limited to C-suite executives (Bishara et al., 2012; Garmaise, 2009), physicians (Lavetti et al., 2014), and electronics engineers (Marx, 2011), which together represent less than 3% of the U.S. Labor Force. A very recent working paper examines noncompetes among hair stylists (Johnson and Lipsitz, 2017). See Bishara and Starr (2016) for a review of the nascent empirical noncompete literature. The existing work on the use of noncompetes points to positive wage effects for physicians (Lavetti et al., 2014) but involuntary career detours for electronics engineers (Marx, 2011). There is a much larger literature on the effects of the “enforceability” of noncompetes, but there are significant theoretical and practical differences between the use of a contractual provision and the potential enforceability of that provision. We discuss the role noncompete enforceability plays in our analysis in depth later in this article.

competitive labor markets should fully compensate employees for giving up their right to compete. If, however, labor markets are imperfect, or employees exhibit various behavioral biases, then firms may be able to implement noncompetes without providing any sort of compensating differential. Employees who are asked to agree to a noncompete after accepting the employer’s offer and who have no alternative options at the time are especially susceptible. With this theoretical backdrop, we characterize the use of noncompetes across types of employees and employers, describe key aspects of the signing process, and exploit the extent to which the initial conditions match the assumptions underlying the competitive framework to provide evidence on the effects of noncompetes on wages, training, and job-satisfaction.<sup>7</sup>

We find that noncompetes are a surprisingly common part of the employment relationship: In 2014, 38.1% of employees had signed a noncompete at some point in their lives, while 18.1% of those in the U.S. labor force (roughly 28 million) were working under one. Noncompetes are more likely to be found in high-skill, high-paying jobs that involve trade secrets, but they are still common in low-skill, low-paying jobs that do not involve trade secrets. For example, we find that 12% of those without a bachelor’s degree and earning less than \$40,000 a year sign noncompetes. We also observe significant heterogeneity in the circumstances under which employees enter into noncompetes. Roughly 1/3 of noncompetes are signed *after* the individual has accepted an employment offer,<sup>8</sup> and only 1/3 of signing individuals had an alternative employment opportunity at the time they were asked to sign. We find that only 10% of individuals report negotiating over noncompetes, and that most individuals simply agree to sign without consulting friends, family, or legal counsel. Finally, we establish that noncompetes are nearly as common in states that do not enforce them as they are in states that enforce them vigorously, raising questions about why employers use unenforceable noncompetes and whether such unenforceable provisions affect employee behavior.

To understand the consequences of noncompetes, we examine wage, training, information sharing, and job-satisfaction outcomes, allowing the effect of a noncompete to vary based on the extent to which the initial conditions under which the noncompete is signed – full information

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<sup>7</sup>In our companion paper, [Starr et al. \(2016\)](#), we investigate how noncompetes affect the process of employee mobility.

<sup>8</sup>This number is quite similar to [Marx \(2011\)](#)’s study of engineers, which offers the only other evidence of this type of potentially strategic delay.

and multiple alternative job options – reflect the competitive model. The results offer important insights into the literature’s competing perspectives, and suggest the existence of a more complex relationship between noncompetes and labor market competitiveness generally.

To begin with, noncompetition agreements signed under competitive labor market conditions appear to improve outcomes for employees. Empirical analysis and direct survey evidence show that those who are asked to sign noncompetes before they have accepted an offer and/or who have alternative employment options are significantly better off than those who are not bound by a noncompete: they earn 19% higher wages, are 6.9 percentage points more likely to have received training in the last year (14% increase relative to nonsigners), are 5.5 percentage points more likely to agree that their employer shares all relevant information with them (an 10.1% increase relative to nonsigners), and are 8.7 percentage points more likely to be satisfied in their job (a 13% increase relative to nonsigners).

By contrast, noncompetes signed in less than perfectly competitive conditions are associated with worse outcomes for affected employees. Specifically, those who are asked to sign noncompetes after they have already accepted their job offer and/or who have no alternative employment options when they are asked to sign are worse off relative to those not bound by noncompetes: our estimates indicate that they receive no wage or training benefits, that they are 14 percentage points less likely to have job relevant information shared with them (a 26% decrease relative to nonsigners) and are 10.4 percentage points less likely to be satisfied with their employment (a 15% decrease). Our detailed survey data allow us to saturate our empirical models with a wide variety of potentially confounding controls, and diagnostic tests (Oster, 2017) indicate that unobserved heterogeneity must be implausibly strong to account for our results.

Our findings are directly relevant to the literature on legal restrictions on employee mobility (Png, 2012; Png and Samila, 2015; Hsu et al., 2015; Naidu and Yuchtman, 2013). First, we present the first evidence that noncompetes are pervasive in the labor force, even among low-skilled workers. Second, we show that the effects of noncompetes on training, wages, and job satisfaction are not only heterogeneous, but point in opposite directions depending on how and when employers deploy these provisions.<sup>9</sup> Lastly, our work suggests that the literature’s emphasis

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<sup>9</sup>Specifically, noncompetes appear to improve incentives to train and to enhance wages (Lavetti et al., 2014; Rubin and Shedd, 1981; Callahan, 1985), but only when they are presented to potential employees before employment commences and/or when these candidates have alternative employment opportunities. Additional training or

on noncompete enforceability should at a minimum be supplemented with careful attention to noncompete “use,” and in particular, the contracting process, including notice and timing.<sup>10</sup>

More broadly, this study links the literatures on management practices (Bloom and Reenen, 2011; Shaw, 2004), monopsony power (Manning, 2003), and employment contracting (MacLeod, 2010). Noncompetes are different from many labor market frictions, such as search costs, because agreements are by definition “voluntary,” at least in theory. Markets are imperfect, however, and employers in our data appear to have the power to dictate terms under particular circumstances, as well as manipulate those circumstances to their advantage, often offering nothing in exchange for employees giving up their right to compete. When individuals are imperfectly informed, lack outside options, or are subject to behavioral biases, firms may likewise be in a position to freely deploy other restrictive clauses, such as arbitration agreements, IP preassignment agreements, or class-action waivers. A combination of strategic management practices, imperfect labor markets, and behavioral biases may thus prevent the minority of “term-conscious” labor market participants from disciplining employment contracting practices for all (Salop and Stiglitz, 1977). If so, such practices may further exacerbate frictions in the labor market, with subsequent implications for earnings (Topel and Ward, 1992), economic dynamism (Decker et al., 2015), innovation (Bloom et al., 2013; Tambe and Hitt, 2014; Almeida and Kogut, 1999), regional growth (Samila and Sorenson, 2011), and labor policy (Babcock et al., 2012).

## 2 Conceptual Framework

In this section, we leverage existing positions on noncompetes to sketch a basic, two-period employment contracting model and analyze its implications. If we assume employees are endowed with the right to leave their current employer at any time for a competitor, then a noncompete agreement represents the *partial* transfer of that right from the employee to the current employer.

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higher wages are unnecessary as inducements if a firm is able “hold up” an employee who is already committed to the employer or who has no other employment options. These results suggest that policy reform proposals calling for firms to offer noncompetes before employment commences, or with some consideration if presented after employment commences, would improve employee welfare (WhiteHouse, 2016).

<sup>10</sup>Much of the noncompete reform debate revolves around whether states should adopt a California-like approach of refusing to enforce noncompetes in court (Lobel, 2013). See Bishara and Starr (2016) and Barnett and Sichelman (2016) for critiques of the empirical noncompete literature, including Marx et al. (2015, 2009); Garmaise (2009) on employee mobility, Stuart and Sorenson (2003); Samila and Sorenson (2011); Starr et al. (2017) on new venture formation, Conti (2014) on the riskiness of firm R&D, Starr (2016) on firm-sponsored training, and Younge and Marx (2013) and Younge et al. (2014) on acquisitions and firm value.

The goal of the model is to understand the conditions under which a current or potential employer will value this right and when and for what price an employee will be willing to part with it.

## 2.1 Employer Motivations for Noncompetes

Consider a two-period incomplete contracting model in which an employer offers a wage contract to a potential employee in period one. In period two, absent a contrary agreement, the employee has the right to leave for a competitor, which might harm the initial employer. The employer has the option in period one to include a noncompete as part of the employment offer as well as the power to offer (or modify) other terms of the employment relationship (e.g., wages, training).

The expected value of hiring the employee is given by

$$J(w_1, \alpha) = \underbrace{(y_1 - w_1 - c_1)}_{\text{period one profit}} + \underbrace{\beta_f [\alpha(y_2 - w_2^s) + (1 - \alpha)(-D - v)]}_{\text{discounted, expected period two profit}}$$

where  $y_t$  is productivity in period  $t \in \{1, 2\}$ ,  $w_1$  represents the first-period wage,  $c_1$  is the cost of training or other investments (which occur in period one),  $\beta_f$  is the employer's discount rate,  $\alpha$  is the probability the individual stays in period two,  $w_2^s$  is the second-period wage if the employee remains,  $D$  is the damage done to the employer if the individual leaves (e.g., by taking clients or by otherwise increasing competitive pressure through reduced demand), and  $v$  is the cost of filling the vacant position. If the employer chooses not to hire, it pays a vacancy cost  $v$  and hires an average employee next period:  $V = -v + \beta_f(y_1 - w_1)$ .

A noncompete enters the model through  $\alpha$  by imposing an additional cost on an employee for starting or joining a competitor—including litigation costs, search costs, or even the cost of waiting out the noncompete. To endogenize  $\alpha$ , suppose that in period two the employee receives a wage draw  $\hat{w}_2 \sim U[0, 1]$  and considers leaving his period-one employer. If the employee switches employers, he incurs a switching cost  $s$ . The employee stays if  $w_2^s > \hat{w}_2 - s$ , and thus the probability of staying is given by  $\alpha = P(\hat{w}_2 < w_2^s + s) = w_2^s + s$ . The noncompete adds costs to the movement between competitors such that under a noncompete switching costs are  $s' > s$ . Define  $\alpha' \equiv w_2^s + s' > \alpha$ . The employer offers a noncompete if  $J(w_1, \alpha') > J(w_1, \alpha)$ .

Although very general, this framework is capable of illuminating the conditions which increases in  $\alpha$  can enhance an employer's valuation of a particular employment contract. Totally

differentiating  $J(w_1, \alpha)$  with respect to  $\alpha$  gives:

$$\frac{J(w_1, \alpha)}{d\alpha} = \frac{d(y_1 - w_1 - c_1)}{d\alpha} + \beta_f \left[ \left( y_2 - w_2^s + D + v \right) + \alpha \left( \frac{d(y_2 - w_2^s)}{d\alpha} \right) + (1 - \alpha) \left( - \frac{d(D + v)}{d\alpha} \right) \right].$$

This expression clarifies the discrete reasons employers have to seek noncompetes (or other mobility constraints generally) in their contracts.

Most intuitively, an increase in  $\alpha$  has the direct effect of making it more likely that the employee will stay in the second period and hence that the employer will enjoy period two profits  $y_2 - w_2^s$  (if any). The noncompete also makes it less likely the employer will suffer “damage” or sustain vacancy costs,  $D + v$ , as a result of the employee’s departure to a competitor. In other words, for employees who generate large period two profits, possess damaging information or training, or are costly to replace, the employer has greater incentives to seek a noncompete.

Just as importantly, there are at least three indirect effects of increasing  $\alpha$  that may alter whether an employer finds noncompetes attractive. First, the term  $\frac{d(y_2 - w_2^s)}{d\alpha}$  seems likely, at least at first blush, to be positive. If noncompetes allow employers to ignore offers from competitors during the second period wage-setting process, period two wages will be lower under a noncompete. Note, however, that because noncompetes offer only protection from competitors, those bound by a noncompete may strategically redirect their job search efforts to noncompetitors outside the umbrella of the noncompete (Starr et al., 2016). Furthermore, if noncompetes create a wedge between the internal and external markets (Acemoglu and Pischke, 1999), employers will have greater incentives to invest in training, leading to higher employee productivity (Meccheri, 2009). On the other hand, if noncompetes temper employee incentives to invest in their own training or otherwise exert effort on the job, the term  $\frac{d(y_2 - w_2^s)}{d\alpha}$  can be negative (Garmaise, 2009). These effects of noncompetes on training and effort may also influence  $w_2^s$  directly if an employee’s wages are tied to productivity.

Second, noncompetes may indirectly affect the damage and vacancy cost term,  $-\frac{d(D+v)}{d\alpha}$ . On balance, any such effect seems likely to cut against an employer finding the use of noncompetes to be appealing. If an employer would otherwise provide additional training to an employee as a result of a noncompete, but this training would also furnish the employee with more power to cripple the employer upon departure (e.g., because noncompetes are incomplete and enforcement



can be difficult), then this latter offsetting effect may reduce the incentive to invest and thus lessen the value of noncompetes to the employer. Relatedly, noncompetes may increase vacancy costs if potential job candidates find noncompetes unappealing.

Third, and finally, increases in  $\alpha$  may indirectly affect period one profitability by altering the wages that the employer must offer in that period in order to induce the employee to assent to an employment contract that includes a noncompete,  $\frac{d(y_1 - w_1 - c_1)}{d\alpha}$ . If an employer must directly compensate an employee for accepting a noncompete, the net value of a noncompete's effects on second period profits is smaller. Assuming that a potential new hire's initial productivity  $y_1$  is unaffected by  $\alpha$  (which may not be true, as a noncompete may alter period one effort, see [Lobel and Amir \(2013\)](#)), and assuming the direct cost of writing and implementing a noncompete,  $\frac{dc_1}{d\alpha}$ , is relatively small,<sup>11</sup> the most important indirect effect in the first period is  $\frac{dw_1}{d\alpha}$ : Will workers demand higher initial wages in order to agree to a noncompete?

## 2.2 Imperfect Labor Markets and the Importance of Initial Conditions

In a perfectly competitive labor market, employer competition for employees ensures that any surplus is transferred to employees and therefore that  $J(w_1, \alpha) = 0$  ([Callahan, 1985](#)). Therefore, if noncompetes produce value by encouraging socially efficient investment in training and information dissemination,<sup>12</sup> even a reasonably competitive labor market will require that employees who agree to a noncompete share in that surplus. Compensating differentials may take the form of higher wages, access to valuable training, information, experiences, other benefits, or all of the above. If wages and training are fully contractible, the employee will demand the wage-training combination on the firm's zero-profit curve that maximizes his utility.

Sophisticated parties negotiating in perfectly competitive labor markets is the standard contracting account proffered by pro-noncompete enthusiasts.<sup>13</sup> However, not all parties are sophisticated, and even sophisticated parties may agree to noncompetes in exchange for little or even

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<sup>11</sup>If noncompetes lead an employer to invest more in training in period one, then the costs of investment  $c_1$  will also rise with  $\alpha$ , though this may also be shared with the worker through changes in  $w_1$ .

<sup>12</sup>Noncompetes make sense when employees are credit constrained and wages cannot be reduced by enough to compensate an employer for providing valuable training or sharing information ([Rubin and Shedd, 1981](#)). As an alternative, the firm requires that the employee promise not to compete, which allows the employer to benefit from the training through lower wages and higher productivity for a longer period. An employee is willing to accept this noncompete if the training or information (and other terms) are sufficiently valuable.

<sup>13</sup>There are other perspectives as well, such as firms using noncompetes as a screening mechanism to distinguish between 'stayers' and 'leavers.' We provide direct empirical evidence in [Section 4](#) that this is not the case.

nothing in return. We describe some of the reasons this may occur below. In so doing, we recognize that once a noncompete is agreed to, the bargaining power in any subsequent negotiation between the worker and firm shifts strongly towards the firm ([Postel-Vinay and Robin, 2004](#)), since the firm possesses the right to prevent the worker from joining a competitor. Thus, the initial conditions under which the noncompete is signed will be paramount to understanding when and why noncompetes operate the way they do ([Beaudry and DiNardo, 1991](#)). In particular, initial conditions that reduce the individual’s willingness or ability to negotiate over that right will result in little, if any, compensating differential .

A simple behavioral reason employees may not receive compensation for agreeing to a noncompete is that they do not read the contract they are offered—a phenomenon known in consumer law as the “no-reading problem” ([Marotta-Wurgler, 2011](#))—and therefore are more likely to agree to unfavorable terms. [Salop and Stiglitz \(1977\)](#) show that in a setting where information gathering is costly, a sufficiently large informed minority of “term-conscious buyers” can discipline sellers from raising prices, but if there are not enough informed individuals or “sellers” can price discriminate, the price may rise to the monopoly level. Recent research in consumer law makes clear just how few individuals actually read their contracts ([Bakos et al., 2014](#)) and show how this limits the ability of competition to improve contract quality ([Ayres and Schwartz, 2014](#)). While individuals have strong incentives to understand the terms and conditions of their employment, many will not read the fine print.<sup>14</sup> Consequently, employees can become bound by noncompetes (and various other provisions) without necessarily knowing or receiving compensation.

Employees may also receive less than full compensation for agreeing to a noncompete if they do not have a sufficiently attractive outside employment option when they receive notice of the noncompete—i.e., search costs can cause labor markets to be less than fully competitive. For example, in a standard search model ([Burdett and Mortensen, 1998](#)), individuals develop a reservation wage and move when they receive an offer above that wage. In this scenario, an employee is willing to accept a job offer with a noncompete when he expects to be better off than he was, but the compensating differential will be lower than the competitive case.

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<sup>14</sup>Moreover, even if employees read their contract, they may not understand what they read. We are unaware of any work testing for understanding in the noncompete context—though in piloting our survey we often found individuals confused noncompetes for nondisclosure agreements and vice versa—but in the employment-at-will context, [Kim \(1997\)](#) shows that employees are very misinformed about their contractual rights.

Compensating differentials may also be too low (or nonexistent) relative to competitive labor market predictions if employees tend to have time-inconsistent (Laibson, 1997) or present-biased (O'Donoghue and Rabin, 1999) preferences. A noncompete only operates as a constraint, if at all, at some point in the post-termination future whereas higher wages and additional training would be received earlier in time. Loss aversion (Kahneman et al., 1990) will also make it more likely that employees who have already accepted an offer when they are asked to sign a noncompete will agree to it without demanding higher wages or more training. In particular, if starting a new job coincides with a change of the individual's reference point, the employee's willingness to switch to an alternative employer is lower than pre-acceptance. Accordingly, the employee will be more willing to sign a noncompete, especially if the individual believes that he might lose his job otherwise. These behavioral tendencies are likely magnified when alternative employment options are limited. Our survey data indicate that individuals felt they "needed the job" or that if they refused to sign a requested noncompete they would be "immediately terminated."

Importantly, employers can often manufacture conditions that result in less-than-full compensation for agreeing to a noncompete. That is, monopsony power may be reflected not only the ability to set wages, but also to determine the circumstances under which any negotiations take place. In practice, the law usually does not prevent an employer from delaying notifying an employee of its desire for a noncompete until after the employee has accepted an employment offer and started work (Marx, 2011).<sup>15</sup> In a competitive labor market, such delay would be irrelevant—an employee would always be free to leave for an equally attractive alternative employment opportunity. However, employees make early, unrecoverable investments in new positions, attractive alternative offers are not always readily available, and employers are free to require a noncompete as a condition of employment at the most opportune moment. As long as the employee is better off than being unemployed, she will agree to a noncompete.<sup>16</sup> Of course, some employees are

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<sup>15</sup>Arnow-Richman (2006) calls this a "cube-wrap" contract in that you have to join before you are informed that you must agree to additional terms if you wish to stay.

<sup>16</sup>The canonical story is one in which the employee comes to work on the first day, finds a stack of papers to sign, and is entirely unaware that a noncompete is waiting for him. The costs (including risks) associated with negotiating the noncompete at that point are likely to be high, especially if the employee relocated to accommodate the new job, is worried that he might upset management, or has already declined all other opportunities. One important question is why employers do not *always* delay, given that employees are always more limited in their ability to bargain once an offer has been accepted, investments made, and other offers declined. Presumably, at least in many employment relationships, such a practice would be regarded as unfair and would reduce employee productivity (or increase employee misconduct) by too much to make the practice profitable.

likely to anticipate such strategic delays,<sup>17</sup> and so will be able to recoup losses ex ante either by insisting on a contingent contract (compensating them in the event a noncompete is requested) or demanding additional compensation upfront. We expect these contracts to be rare due to the potentially large negotiation costs.<sup>18</sup>

To summarize, employers can use noncompetes to reduce turnover costs, to prevent damaging information or clients from reaching competitors, to reduce wage competition, and/or to increase productivity through training. Any particular set of noncompetition agreements can be welfare enhancing (e.g., by inducing productive investments in employees) or welfare reducing (e.g., by limiting wage competition). Regardless, if labor markets are fully competitive, employees should at least be fully compensated for agreeing not to compete. Labor markets are unlikely to be fully competitive, however, especially when noncompetes can be requested after an employee has already begun his employment and workers have few alternative options. Under these initial conditions, firms have greater ‘term-setting’ power and employees are likely to receive less, if any, compensation for their concessions on post-separation competition. In what follows we examine how noncompetes and the competitive conditions under which they are signed are related to wage, training, and job satisfaction outcomes, leaving the study of the mobility process to our companion paper [Starr et al. \(2016\)](#).

### 3 Data and Survey Methodology

Our data come from a large-scale survey that we developed and administered between April and July 2014 to a panel of verified respondents via Qualtrics. We provide a limited discussion of the data here, with more details described in Data Appendix [E](#), and an even more extensive discussion in [Prescott et al. \(2016\)](#), which describes in meticulous detail the cleaning process, our investigation into sample-selection issues, hand-coding of occupation and industries, weighting methods, and imputation procedures. The sample population are labor force participants aged 18 to 75 and

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<sup>17</sup>Some, of course, will be surprised. See, for example, the Reddit thread on how a new CEO forces 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](https://www.reddit.com/r/personalfinance/comments/65p21w/hrrecruiters_of_reddit_im_a_27_year_old_being/?limit=500).

<sup>18</sup>In theory, a “lemons market” could develop in which employers whose employees are no more productive with noncompetes—and which therefore do not use them—are unwilling to pay an upfront premium and leave the market. In such a scenario, such employers would probably devise an ex ante commitment strategy, like a standardized contingency contract.

who are either unemployed or employed in the private sector or in a public healthcare system. The final sample contains 11,505 respondents sampled from all states, industries, occupations, and other demographic characteristics. To ensure that the data are nationally representative, we created weights for our analysis using iterative proportional fitting (called ‘raking’) to match the marginal distribution of key variables in the 2014 American Community Survey.<sup>19</sup>

One of the primary challenges we face in studying the use and effects of noncompetes is that individuals may not be aware of whether they are bound by one. For example, 8.8% of those who have ever signed a noncompete report that they have at least once discovered that they had unknowingly signed a noncompete previously. To identify the extent of these concerns, we first defined a noncompete in the survey and asked respondents if they had ever heard of a noncompete before. Of those who had heard of a noncompete, we asked whether they had ever signed one, and of those who had ever signed one we ask whether they had signed one in their current job. For the 11,505 respondents, the unweighted distribution for those who had signed a noncompete in their current job is 15.2% Yes, 55.1% No, and 29.7% Maybe, where the maybe category includes those who have never heard of a noncompete (24.8%), those who do not know if they have ever signed one (2.2%), those who do not want to say (0.23%), and those who cannot remember (2.5%).<sup>20</sup>

We address the uncertainty regarding whether an individual is subject to a noncompete in two ways. First, we use a bounding approach, recognizing that if none of the maybes signed then the proportion of yes’s is a lower bound on the incidence of noncompetes, and if all of the maybes signed, then that would be an upper bound. Second, we recognize that to generate an overall incidence estimate, as well as capture the overall effect of a noncompete, we need to be able to identify if a given individual has signed one, whether or not they are aware of it. To do this, we use multiple imputation methods (King et al., 2001) to impute whether or not the individuals in the ‘Maybe’ category have signed a noncompete.<sup>21</sup> In particular, we use chained multiple imputation

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<sup>19</sup>We considered numerous weighting schemes. See Tables 16 and 17 in Prescott et al. (2016) for more details. The marginal distributions matched via iterative proportional fitting are gender, age (deciles), annual compensation (20 quantiles), industry (2 digit NAICS), occupation (2 digit SOC), education (9 categories), a dummy for being in school, class of the worker (for-profit, non-profit), a dummy for being unemployed, a dummy for working more than 40 weeks per year, a dummy for working more than 40 hours per week, and the state of the respondent. We allow for 1000 iterations and set the maximum weight at 5.

<sup>20</sup>The distribution among the full sample for whether an individual has ever signed a noncompete is 31.5% Yes, 41.5% No, and 27% Maybe. Among those individuals who reported that they had or had not ever signed, we asked them how confident they were in their answer and 74.2% report that they were completely sure, while 23% reported that they were fairly sure.

<sup>21</sup>We thank Ben Klemens for this suggestion.

to create 25 different datasets in which we impute both whether the ‘Maybes’ have ever signed, or currently signed, as well as a variety of other variables that are missing (see [Prescott et al. \(2016\)](#) Section II.F). One often confused point about multiple imputation is whether dependent variables (e.g., wages) should be used as a control in the imputation of an independent variable (e.g., noncompetes). The answer is unequivocally yes ([Sterne et al., 2009](#)), or else the imputed estimates will be attenuated. Indeed a general rule is that all variables used in the analysis should be included in the imputation model.<sup>22</sup>

Multiple imputation estimates of the use or effects of noncompetes are created by estimating each model on the 25 different but complete datasets, aggregating the point estimates across the 25 different estimates, and correcting the standard errors to reflect the variation across estimates. The benefit of multiple imputation methods is that it allows us to create an overall estimate of the use of noncompetes that accounts for the uncertainty in whether the ‘maybe’ group has signed. The unweighted multiple imputation estimates calculate that 19.9% of individuals are currently bound by a noncompete (implying that 16% of the Maybe’s have signed in their current job), while 40.5% have ever signed (implying that 33% of the Maybes have ever signed a noncompete).

In the following section, we report the weighted incidence of noncompetes. In Figures [A1](#) to [A12](#) describing the use of noncompetes, the size of the red bars show the size of the “maybe” category, such that the lower end of the red bar represents the lower bound on the incidence of noncompetes, the upper end of the red bar represents the upper bound of the incidence, and the dark blue dot within the red bar represents the multiple imputation estimates. Note that in most instances, the multiple imputation estimate is rather close to the lower bound, such that relatively few of the ‘Maybe’ category are imputed to have signed.

## 4 The Use of Noncompetes

Our framework generates predictions about when we ought to observe noncompetes (i.e., incidence) and the likely effects of noncompetes on employee outcomes, depending on the initial conditions. In this part, we use our comprehensive survey data to construct the first nationally representative estimates of noncompete incidence, characterize the relationship between noncom-

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<sup>22</sup>For a simple explanation, see <http://thestatsgeek.com/2015/05/07/including-the-outcome-in-imputation-models-of-covariates/>, which walks through why including outcome variables is necessary.

pete use and individual- and employer-level characteristics, and describe the variation in the noncompete contracting process.<sup>23</sup>

## 4.1 Descriptive Statistics

We describe the incidence of noncompetes in some detail because this paper provides the first comprehensive examination of noncompete use in the U.S. labor force. We find that noncompetes are a regular part of the employment relationship: 38.1% of the U.S. labor force report agreeing to a noncompete at some point in their lives, while 18.1%, or roughly 28 millions individuals,<sup>24</sup> report currently working under one.<sup>25</sup>

Consistent with standard accounts, noncompetes are more frequent among the more highly educated and those with higher earnings, but are common across all segments of the labor force, even among those without college degrees and with low earnings (see Figures A1 and A2). Among those *without* a bachelor’s degree, for example, 34.7% report having signed a noncompete at some point in the past, while 14.3% report currently working under one. Of those earning at most \$40,000 per year, 13.5% are currently bound by a noncompete, with 33.3% reporting that they have submitted to one at some point.<sup>26</sup> Note also that uncertainty around noncompetes is also higher for the less educated and lower earning. In Figures A1 and A2, the size of the red bar – which reflects the size of the maybe group – is much larger for less educated and poorer individuals.

Noncompetes are also more commonplace in certain high-skilled occupations and industries, though still common in most occupations and industries. Figures A3 to A5 examine the distribution of noncompetes across occupations and industries.<sup>27</sup> The occupations in which noncompetes

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<sup>23</sup>Figures A1 through A12 show bivariate relationships between noncompete use and a range of variables.

<sup>24</sup>The BLS estimates that the labor force had 156,090,000 members in July of 2014.

<sup>25</sup>Noncompetes are 10 percentage points more likely to occur in for-profit employers (19%) than in private non-profits (9.8%). Men are slightly more likely than women to have signed a noncompete at some point (39.7% versus 36.3%) and to be currently bound by one (18.8% versus 17.3%). Noncompetes are also more common among the young. See Figure A7 for a more detailed breakdown. 19.5% of those younger than 50 have signed (40% have signed one at some point), compared to 14.7% (34.2%) of those over 50.

<sup>26</sup>By contrast, 45.4% of those with at least a bachelor’s degree have signed a noncompete in their lives, while 26.6% are currently restricted from competing post-termination. 36.5% of those earning at least \$100,000 per year are currently a party to a noncompete and 56.1% have been at some point in the past.

<sup>27</sup>We use two methods to identify the use of noncompetes across occupations and industries: First, we simply calculate the proportion of respondents who sign within a given occupation or industry. Second, we ask individuals to project how common noncompetes are within their occupation and industry, and then aggregate those estimates into a single occupation or industry-specific number. The idea behind using “projected estimates” is that while the employee’s experience is only one data point, her knowledge about the occupation and industry as a whole represents many data points. See Rothschild and Wolfers (2013) for an example of this method in a voting context.

are found most frequently are architecture and engineering (36%) and computer and mathematical jobs (35%). Farm, fishing, and forestry have the lowest incidence (6%).<sup>28</sup> With respect to industries, noncompetes are most prevalent in information (32%), mining and extraction (31%), and professional and scientific services (31%). Noncompetes are found least frequently in the agriculture and hunting (9%) and the accommodation and food services (10%) industries. Figure A5 shows that among the joint occupation-industry distribution, the use of noncompetes is highest at the intersection of technical jobs (computer, mathematical, engineering, architecture) in the manufacturing and information industries.<sup>29</sup>

As the occupation and industry results suggest, noncompete incidence is significantly higher among those who report possessing some type of trade secret or valuable information. Figure A6 breaks down the incidence of noncompetes by type of “legitimate business interests.”<sup>30</sup> Those who work with trade secrets are the most likely to be bound by a noncompete (33 – 36%), while those who only work with clients or who have client-specific information are roughly half as likely to have a noncompete (15 – 16%).

Importantly, and surprisingly, we find very little difference in the use of noncompetes between states that will and will not enforce them. Figure A8 breaks out the use of noncompetes into non-enforcing states and quintiles of enforcing states using the measure of enforceability developed in Starr (2016). There is no evidence of any difference in the incidence of noncompetes in the two non-enforcing states (California and North Dakota) relative to states that are most likely to enforce noncompetes (both around 19%). One might surmise that this finding reflects the fact that multi-state firms that locate in non-enforcing states are still going to use noncompetes with choice of law provisions invoking an enforcing state’s law. Yet even in single-unit firms, the incidence of noncompetes in non-enforcing states is 14%, only slightly less than the 16.5% incidence level we find in the highest enforcing states.<sup>31</sup>

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<sup>28</sup>Legal occupations have the second lowest (10%), likely because the only occupation in which noncompetes are unenforceable in all states is the practice of law (Starr et al., 2017).

<sup>29</sup>Note that the figure only considers occupation-industry pairs for which there are at least 20 individuals in the sample in order to ensure that the results are representative.

<sup>30</sup>We define these as having access to trade secrets, working directly with clients, or simply having access to client information, such as contacts or marketing database.

<sup>31</sup>Only the 4th (14.9%) and 5th quintile (16.5%) among enforcing states have higher incidence rates than the non-enforcing states in single-unit firms. The first quintile (11.3%), second quintile (11.6%), and third quintile (13.5%) are all lower.



We also study noncompete incidence along other dimensions, including the employee’s number of employers in the last five years, the length of time the employee expected to work for his employer at the time of signing, the employer’s size, and the number of establishments in the respondent’s county-industry. See Figures [A9](#), [A10](#), [A11](#), [A12](#). We find that employees who expected to stay with their present employer two years or less are just as likely to sign noncompetes as those who planned to stay indefinitely. Furthermore, employees with many different employers in the last five years appear to be nearly as likely to sign compared to employees with only a single other employer. Noncompetes are more prevalent in firms with more than 5,000 employees (21%) relative to firms with less than 25 employees (12%), but the difference between the largest category and any other category is marginal at best. Lastly, noncompetes are somewhat more common in county-industries with more establishments, reaching 24% and 22% in the top two of 20 quantiles, though the incidence is essentially constant across the rest of the quantiles.

Moving to a multivariate regression framework, we report two types of analyses in Table [1](#). Panel A reports the marginal effects of a multinomial logit model in which the dependent variable is a categorical variable reflecting whether the individual has, has not, or is not sure whether they have signed a noncompete. Because these are marginal effects, each row must add up to 0, since changes in the probability of being in one category must be offset by changes of being in another. To interpret these results, consider the marginal effects reported for the private non-profit variable. The omitted, reference category is a private for-profit. The coefficients highlight that employees in private non-profits are 8.1 percentage points less likely to sign noncompetes than those in private for-profit companies, and that among those 8.1 percentage points, 3.9 did not sign and 4.2 were a maybe. Panel B reports results from a linear probability model in which the dependent variable is an indicator for whether the respondent was bound by a noncompete, where the maybe category are included as determined by the multiple imputation. Standard errors are clustered at the state level and all models include separate occupation and industry fixed effects. For brevity and ease of interpretation, we restrict our attention to the results in Panel B.

The estimates in Panel B are largely consistent with the bivariate results presented above. We do see that higher skilled individuals are more likely to sign: A 10% increase in income is associated with a .24 percentage point increase in the likelihood of signing a noncompete, salaried workers are 4 percentage points more likely to have signed than hourly workers, and having an

advanced degree is associated with a 6.4 percentage point increase in the probability of signing. We also see that trade secrets are strongly positively associated with the provision of noncompetes, increasing the probability of signing by roughly 20 percentage points. We also see that, perhaps surprisingly, women are 3 percentage points more likely to be bound by a noncompete than men.

With regards to “supply side” variables, we expected employees with a preference for mobility—as proxied for by either the number of employers in the last 5 years or their expected duration at their employer—would be less likely to agree to a noncompete. We find no evidence that this is the case. We also might have expected that employers would require more noncompetes when state-level unemployment was higher (at the time the individual was hired) because they had additional leverage, but we see no evidence that this is the case.

With regards to employer-level variables, we find that multi-state employers are 4 percentage points more likely to use noncompetes and that firms with more than 5,000 employees are 5 percentage points more likely to use noncompetes than firms with less than 25 employees.

Lastly, we find variation in the use of noncompetes to be marginally related to their enforceability: noncompetes are 4.1 percentage points more likely in the highest enforcing quintile (in the most saturated model) relative to states that refuse to enforce such contracts. This surprising finding highlight again the fact that noncompetes are still being used in relatively high proportions in states where they are not enforceable.<sup>32</sup>

## 4.2 Negotiation and the Contracting Process

Our theoretical analysis argues that the initial conditions under which noncompete negotiation and contracting occur determine whether the labor market operates in a more or less competitive way during the process, and therefore, whether we should expect to observe some form of compensating differential for an employee’s willingness to accept a noncompete. In particular, whether the potential employee has another employment opportunity, whether the individual has already accepted (and/or started) employment, and whether the individual is willing and able to negotiate with the employer over the noncompete should affect whether a noncompete will be associated with higher wages and additional training. Before turning to explicitly testing this hypothesis, we

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<sup>32</sup>Firms clearly still find value in using such unenforceable provisions, perhaps because individuals are likely to be uninformed of actual state law.

present descriptive statistics regarding the contracting process (among those who indicated they agreed to a noncompete).

Table 2 shows that 61% of individuals with noncompetes first learned they would be asked to agree to the provision before accepting the offer, while more than 30% first learned they would be asked to sign a noncompete *after* taking the job.<sup>33</sup> This delay appears to matter to employees because among those who received notice of an expected noncompete after accepting their offer, 26% reported that if they had known about the noncompete upfront, they would have reconsidered accepting the offer in the first place. If the applicant had another job employment opportunity, 33% report they would have reconsidered the offer (versus 22% without an alternative opportunity).

Negotiation over noncompetes is rare—only 10% report attempting to negotiate over a proposed noncompete or for additional benefits in exchange for agreeing to it. Nevertheless, the timing of the offer is somewhat correlated with whether an individual attempts to negotiate: 11.6% report negotiating when asked to sign before they accepted their offer, compared to just 6% of those asked to sign after they accepted.<sup>34</sup> Most report that when they were presented with a noncompete, they just read it and signed (88%), with a nontrivial fraction not even reading it (6.7%). Consultation of friends, family, or a lawyer is relatively uncommon (17%), but is strongly associated with attempting to negotiate relative to just signing. Roughly a third of individuals had an outside employment opportunity when they were asked to sign a noncompete. Those with such opportunities were almost three times as likely to report negotiating over their noncompete (16.5% vs. 6.8%).

Examining the interaction of our two categories of competitive conditions—the timing of the request for the noncompete (pre- or post-acceptance of a job offer) and the availability of an alternative employment opportunity—we find that 21% of workers who sign noncompetes receive them after accepting their offer and without an alternative employment opportunity. Roughly an equal proportion (23.63%) report knowing about the noncompete before accepting the job while also having another employment opportunity. Perhaps unsurprisingly, those who are informed

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<sup>33</sup>Marx (2011) finds even more substantive delays in noncompete contracting, with only 30% of his sample of engineers receiving notice of a noncompete requirement with the employment offer.

<sup>34</sup>By contrast, 31% of those asked to sign a noncompete before a promotion or raise report negotiating over their noncompete, suggesting the circumstances allow the employee a more favorable bargaining position.

and have other options are the most likely to negotiate over a proposed noncompete (16%), while those who are uninformed and have no other options are the least likely (3.4%).<sup>35</sup>

In Table 3, we report the reasons individuals cite for not negotiating over their noncompete conditional on when they were asked to sign the noncompete and whether they had an alternative employment opportunity. The top two reasons for not negotiating include that the terms were reasonable (52%) and the assumption that they could not negotiate (41%). Roughly 20% of the sample was also concerned that they would create tension with their employer or that they would be fired if they refused to sign.<sup>36</sup> Those asked to sign a noncompete upfront were 9 percentage points more likely to report that the terms were reasonable (55% vs 46%) while those with another opportunity were 14 percentage points more likely (62% vs 48%). Those who were unaware of the noncompete when they accepted their offer were also 10 percentage points more likely to assume they could not negotiate (48% versus 38%), while those without another opportunity were 6 percentage points more likely (43% vs 37%).

In unreported tabulations, we also explore respondent beliefs about the consequences of refusing to sign a noncompete, which are clearly important for understanding whether higher wages or additional training will be necessary as an inducement. We asked respondents who signed noncompetes, “Would you still have been hired if you refused to sign the non-compete?” Only 11.4% answered affirmatively; 61.6% believed not, and 27% did not know. Breaking these numbers down by the existence of an alternative employment opportunity, 18.4% of those with another opportunity reported that they thought they would still be hired if they declined to sign the noncompete, versus just 7.2% of those without another employment opportunity. Thus, the decision to sign a noncompete is really a choice of whether they want the job or not, which is critical for those without other opportunities.

Overall, this evidence reveals significant variation in the contracting process. Roughly a third of the time, employers ask individuals to sign after they have accepted the job, and a third of the time the applicant or employee has another employment opportunity when asked to sign. These

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<sup>35</sup>In unreported results, we also find that negotiation is twice as likely for those with a bachelor’s degree relative to those without a bachelor’s degree (13% vs. 6.2%), and that men are more likely to report negotiating than women (13.% vs. 4.5%). We also discover that negotiation is uncorrelated with a noncompete’s enforceability within a state. This is true even after controlling for a host of characteristics such as firm size, age, gender, industry, occupation, and education.

<sup>36</sup>For example, in an open text answer, one respondent wrote “i needed the job <expletive>, i wasn’t trying to make any waves on the first day.”

differences are not only correlated with differences in the propensity to negotiate, but also with materially different reasons for not negotiating. Given the relative lack of negotiation, a reasonable interpretation from our data is that noncompetes are typically a take it or leave it proposition.

## 5 Compensation and Consequences of Noncompetes

In this part, we examine what employees receive, if anything, when they agree to a noncompete. Competitive labor market assumptions imply that they should receive compensation of some kind, but imperfectly competitive conditions during the contracting process (even strategically created ones) may enable employers to constrain their employees for little or nothing in return.

### 5.1 Employee Beliefs about Compensation

In Table 4, in order to assess employee perceptions of the noncompete contracting process, we tabulate responses to two survey questions: First, we asked individuals with noncompetes what they were promised by their employer in exchange for agreeing to their noncompete. Second, we asked them what they *actually* received in exchange for agreeing not to compete.<sup>37</sup>

Our calculations in Panel A show that it is relatively rare for employees to report that employers explicitly or implicitly make any promises to them in return for their signing a noncompete. Approximately 86% of our respondents indicate they were not promised anything, and only 9% report that they were promised better pay, while job security (7%) and training (7%) round out the most common promises. Columns (1)–(4) indicate that the contracting process matters for whether individuals were promised benefits in exchange for agreeing to their noncompete. For example, employees who were asked to sign before they accepted their offer were 7 percentage points more likely to report having been promised benefits of some sort, with most of the difference coming from assurances of better pay (5 percentage points higher). Similarly, those who had other employment opportunities were 14 percentage points more likely to report receiving a promise of

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<sup>37</sup>The precise wording of the questions follows: 1) “Which of the following benefits did your employer promise you [beyond employment alone], either explicitly or implicitly, in exchange for signing the noncompete?” 2) “Regardless of what your employer did or did not promise, which of the following tangible benefits do you believe you have received because you signed a noncompete?” Individuals were able to select multiple choices. The table breaks down the most common responses, and cuts the data by when respondents first learned they would be asked to sign a noncompete and whether they had an alternative employment opportunity at the time.

a new benefit, with the largest source of the differential being promises of training (8 percentage points), trust and job security (7 percent points), and better pay (5 percentage points).

Panel B of Table 4 examines what employees believe they received, regardless of what their employer may or may not have promised. Column (5) shows that half of employees who sign noncompetes feel they received nothing in exchange for signing (aside from continued employment itself). Interestingly, the two top benefits employees report receiving after they agree to a noncompete is job security (30%) and more trust within the company (29%).<sup>38</sup> Columns (1)-(4) show important differences conditional on the timing of the request for a noncompete and whether the individual had other employment opportunities when they were asked to sign. In particular, those who were asked to sign *after* accepting their offer are 15 percentage points more likely to report that they did not receive anything in exchange for signing (59% vs. 44%), while those who did not have other opportunities when asked were 16 percentage points more likely to report not receiving anything (55% vs. 39%).<sup>39</sup>

These tabulations are useful in giving us a sense of an employee’s subjective experience, but it is difficult to place too much weight on these results because employees may dramatically misperceive what they actually received in exchange for agreeing to a noncompete.

## 5.2 Wages, Training, Information Sharing, and Job Satisfaction

In this section, we study the relationship between noncompete incidence, initial conditions in the labor market at the time of contracting, and various employee outcomes, including reported wages, training benefits, information sharing, and job-satisfaction.<sup>40</sup> In essence, we use variation in the initial conditions at the time of contracting to identify the effect of noncompetes on employee welfare, as measured by differences in the outcomes we consider.

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<sup>38</sup>Additional monetary compensation is the next highest with 19%, followed by more responsibility at 16%; 15% report they received more access to confidential information, 14% report were provided with more training, and 11% report more access to clients or client lists.

<sup>39</sup>This difference is persistent across every form of potential compensation: those who are asked to sign before accepting the job offer and those who have another employment opportunity when they are asked to sign are more likely to report receiving every type of benefit. The largest differences are in earnings, where a delay is associated with a 13 percentage point decrease in the likelihood of receiving additional pay (23% vs. 10%), and the lack of an outside option is associated with an –11 percentage point differential (26% vs 15%).

<sup>40</sup>Summary statistics on these dependent variables by noncompete signing status and by contracting process characteristics are in Table 5. Within the survey design, these objective measures were captured before the explicit questions regarding what the employee received (or believed he received) in exchange for signing, and respondents were prohibited from returning to earlier questions in the survey. This way, the objective measures are not contaminated by respondents trying to make sure the objective measures line up with their subjective responses.

To carry out this analysis, we begin by noting that variation in the use of noncompetes is highly nonrandom. Therefore, an empirical strategy that involved simply regressing the outcome of interest on an indicator for a noncompete and a range of observable controls might well produce biased estimates of a noncompete’s effects, essentially by failing to account for important but unobserved omitted variables, including ignoring the possibility of reverse causation. For example, if noncompetes are used with unobservably higher-skilled employees, and unobservably higher-skilled employees command higher wages, then unobserved skill will positively bias our estimates of the effect of noncompetes on wages. Likewise, if higher quality firms are more likely to use noncompetes, and such firms also pay higher wages or provide more training, then unobserved firm quality will bias our point estimates upward. Reverse causality may also confound our estimates if a wage increase or promotion came with a noncompete. Thus, unless we can control for such unobserved heterogeneity, we cannot identify the causal effects of noncompetes.

One natural solution is to instrument the use of noncompetes with a measure of state-level noncompete enforceability, given that differences in the enforcement regime may exogenously affect whether employers and employees engage in noncompete contracting. However, inclusion of enforceability into the second stage sometimes indicates a statistically significant relationship, and estimates from this IV approach yield wildly large estimates, suggesting a likely violation of the exclusion restriction. We also considered other potential competition- and enforcement-oriented instruments,<sup>41</sup> but they also produced implausible results and seem unlikely to satisfy the exclusion restriction.<sup>42</sup>

Instead, our empirical strategy relies on the detailed noncompete contracting “process” data we have collected and leverages our unusually rich set of controls. We combine these two elements to estimate very saturated models that exploit variation in the extent to which initial conditions reflect competitive labor market assumptions. Effectively, we allow noncompetes to have differential effects conditional on when the individual first learns she will be presented with a noncompete

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<sup>41</sup>Examples include historical changes in local competition, the use of noncompetes in the occupation and industry in all other states, the growth in the number of employment law practices in a region, and arrival of managers from high noncompete use industries.

<sup>42</sup>Unfortunately, anything that causes a change in the competitive dynamics of a local market that may result in changes in noncompete usage may also cause a host of other effects that affect wages and training. Likewise, if some industries are likely to see greater noncompete usage because the industry has a greater inflow of managers from high-use industries, these managers may bring other practices from high use industries that directly effect training and wage outcomes also.

agreement and on whether she had another employment opportunity when he was asked to sign. The validity of this approach depends strongly on the plausible exogeneity of one or both of these competitive conditions or, alternatively, on the extent to which one believes our saturated models account sufficiently for any confounding variables. To assess the selection concern directly, we employ the diagnostic test developed in [Oster \(2017\)](#) in Section 5.3, which extends the methods in [Altonji et al. \(2005\)](#) to examine how serious any selection on unobservables must be to overturn our results.

In our estimating equation below, we allow the effect of a noncompete to vary by the timing of the noncompete request and the existence of alternative employment opportunities at the time the noncompete was signed, as well as the interaction of these two dimensions. We also augment the specification to include a variety of demographic, contractual, and benefit-type controls.

$$Y_{iojs} = \beta_0 + \beta_1 Noncompete_i + \beta_2 Noncompete_i * Initial Condition_i + \gamma X_{ij} + \omega_{o,j} + \epsilon_{iojs} \quad (1)$$

$Y_{iojs}$  refers to logged hourly wages, receipt of training indicators, the employer’s willingness to share valuable information, or job satisfaction for employee  $i$  in occupation  $o$ , industry  $j$ , and state  $s$ . Industry (NAICS 2 digit) by occupation (SOC 2 digit) fixed effects are captured by  $\omega_{o,j}$ .<sup>43</sup>  $X_{ij}$  accounts for a broad array of employer, employee, and employment relationship characteristics that may otherwise confound our analysis.

Specifically,  $X_{ij}$  first includes our baseline controls: employee type (e.g., hourly, salaried, commission), gender, education,<sup>44</sup> employer size, employer’s multi-state and multi-unit status, as well as linear controls for hours worked per week, weeks worked per year, their interaction, a third-degree polynomial in age, the log of the number of employers in the county-industry (plus one). Notably, because we care about the initial conditions under which the noncompete was signed, we include the log of the unemployment rate and the log of the size of the labor force in the state and year in which the individual was hired ([Beaudry and DiNardo, 1991](#)).  $X_{ij}$  also controls for more novel characteristics, such as baseline employee mobility (number of employers in the last 5

<sup>43</sup>In our regressions, standard errors are clustered at the state level ([Moulton, 1990](#)).

<sup>44</sup>These categories include less than a bachelor’s degree, a bachelor’s degree, and more than a bachelor’s degree.



years), reported poaching inflow and outflow at the respondent’s employer, the reported degree of within-industry mobility, and the types of confidential information the employee possesses.

Lastly, because noncompetes may be correlated with other features of the employment relationship (DiNardo and Pischke, 1997),  $X_{ij}$  also controls for a host of other HR benefit-type variables, including whether the position offers a retirement plan, health insurance, paid vacation, sick leave, and life insurance, and whether it involves other post-employment restrictive covenants and provisions, including a non-disclosure agreement, a non-poaching agreement, a non-solicitation (of clients) agreement, an arbitration agreement, and/or an IP pre-assignment agreement. Our fully saturated specification accounts for 53% of the cross-sectional variation in wages.

We hypothesize that employees who 1) had other employment opportunities at the time they were presented with their noncompete and/or 2) were asked to sign their noncompete *before* they accepted their employment offer were contracting under more competitive labor market circumstances. If the pro-noncompete theories have merit, then these individuals should experience some sort of compensating differential in exchange for signing. By contrast, those employees who did not have other employment opportunities or who had already accepted their offers at the time they agreed not to compete should receive *less*, at a minimum, and perhaps no compensation. However, the timing of a noncompete request and the availability of an alternative offer are also not necessarily conditionally random events. If unobservably higher-skilled individuals are more likely to have other employment opportunities or if higher-quality employers are more likely to ask employees to sign before they accept their offer, and these employers offer better wages and training, our estimates of a noncompete effect will be upward biased. We look for *prima facie* evidence of this sort of selection by examining whether employee and employer characteristics predict our key initial, noncompete-signing conditions (i.e., alternative employment opportunity, notification before employment offer accepted). The results show surprisingly little evidence of selection.<sup>45</sup>

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<sup>45</sup>Table B1 reports our estimates. We find few predictive relationships. For example, education, age, and employer size are not predictive. The only statistically significant differences relate to employee type: salaried employees are less likely to sign a noncompete after accepting their offer (though the effect falls and becomes insignificant when controlling for occupation-industry fixed effects) and more likely to have another employment opportunity at signing. Interestingly, men are less likely to be asked to sign a noncompete after accepting their offer. We were surprised to see that those hired when unemployment rates were higher in their state were more likely to report having another employment opportunity, but this statistically significant finding falls and becomes statistically insignificant with occupation by industry fixed effects. Lastly, in high enforcement states, contracting conditions

Table 6 columns (1) through (3) report our main estimates on the effects of noncompetes on wages. Panel A examines the average effect across all employees,<sup>46</sup> while Panels B, C, and D test for heterogeneous effects across different competitive contracting conditions. In Panel B, we find that those who learned about their noncompetes before accepting their offers have on average 9.8% higher wages (column (3)) than nonsigners,<sup>47</sup> while those who learned about their noncompete afterwards receive 1.6% higher wages, but this latter effect is indistinguishable from zero. Panel C tells a similar story: those who agree to noncompetes when they had an alternative employment opportunity have on average 13% higher wages than nonsigners, while those who were without alternatives have an imprecisely estimated 1% lower wages (column (3)). Panel D examines the intersection of the timing and alternative employment dimensions and shows that the group with the highest wage differential relative to nonsigners are those who learned they would be asked to sign before accepting the job and who had another employment opportunity (19.3%). By contrast, those who did not have another option and who were asked afterwards have 1.4% lower wages, though the point estimate is not statistically significantly different from zero.

Columns (4) and (5) of Table 6 attempt to better control for worker quality by including an indicator for whether the worker has been recruited by an alternative employer in the last year (column (4)), subsequently breaking that indicator into being recruited by a competitor or noncompetitor. Since recruitment is endogenous to signing a noncompete, it is ‘bad control,’ and one which we examine directly in our companion paper examining the process of employee mobility (Starr et al., 2016).<sup>48</sup> The inclusion of these variables hardly change the magnitudes of the estimated coefficients. In Table B2 in the Appendix, we extend this analysis to allow the effect of noncompetes on wages to vary by whether the individual was recruited in the last year. The results show that recruitment by competitors drives wages higher for those who are bound by noncompetes, but those who are bound by noncompetes experience relatively greater higher wages via recruitment from noncompetitors.

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appear to be slightly less competitive. Taken together, this evidence, at worst, suggests that lower-skilled employees are somewhat more likely to have worse contracting conditions.

<sup>46</sup>Panel A of Table 6 shows that noncompetes are associated with, on average, 7.8% higher wages, but this effect falls dramatically with the inclusion of controls such that in the most saturated model, noncompetes are only associated with a 3.9% higher wage and the point estimate is statistically insignificant.

<sup>47</sup>Those who sign a noncompete with a promotion show the largest wage effects, though the standard errors are large since there are so few of these individuals in the data.

<sup>48</sup>Results from Starr et al. (2016) show that those who sign noncompetes actually receive more recruitment attention than those who are not bound by noncompete.

In Table 7, we use only our most saturated specification to examine other dependent variables related to training, information sharing, and job satisfaction.<sup>49</sup> Panel A shows that the average effects of noncompetes across all employees are small and statistically insignificant, with only formal firm-sponsored training showing a statistically significant effect. However, as before, these estimates obscure significant heterogeneous effects across competitive contracting conditions, as shown in Panels B, C, and D. In particular, relative to nonsigners, those who first learned of their noncompete before joining their employer (first row of Panel B) or had another employment opportunity when they were asked to agree (first row of Panel C) are more likely to report that they receive all necessary work-related information and both formal and informal firm-sponsored training, and are somewhat more likely to report recently investing in themselves. These effects are primarily driven by those who contracted under both of these conditions. These individuals are also 8.7 percentage points more likely to report that they are satisfied with their job. By contrast, for those who are asked to sign noncompetes after they accepted the offer (second row of Panel B) or who did not have an alternative employment opportunities when they were asked to sign (second row of Panel C) or both (fourth row of Panel D), most point estimates are negative, suggesting that these individuals on average receive less information and less training, though only the effect on information sharing is statistically significant. Combined with the fact that these individuals experience no wage benefits from their noncompete, it may unsurprising that they are 10.4 percentage points less likely to report being satisfied in their job.

We conduct other analyses to further probe our theory and findings. First, we examine a related set of subjective dependent variables, including whether the employee agrees or strongly agrees that his job is secure, that the employer is committed to upgrading his skills, that the firm values creativity, and that the employee would consider returning to his employer if he left (e.g., boomerang employee).<sup>50</sup> The results are consistent with our earlier findings.<sup>51</sup> Second, in

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<sup>49</sup>Column (1)'s DV is an indicator that the employee agrees or strongly agrees that the firm shares all relevant job information. Column (2)'s DV is an indicator for receiving any training in the last year, decomposed into firm-sponsored training, which is either formal (3) or informal (4), and self-sponsored training (5). Column (6)'s DV is an indicator that the individual agrees or strongly agrees that he is satisfied with his job.

<sup>50</sup>See Table B3. It also considers self-reported measures of daily effort, creativity, and performance on the job.

<sup>51</sup>Specifically, noncompetes agreed to upfront by individuals with other opportunities are more likely to report that their employer is committed to upgrading their skills, that their employer values creativity, and that they are more creative in their jobs. Furthermore, those who receive their noncompete after accepting their employer's offer and without any alternatives are less likely to agree that their job is secure, that the firm is committed to upgrading their skills, that the firm values creativity, that they would be a boomerang employee, and they also report that they are less creative in their jobs.

Figure 1 we explore heterogeneous effects for those with and without a college degree. We find that the negative wage and training effects associated with noncompete delays and lack of outside options at signing are strongest for those without bachelor’s degrees. In contrast, the positive effects of noncompetes signed with upfront knowledge and alternative employment opportunities are somewhat stronger for those without a bachelor’s degree. Third, we explore whether the effects of noncompetes vary with an employee’s tenure. Pro-noncompete theory suggests that employees should receive a compensating differential in the form of greater *initial* wages or training, whereas anti-noncompete theory predicts little or no immediate benefits, though some benefits may accrue over time if firm-sponsored training is important. We find that those who sign noncompetes under more competitive conditions are better off initially while those who sign under more monopsonistic conditions are worse off initially.<sup>52</sup> For instance, Panel A of Figure 2 shows that those who contract under both competitive conditions receive roughly 30% higher initial wages relative to those without noncompetes, though this differential diminishes quickly over tenure. Consistent with intuition, the differences between noncompete signers and those who do not sign tend to be largest early in the relationship and dissipate over time.

### 5.3 Selection Robustness and Enforceability

Our causal interpretation relies on variation in the competitive noncompete contracting conditions or noncompete use being conditionally random. While we have an extensive set of controls to account for most plausible sources of selection, we cannot rule out the possibility that unobserved employer or employee quality, or other such variables, can explain our results.<sup>53</sup> However, we can examine how significant any such selection bias must be to account for our results, and then evaluate whether such a scenario is at all realistic. Specifically, we use Oster (2017)’s method to examine how strong selection on unobservables would have to be, relative to selection on observables, to drive our point estimates to zero.<sup>54</sup> Given the comprehensiveness of our controls,

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<sup>52</sup>Figure 2 displays the results from a series of regressions in which the coefficient we report is on the interaction between our competitive contracting conditions, with the specification run separately on groups of employees with different tenures. The tenure bins are early tenure (0-2 years), short-tenure (2-5 years), medium tenure (5-9 years), and long tenures (more than 9 years).

<sup>53</sup>One possibility is that we haven’t captured the conditions at the time of signing sufficiently well. Our results are robust to accounting for state-by-year-of-hire fixed effects, which we opt not to include because doing so effectively controls for tenure, which is endogenous to noncompete use.

<sup>54</sup>The logic behind this test is that if our coefficients of interest stay largely the same as we add controls to an initially sparse model *and* the  $R^2$  rises significantly, then the estimates are more reliable because there is little

we suggest setting the test’s  $\delta$  statistic equal to one as a natural cutoff to assess the stability of our results, and we follow [Oster \(2017\)](#) by setting the maximum  $R^2$  at 50% higher than the  $R^2$  in our fully saturated model.<sup>55</sup> Table 9 replicates our saturated models from Table 6 and Table 7, showing the  $\delta$  term in brackets. We calculate  $\delta > 1$  for all point estimates for those contracting under competitive conditions, and so we conclude that only implausibly strong selection on unobservables can explain away a causal account of our results.<sup>56</sup>

Previous empirical work on noncompetes has relied on state-level enforceability to overcome concerns about selection, since enforcement intensity is plausibly exogenous.<sup>57</sup> We find that noncompetes are still prevalent in non-enforcing states, which raises some concern about this strategy. Nevertheless, given the importance of noncompete enforceability in the existing empirical literature ([Stuart and Sorenson, 2003](#); [Marx et al., 2009](#); [Garmaise, 2009](#); [Younge et al., 2014](#); [Samila and Sorenson, 2011](#); [Starr et al., 2017](#)), we also estimate a psuedo-difference-in-difference model examining the differential effect of noncompetes in states where they are more enforceable. We use the enforceability measure developed in [Starr \(2016\)](#), which is measured in standard deviations from a mean enforcement score of zero, and modify our specification in Equation (1) by adding enforceability and its interaction with noncompete use as regressors.<sup>58</sup>

Table 8 examines the enforceability interactions for all of our outcomes of interest in our fully saturated specification.<sup>59</sup> Overall, Panel A, which assumes a common effect across all competitive conditions, suggests noncompete enforceability influences the effect of a noncompete on training, but plays little role with respect to wages, information sharing, and job satisfaction.<sup>60</sup> Panel B

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variation left to explain in the dependent variable, whereas if the  $R^2$  does not increase much, significant unexplained variation remains that might explain our results. The diagnostic exercise requires selecting a maximum R-squared possible for a given regression, and the test delivers one parameter,  $\delta$ , which indicates how powerful selection on unobservables would have to be, relative to selection on the observables, to push the point estimate in question to zero. A value of  $\delta = 1$  suggests that selection on unobservables would have to be as strong as selection on observables, while a value of  $\delta > 1$  would require selection on unobservables to be stronger than selection observables.

<sup>55</sup>We include our baseline set of controls in all models, which do not contribute towards calculating  $\delta$ .

<sup>56</sup>For those contracting under the least competitive conditions, the lack of information sharing estimate and the job-satisfaction estimate would both take stronger selection on unobservables relative to observables to overturn, while the rest of the estimates, which were already close to zero, are more sensitive to selection on unobservables.

<sup>57</sup>We provide a discussion of noncompete enforceability and measures from a recent study in Appendix D

<sup>58</sup>We omit state fixed effects from these specifications to examine the main effect of enforceability in addition to the interaction effect. The inclusion of state fixed effects does not materially affect the interaction effects.

<sup>59</sup>Of note here is the fact that those in the “maybe” category have been dropped from these regressions because the enforceability interactions with the process characteristics runs into conformability problems in the multiply imputed data.

<sup>60</sup>Noncompete enforceability negatively moderates the positive effect on wages, though the effect is statistically insignificant, and those who sign noncompetes in high enforcing states appear to be more likely to receive training.

allows the effect of noncompetes and noncompete enforceability to vary by our two competitive contracting conditions. As before, the main effects of the noncompete suggest that those who sign under competitive conditions better off relative to nonsigners, while those contracting under monopsonistic conditions earn (imprecisely estimated) lower wages, receive less information and training, and are less satisfied. The main effects of noncompete enforceability are largely insignificant, though most point estimates are negative, implying that in general employees are worse off in states that enforce noncompetes. The interaction effects are also largely insignificant.<sup>61</sup>

Our overall interpretation of the enforceability results is that noncompetes themselves, and the competitive conditions of the contracting process, have strong effects independent of their enforceability, and that the effects of noncompete enforceability are relatively less predictive of the outcomes, though there is some complexity to the pattern of results we present.

## 6 Discussion

There are three primary takeaways from this study. First, firms have many profit-maximizing reasons to use noncompetes and their use is indeed widespread: noncompetes are more common in high-skilled, high-paying jobs, but are also be found in low-skilled and low-paying jobs, even in states where the courts will not enforce such contracts. Second, noncompetes are typically a take-it-or-leave-it proposition, with less than 10% negotiating over the terms of the provision or for other benefits in exchange for agreeing. Third, variation in competitiveness of the contracting conditions—i.e., the extent to which individuals are informed about a noncompete prior to accepting their employment offer and the extent to which they have alternative job opportunities—are associated with vastly different wage, training, and job-satisfaction outcomes.

There are numerous implications of these findings. Most directly, the results suggest that when the assumptions that underlie the perfectly competitive framework are satisfied, employees appear to receive compensating wage differentials early in tenure, increased investment in human capital, and greater job satisfaction. However, when employees contract with an employer under circumstances that favor the employers' wage and term-setting power, they receive no compensa-

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<sup>61</sup>For those contracting under competitive conditions, all but one of the interactions with noncompete enforceability are statistically insignificant. For those contracting less competitive conditions, the moderating effect of noncompete enforceability is also statistically insignificant in most specifications, though we estimate a positive firm-sponsored formal training interaction.

tion in terms of either training or wages, and are less satisfied with their job relative to those not bound by a noncompete. Our results thus accommodate both theories in the literature: the models make appropriate predictions when the assumptions that underly them hold. In the aggregate, the contrasting effects offset each other.

The fact that in some cases firms can employ noncompetes without paying any sort of compensating differential suggests that labor markets are (often) imperfect, that firms do have substantial wage-setting power (Manning, 2011), and that the minority of “term-conscious” buyers do not discipline contract terms for all (Salop and Stiglitz, 1977). And while noncompetes are just one provision, albeit a powerful one, it is likely that firms are exploiting this monopsony power to incorporate other provisions in their employment contracts as well, such as class-action waivers and arbitration agreements. Our analysis suggests that where labor markets are imperfect, or employees suffer from behavioral biases, employees may not necessarily be compensated for these provisions either, and, perhaps more importantly, such provisions may exacerbate market frictions by creating additional supply-side constraints that reduce the elasticity of labor supply. In this sense, specific management practices may exploit existing labor market frictions to generate additional frictions from which the employer can profit at the expense of the employee.

Our results also encourage a reconsideration of the role that courts play relative to the use of the provisions they adjudicate. The fact that noncompetes are used in places where they are unenforceable and that the effects of noncompetes are, for the most part, independent of their enforceability, suggests that the operative variable is the existence of the provision in the first place. Some may argue that noncompetes are simply a partial transfer of the right to leave the employer and that if transaction costs are small then efficient labor markets will still result, and there may be no need for policy interventions (Coase, 2013). However transaction costs may also be large, especially if employees are uninformed and acquiring information on state laws is costly, or threats are effective in reducing employee mobility.<sup>62</sup> If so, then states that wish to reduce the impact of noncompetes on employees may want to consider policies that reduce their use as opposed to just reducing their enforceability in court. In addition, our results suggest that policies which encourage labor market competition and the provision of all job-relevant information before

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<sup>62</sup>This is indeed what we find in our companion paper examining how noncompetes affect the rate and direction of employee mobility (Starr et al., 2016).

accepting the job will allow workers to receive compensation for giving up their right to leave for a competitor.

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Figures

Figure 1: Marginal effect of noncompete across education by timing and existence of another offer

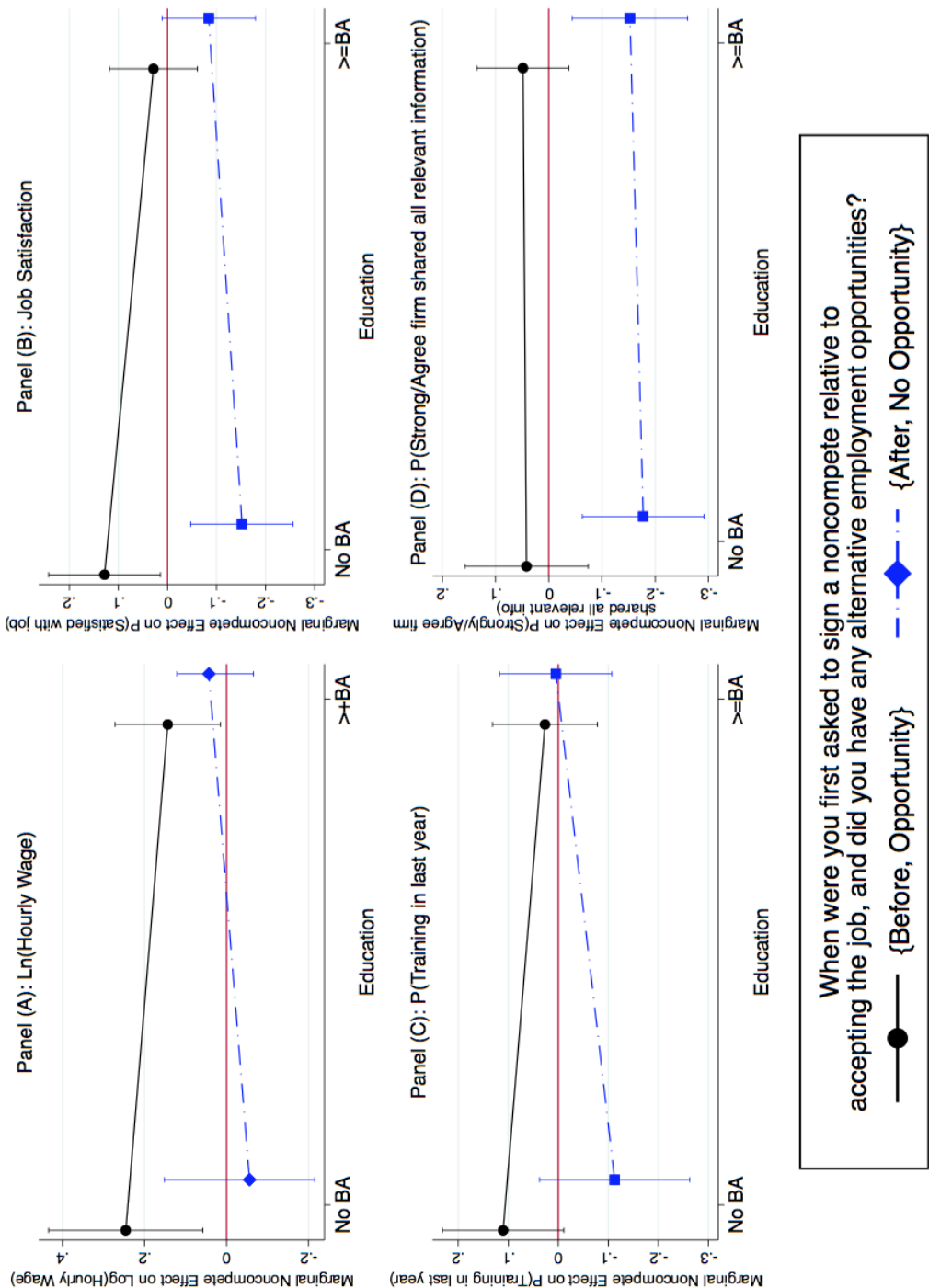
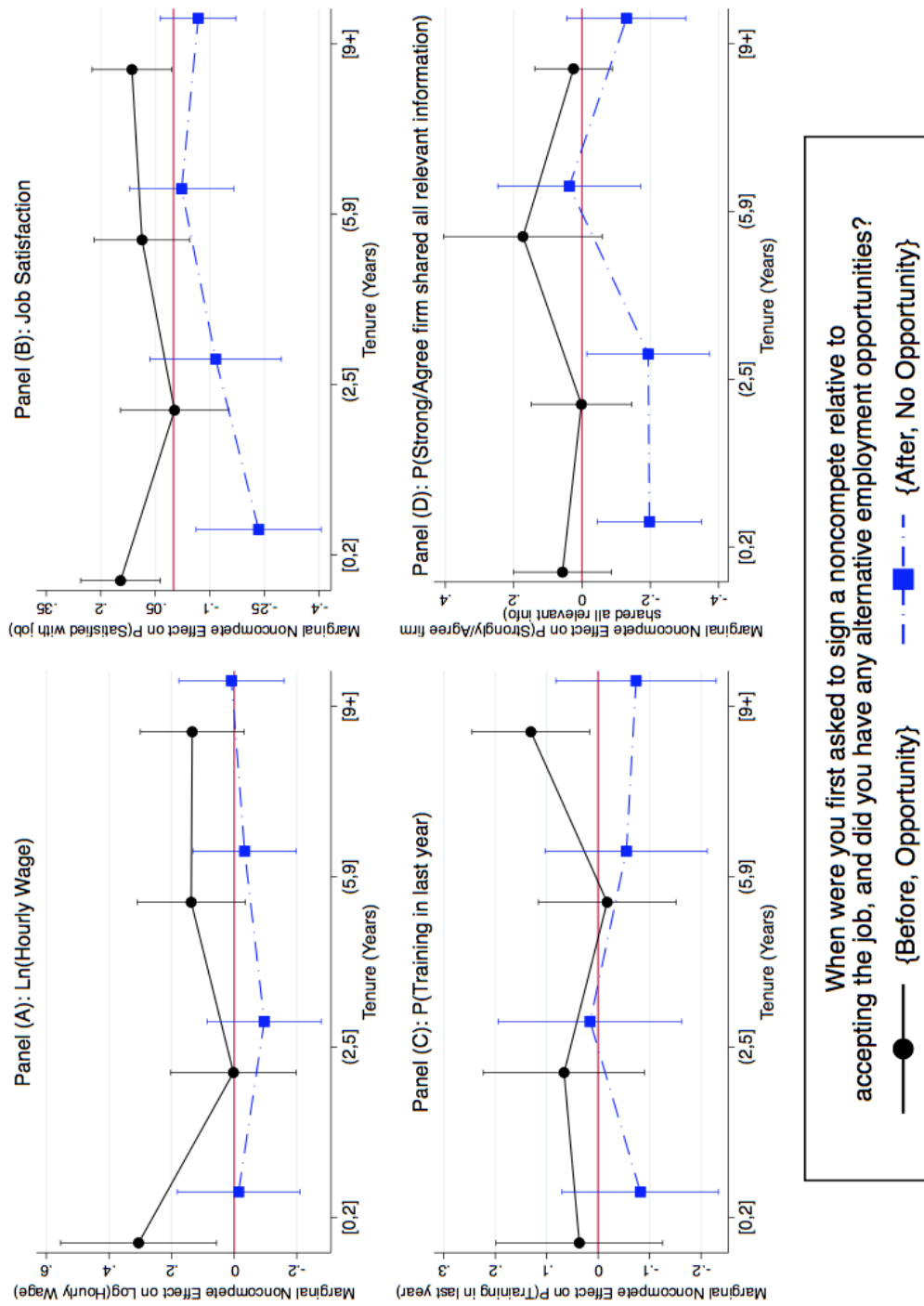


Figure 2: Marginal effect of noncompete across tenure by timing and existence of another offer



# Tables

Table 1: Who Signs Noncompetes?

	<i>Panel A: Compare Yes, No, Maybes</i>			<i>Panel B: Imputed Maybes</i>
	Changes in Probability of Signing Noncompete from Multinomial Logit			OLS: Signed Noncompete (with Imputed Maybes)
	(1) Maybe	(2) No	(3) Yes	(4) 1(Signed Noncompete)
Ln(Hourly Wage)	-0.037** (0.017)	0.006 (0.018)	0.031*** (0.011)	0.026** (0.013)
1(Highest Degree= BA)	-0.108*** (0.017)	0.076*** (0.019)	0.032*** (0.010)	0.039*** (0.014)
1(Highest Degree> BA)	-0.119*** (0.029)	0.085*** (0.029)	0.033** (0.014)	0.064*** (0.020)
1(Salaried)	-0.049*** (0.018)	0.015 (0.017)	0.034** (0.015)	0.042** (0.017)
1(Paid via commission)	-0.116*** (0.042)	-0.006 (0.042)	0.121*** (0.041)	0.132*** (0.046)
1(Paid via other method)	-0.012 (0.062)	-0.018 (0.065)	0.030 (0.044)	0.051 (0.057)
1(Private non-profit)	0.042 (0.033)	0.039 (0.033)	-0.081*** (0.014)	-0.065*** (0.022)
1(Public healthcare)	0.087** (0.042)	-0.034 (0.036)	-0.053** (0.022)	-0.071** (0.032)
Age	-0.005*** (0.000)	0.005*** (0.000)	-0.000 (0.000)	-0.002*** (0.000)
Hours worked per week	0.000 (0.001)	-0.001 (0.001)	0.001 (0.000)	0.001 (0.001)
Weeks worked per year	-0.003** (0.001)	0.002* (0.001)	0.000 (0.001)	0.000 (0.001)
1(Male)	-0.024 (0.015)	0.047*** (0.016)	-0.023** (0.011)	-0.030** (0.013)
Ln(Unemp. rate when hired)	0.001 (0.027)	-0.023 (0.024)	0.022 (0.016)	0.009 (0.017)
Ln(Labor force when hired)	-0.008 (0.010)	0.005 (0.009)	0.003 (0.007)	0.004 (0.008)
Ln(Estabs. in County-Ind.)	0.006 (0.005)	-0.007 (0.005)	0.001 (0.003)	0.003 (0.003)
1(Works with clients (WC))	-0.058*** (0.020)	0.004 (0.023)	0.053*** (0.008)	0.049*** (0.015)
1(Has client info(CI))	-0.121*** (0.038)	0.055 (0.041)	0.066*** (0.018)	0.058* (0.030)
1(Knows trade secrets (TS))	-0.178*** (0.024)	0.021 (0.028)	0.157*** (0.018)	0.190*** (0.026)
1(WC, CI)	-0.193*** (0.027)	0.132*** (0.031)	0.061*** (0.013)	0.033 (0.020)
1(WC, TS)	-0.217*** (0.042)	-0.013 (0.046)	0.230*** (0.049)	0.238*** (0.059)
1(CI, TS)	-0.194*** (0.031)	0.016 (0.029)	0.178*** (0.022)	0.190*** (0.035)
1(WC, CI, TS)	-0.240*** (0.027)	0.039 (0.026)	0.201*** (0.015)	0.212*** (0.023)
1(1st Enforceability quintile)	-0.109*** (0.026)	0.068*** (0.018)	0.041*** (0.013)	0.039** (0.018)

Continued on next page



Table 1 – continued from previous page

1(2nd Enforceability quintile)	-0.100*** (0.021)	0.073*** (0.019)	0.027** (0.012)	0.023 (0.019)
1(3rd Enforceability quintile)	-0.136*** (0.026)	0.074*** (0.025)	0.062*** (0.019)	0.054* (0.028)
1(4th Enforceability quintile)	-0.118*** (0.021)	0.084*** (0.024)	0.035** (0.017)	0.031 (0.025)
1(5th Enforceability quintile)	-0.111*** (0.026)	0.064*** (0.017)	0.047*** (0.015)	0.041** (0.020)
1(2 employers in last 5 yrs)	-0.026 (0.021)	0.027 (0.022)	-0.002 (0.011)	0.002 (0.017)
1(3-4 employers in last 5 yrs)	0.010 (0.020)	-0.014 (0.029)	0.004 (0.018)	0.015 (0.021)
1(>4 employers in last 5 yrs)	0.048** (0.022)	-0.056** (0.023)	0.008 (0.018)	0.032 (0.025)
1(E Duration 1-2 years)	0.064* (0.039)	-0.014 (0.042)	-0.050 (0.032)	-0.015 (0.037)
1(E Duration 2-4 years)	0.038 (0.040)	0.015 (0.036)	-0.053** (0.027)	-0.028 (0.035)
1(E Duration 4-10 years)	0.056 (0.035)	-0.027 (0.035)	-0.029 (0.028)	0.005 (0.029)
1(E Duration >10 years)	0.122** (0.062)	-0.095* (0.051)	-0.028 (0.037)	0.036 (0.051)
1(E Duration Indefinite)	0.059* (0.032)	-0.015 (0.035)	-0.044* (0.025)	-0.013 (0.027)
1(Multi-unit firm)	-0.033 (0.022)	0.000 (0.027)	0.032*** (0.011)	0.040** (0.015)
1(Firm size 25-100)	0.016 (0.018)	-0.046** (0.024)	0.031* (0.016)	0.032* (0.019)
1(Firm size 101-250)	0.017 (0.027)	-0.038 (0.025)	0.022 (0.017)	0.038* (0.023)
1(Firm size 251-500)	-0.003 (0.032)	-0.035 (0.031)	0.038** (0.019)	0.050** (0.024)
1(Firm size 501-1000)	0.059* (0.033)	-0.076** (0.038)	0.017 (0.025)	0.009 (0.031)
1(Firm size 1001-2500)	0.054 (0.047)	-0.078* (0.046)	0.024* (0.013)	0.056** (0.025)
1(Firm size 2501-5000)	0.088*** (0.028)	-0.106*** (0.027)	0.019 (0.018)	0.042 (0.025)
1(Firm size 5000+)	0.046* (0.026)	-0.078*** (0.027)	0.032* (0.017)	0.051** (0.022)
Constant				-0.126 (0.184)
Observations	11,462	11,462	11,462	11,462
Mean R-Squared				0.127
Occupation and Industry FE	Yes	Yes	Yes	Yes

Note: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Robust standard errors in parentheses, clustered at the state level. Panel A shows the marginal effects of a unit increase in a variable on the probability of maybe, signing, or not signing a noncompete. Each row adds to zero since increases in the chance of being in one cell are offset by lower chances of being in another. Panel B is a linear probability model in which the dependent variable is a dummy for signing a noncompete, where the maybes are included as imputed. The omitted enforceability group is the non-enforcing group (North Dakota and California) and the measure of noncompete enforceability is taken from [Starr \(2016\)](#).

Table 2: The Signing Process and Negotiation

	Distribution (%)	% Negotiate
<i>At what point did you first learn you would be asked to sign a noncompete?</i>		
Before accepting job offer	60.80	11.61
After accepting (non-promotion, non-raise)	29.26	6.26
Before promotion or raise	2.22	30.79
Other/Cannot remember	7.72	6.55
<i>What did you do when asked to sign?</i>		
Signed without reading	6.66	7.92
Read quickly and signed	31.20	7.10
Read slowly and signed	56.55	11.59
Consulted friends/family	10.41	30.82
Consulted a lawyer	7.93	48.56
<i>Did you have any alternative employment opportunities when you were asked to sign?</i>		
Yes	33.64	16.54
No	66.35	6.81
<i>When were you initially asked to sign and did you have an alternative employment opportunity at the time?</i>		
{Before accepting job, Yes}	23.63	16.02
{Before accepting job, No}	37.18	8.82
{After accepting job, Yes}	8.03	14.10
{After accepting job, No}	21.22	3.37
Overall		10.07

Table 3: If you did not negotiate over the noncompete, why not?

	(1) <i>Asked to sign before accepted job?</i>	(2) <i>Asked to sign before accepted job?</i>	(3) <i>Had other employment opportunities when asked?</i>	(4) <i>Had other employment opportunities when asked?</i>	(5) <i>Overall</i>
	Yes	No	Yes	No	
Terms were reasonable	0.55	0.46	0.62	0.48	0.52
Assumed could not negotiate	0.38	0.48	0.37	0.43	0.41
Didn't want to create tension	0.18	0.19	0.15	0.21	0.19
Worried would be fired	0.20	0.22	0.15	0.22	0.19
Didn't think firm would sue	0.07	0.11	0.09	0.08	0.08
Didn't think court would enforce	0.08	0.05	0.07	0.07	0.07
Other	0.04	0.07	0.05	0.05	0.05

The table shows the reasons individuals report not negotiating over their noncompete. Individuals could select more than one response. Those who signed before a promotion or who can't recall are omitted from columns (1) and (2) for brevity. Column (5) reports the overall average, and the rows are sorted based on these proportions.

Table 4: Direct evidence on the price of a noncompete

	(1)	(2)	(3)	(4)	(5)
	<i>Asked to sign before accepted job?</i>		<i>Had other employment opportunities when asked?</i>		<i>Overall</i>
	Yes	No	Yes	No	
<i>Panel A: "What did your employer promise, either explicitly or implicitly, in exchange for asking you to sign a noncompete?"</i>					
Nothing	0.84	0.91	0.77	0.91	0.86
More Compensation	0.09	0.04	0.11	0.06	0.07
Job Security	0.08	0.04	0.11	0.04	0.07
More training	0.07	0.04	0.11	0.03	0.06
More trust within company	0.07	0.04	0.10	0.03	0.06
Better working conditions	0.05	0.03	0.06	0.03	0.04
More responsibility	0.05	0.02	0.07	0.02	0.04
Promotion	0.03	0.03	0.06	0.02	0.03
More access to confidential info.	0.04	0.03	0.07	0.02	0.03
More access to clients/lists	0.03	0.02	0.04	0.02	0.02
More client referrals	0.02	0.02	0.03	0.01	0.02
Other benefits	0.01	0.01	0.01	0.01	0.01
<i>Panel B: "What do you believe you received in exchange for signing a noncompete?"</i>					
Nothing	0.44	0.59	0.39	0.55	0.50
Job security	0.34	0.24	0.39	0.26	0.30
More trust within company	0.33	0.23	0.35	0.27	0.29
More compensation	0.23	0.10	0.26	0.15	0.19
More responsibility	0.17	0.14	0.22	0.13	0.16
More access to confidential info.	0.17	0.13	0.23	0.11	0.15
More training	0.18	0.10	0.16	0.13	0.14
More access to clients/lists	0.13	0.07	0.16	0.08	0.11
Better working conditions	0.13	0.08	0.13	0.10	0.11
Promotion	0.10	0.05	0.14	0.06	0.09
More client referrals	0.07	0.03	0.07	0.04	0.05
Other benefits	0.01	0.02	0.02	0.01	0.01

The table shows the proportion of individuals who report receiving or being promised various benefits in exchange for signing a noncompete conditional on when they were asked to sign and if they had another job offer when they were asked to sign. Those who signed before a promotion or who can't recall are omitted from the columns (1) and (2) for brevity. Column (5) reports the overall average, and the rows are sorted based on these proportions.

Table 5: Outcome Summary Statistics by Process Characteristics

Dependent variable:	(1) Log(Hourly Wage)	(2) 1(Agree firm shares all relevant info)	(3) Any	(4) 1(Training in last year) Firm-Sponsored Formal	(5) Informal	(6) Self- sponsored	(7) 1(Satisfied with job)
<i>Panel A: Did you sign a noncompete?</i>							
No noncompete	2.83	0.54	0.48	0.27	0.41	0.08	0.67
Noncompete	3.12	0.57	0.62	0.43	0.52	0.13	0.70
<i>Panel B: When did you first learn you would be asked to sign relative to job acceptance and did you have an alternative employment opportunity at the time?</i>							
No noncompete	2.83	0.54	0.48	0.27	0.41	0.08	0.67
{Before, Yes}	3.49	0.72	0.72	0.55	0.64	0.17	0.81
{Before, No}	3.20	0.62	0.67	0.46	0.58	0.16	0.71
{After, Yes}	3.45	0.49	0.61	0.47	0.52	0.18	0.64
{After, No}	2.97	0.45	0.56	0.34	0.48	0.06	0.55
Imputed Noncompete	2.85	0.52	0.58	0.40	0.46	0.12	0.69

Table 6: Noncompetes and Earnings

Model: OLS	(1)	(2)	(3)	(4)	(5)
Dependent variable: Log Hourly Wages					
<i>Panel A: Baseline</i>					
Noncompete	0.076*** (0.028)	0.056** (0.028)	0.039 (0.029)	0.037 (0.029)	0.038 (0.029)
<i>Panel B: When did you first learn you would be asked to sign a noncompete?</i>					
Before accepted job	0.146*** (0.035)	0.119*** (0.034)	0.094*** (0.032)	0.090*** (0.032)	0.090*** (0.032)
After accepted job	0.050 (0.042)	0.022 (0.042)	0.016 (0.038)	0.017 (0.038)	0.018 (0.038)
Before promotion	0.219** (0.090)	0.163* (0.083)	0.150* (0.083)	0.138 (0.086)	0.153* (0.083)
Can't remember	0.013 (0.057)	-0.008 (0.059)	-0.004 (0.065)	-0.007 (0.065)	0.001 (0.063)
Imputed to sign	0.003 (0.053)	0.000 (0.052)	-0.013 (0.058)	-0.015 (0.058)	-0.014 (0.059)
<i>Panel C: Had alternative employment opportunity when asked to sign a noncompete?</i>					
Yes	0.217*** (0.047)	0.149*** (0.046)	0.123*** (0.041)	0.111*** (0.040)	0.111*** (0.040)
No	0.024 (0.035)	0.003 (0.034)	-0.014 (0.031)	-0.011 (0.031)	-0.010 (0.031)
Imputed to sign	-0.017 (0.054)	-0.026 (0.053)	-0.047 (0.059)	-0.049 (0.058)	-0.048 (0.059)
<i>Panel D: When were you initially asked to sign a noncompete and did you have an alternative employment opportunity at the time?</i>					
{Before, Yes}	0.239*** (0.057)	0.210*** (0.056)	0.177*** (0.054)	0.167*** (0.053)	0.164*** (0.052)
{Before, No}	0.085** (0.037)	0.062 (0.037)	0.042 (0.036)	0.043 (0.035)	0.045 (0.036)
{After, Yes}	0.149* (0.082)	0.125 (0.081)	0.106 (0.078)	0.094 (0.077)	0.093 (0.077)
{After, No}	0.013 (0.052)	-0.015 (0.051)	-0.015 (0.047)	-0.010 (0.047)	-0.007 (0.047)
Imputed to sign	0.003 (0.053)	0.000 (0.052)	-0.013 (0.059)	-0.014 (0.058)	-0.014 (0.059)
Observations	11,505	11,505	11,505	11,053	11,053
Basic Controls	Yes	Yes	Yes	Yes	Yes
Occupation-Industry FE	Yes	Yes	Yes	Yes	Yes
Flow & Info Controls	No	Yes	Yes	Yes	Yes
Benefits & Contract FE	No	No	Yes	Yes	Yes
Recruited in Last Year FE	No	No	No	Yes	No
Recruited by Non/Competitor FE	No	No	No	No	Yes

Note: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors in parentheses, clustered at the state level. The omitted category in each specification are those who did not sign a noncompete agreement.

Table 7: Noncompetes and Information Sharing, Training, and Job Satisfaction

	(1)	(2)	(3)	(4)	(5)	(6)
Model: OLS						
Dependent variable:	<b>1</b> (Agree firm shares all relevant info)	<b>1</b> (Training in last year) Any	<b>1</b> (Training in last year) Firm-Sponsored Formal	<b>1</b> (Training in last year) Self- sponsored	<b>1</b> (Satisfied with job)	
<i>Panel A: Baseline</i>						
Noncompete	-0.036 (0.025)	0.007 (0.024)	0.048** (0.022)	-0.005 (0.023)	0.027 (0.017)	0.014 (0.019)
<i>Panel B: When did you first learn you would be asked to sign a noncompete?</i>						
Before accepted job	0.034 (0.025)	0.054** (0.026)	0.080*** (0.020)	0.053** (0.026)	0.044* (0.024)	0.044** (0.021)
After accepted job	-0.141*** (0.038)	-0.055 (0.040)	-0.009 (0.031)	-0.048 (0.042)	-0.013 (0.022)	-0.083** (0.035)
<i>Panel C: Did you have an alternative employment opportunity when you were asked to sign?</i>						
Yes	0.000 (0.033)	0.036 (0.033)	0.097*** (0.028)	0.056* (0.031)	0.031 (0.030)	0.056** (0.028)
No	-0.042 (0.031)	-0.009 (0.024)	0.015 (0.023)	-0.021 (0.029)	0.025 (0.019)	-0.018 (0.021)
<i>Panel D: When were you initially asked to sign and did you have an alternative employment opportunity at the time?</i>						
{Before, Yes}	0.055* (0.032)	0.069* (0.035)	0.106*** (0.031)	0.092** (0.037)	0.028 (0.030)	0.087** (0.033)
{Before, No}	0.020 (0.032)	0.048 (0.031)	0.063** (0.028)	0.031 (0.033)	0.057** (0.027)	0.016 (0.026)
{After, Yes}	-0.130* (0.072)	-0.052 (0.060)	0.058 (0.062)	-0.042 (0.056)	0.037 (0.061)	-0.023 (0.066)
{After, No}	-0.142*** (0.045)	-0.054 (0.046)	-0.034 (0.033)	-0.049 (0.051)	-0.031 (0.022)	-0.104** (0.041)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Occupation-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Flow & Info Controls	Yes	Yes	Yes	Yes	Yes	Yes
Benefits & Contract FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors in parentheses, clustered at the state level. The omitted category in each specification are those who did not sign a noncompete agreement. The imputed noncompete signers are in the regressions in each Panel, but are not reported in the table for brevity. Those asked to sign a noncompete before a promotion or who cannot remember are in the regressions in Panel B, but are not shown for brevity; they are excluded from the analysis in Panel D.

Table 8: Interactions with Noncompete Enforceability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Model: OLS							
Dependent variable:	Log(Hourly	1(Agree firm		1(Training in last year)			1(Satisfied
	Wage)	shares all	Any	Firm-Sponsored	Self-		with job)
		relevant info)		Formal	Informal	sponsored	
<i>Panel A: Baseline</i>							
Enforceability (Enforce)	-0.016** (0.007)	0.002 (0.007)	0.001 (0.004)	0.004 (0.005)	-0.000 (0.004)	-0.003 (0.003)	0.004 (0.005)
Noncompete	0.038 (0.030)	-0.037 (0.024)	0.010 (0.026)	0.049** (0.023)	-0.002 (0.023)	0.030* (0.017)	0.011 (0.019)
Noncompete*Enforce	-0.012 (0.013)	-0.001 (0.017)	0.010 (0.011)	0.007 (0.011)	0.015 (0.009)	0.009 (0.006)	-0.010 (0.008)
<i>Panel B: When were you initially asked to sign and did you have an alternative employment opportunity at the time?</i>							
Enforceability (Enforce)	-0.018** (0.008)	-0.001 (0.009)	-0.003 (0.007)	0.007 (0.008)	-0.005 (0.006)	-0.002 (0.003)	-0.006 (0.006)
{Before, Yes}	0.161*** (0.055)	0.069* (0.038)	0.077* (0.039)	0.111*** (0.033)	0.102** (0.040)	0.027 (0.033)	0.096*** (0.032)
{Before, No}	0.037 (0.040)	0.026 (0.032)	0.053* (0.031)	0.057* (0.032)	0.032 (0.033)	0.067*** (0.023)	0.015 (0.026)
{After, Yes}	0.086 (0.077)	-0.132* (0.071)	-0.061 (0.056)	0.038 (0.055)	-0.044 (0.056)	0.042 (0.060)	-0.039 (0.056)
{After, No}	-0.017 (0.047)	-0.140*** (0.047)	-0.058 (0.048)	-0.052 (0.034)	-0.048 (0.052)	-0.021 (0.021)	-0.119*** (0.040)
{Before, Yes}*Enforce	-0.009 (0.018)	0.016 (0.020)	0.006 (0.013)	0.013 (0.013)	0.016 (0.012)	-0.000 (0.008)	0.021* (0.011)
{Before, No}*Enforce	-0.029* (0.015)	0.003 (0.020)	0.045*** (0.015)	0.002 (0.014)	0.048*** (0.014)	0.027** (0.010)	-0.001 (0.014)
{After, Yes}*Enforce	-0.033 (0.032)	-0.062 (0.040)	-0.066** (0.029)	-0.066** (0.028)	-0.024 (0.030)	0.030 (0.026)	-0.074*** (0.021)
{After, No}*Enforce	-0.008 (0.016)	0.053 (0.047)	0.036 (0.021)	0.045*** (0.015)	0.025 (0.019)	0.006 (0.010)	0.013 (0.026)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flow & Info Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Benefits & Contract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors in parentheses, clustered at the state level. The omitted category in each specification are those who did not sign a noncompete agreement. Those who do not know if they have signed a noncompete are dropped from the estimation sample in Panel B.

Table 9: Testing for the Extent of Selection on Unobservables using the Method in [Oster \(2017\)](#)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Model: OLS							
Dependent variable:	Log(Hourly Wage)	1(Agree firm shares all relevant info)	1(Training in last year)				1(Satisfied with job)
			Any	Firm-Sponsored	Self-sponsored		
				Formal	Informal		
<i>When were you initially asked to sign and did you have an alternative employment opportunity at the time you were asked to sign?</i>							
{Before, Yes}	0.167***	0.055*	0.069*	0.106***	0.092**	0.028	0.087**
$\delta_{B,Y}$	[0.956]	[0.824]	[0.963]	[1.474]	[1.636]	[1.334]	[3.108]
{Before, No}	0.043	0.020	0.048	0.063**	0.031	0.057**	0.016
$\delta_{B,N}$	[0.411]	[0.616]	[0.781]	[1.234]	[0.574]	[8.856]	[1.924]
{After, Yes}	0.094	-0.130*	-0.052	0.058	-0.042	0.037	-0.023
$\delta_{A,Y}$	[0.582]	[6.055]	[1.276]	[2.577]	[1.408]	[2.822]	[3.910]
{After, No}	-0.010	-0.142***	-0.054	-0.034	-0.049	-0.031	-0.104**
$\delta_{A,N}$	[0.173]	[4.888]	[0.884]	[0.620]	[1.298]	[11.452]	[6.652]
Mean R-Squared	0.532	0.138	0.191	0.190	0.174	0.148	0.144
P-value: $\beta_{B,Y} > \beta_{A,N}$	0.007	0.000	0.029	0.002	0.017	0.130	0.001
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flow & Info Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Benefits & Contract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . The point estimates are replicated from Table 6 and 7, while the term in brackets reports the term  $\delta$  from [Oster \(2017\)](#), which captures how much selection on unobservables would be required to drive the point estimate to zero, conditional on the maximum R-squared possible. If  $\delta > 1$ , then selection on unobservables must be stronger than selection observables in order to make the point estimate zero. Following [Oster \(2017\)](#), we set the maximum R-squared to be 50% higher than the observed R-squared in the most saturated model. Note that because our data is multiply imputed, we run Oster's program `psacalc` on each of our 25 datasets. In the table we report the median value of  $\delta$  across the 25 estimates. We also allow for a baseline set of controls in each model, which do not contribute to the estimate of  $\delta$ . These controls include indicators for way paid, a third degree polynomial in age, main effects and the interaction of hours and weeks worked, a dummy for gender, sector of the worker (e.g., for-profit), education, firm-size, a dummy for multi-unit firms, the number of firms in the county-industry of the respondent, and state fixed effects. The omitted category in each specification are those who did not sign a noncompete agreement. The imputed noncompete signers are in the regressions, but are not reported in the table for brevity. Those asked to sign a noncompete before a promotion or who cannot remember are not in the specifications.

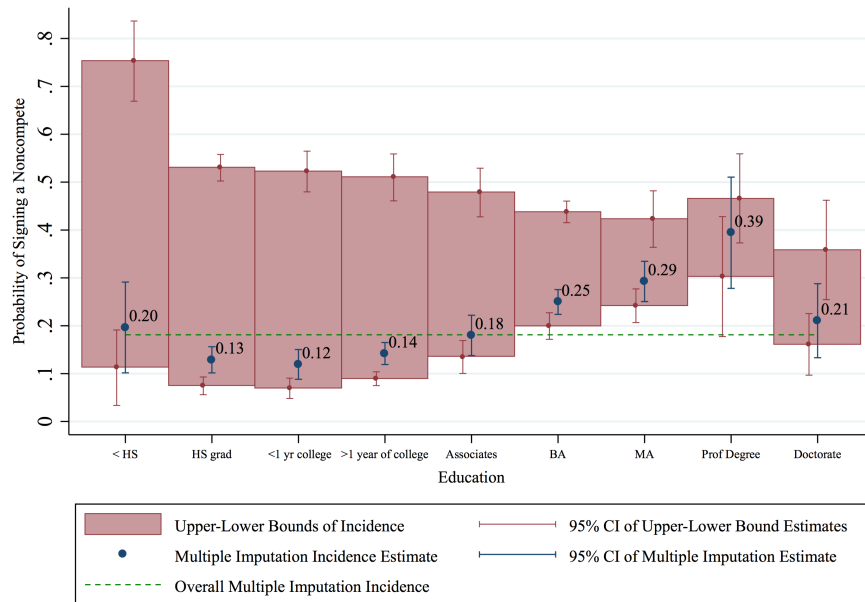


# Appendix

## A The Incidence of Noncompetes

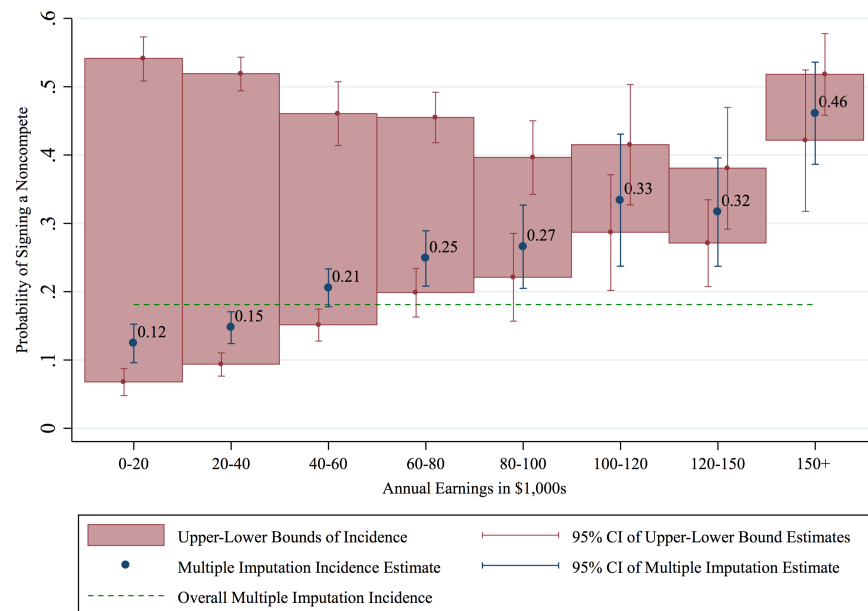
The figures below show the incidence of noncompetes by a variety of worker, firm, and regional characteristics. Within each plot the bottom and top of the red bars show the possible range of the incidence of noncompetes, which are calculated by assuming that those in the maybe group did and did not sign noncompetes. The dark dot within the 95% confidence interval is the multiple imputation estimate, which is our best guess at the overall use of noncompetes for the category. The dashed horizontal line is at the population level, 18.1%. In the occupation and industry figures (A3 and A4), we also report the ‘projected’ use of noncompetes in the occupation and industry which are calculated by averaging respondent responses to the question ‘*What proportion of individuals in your <occupation or industry> have agreed to noncompetes*’ within occupation and industry respectively.

Figure A1: Incidence of noncompetes by education



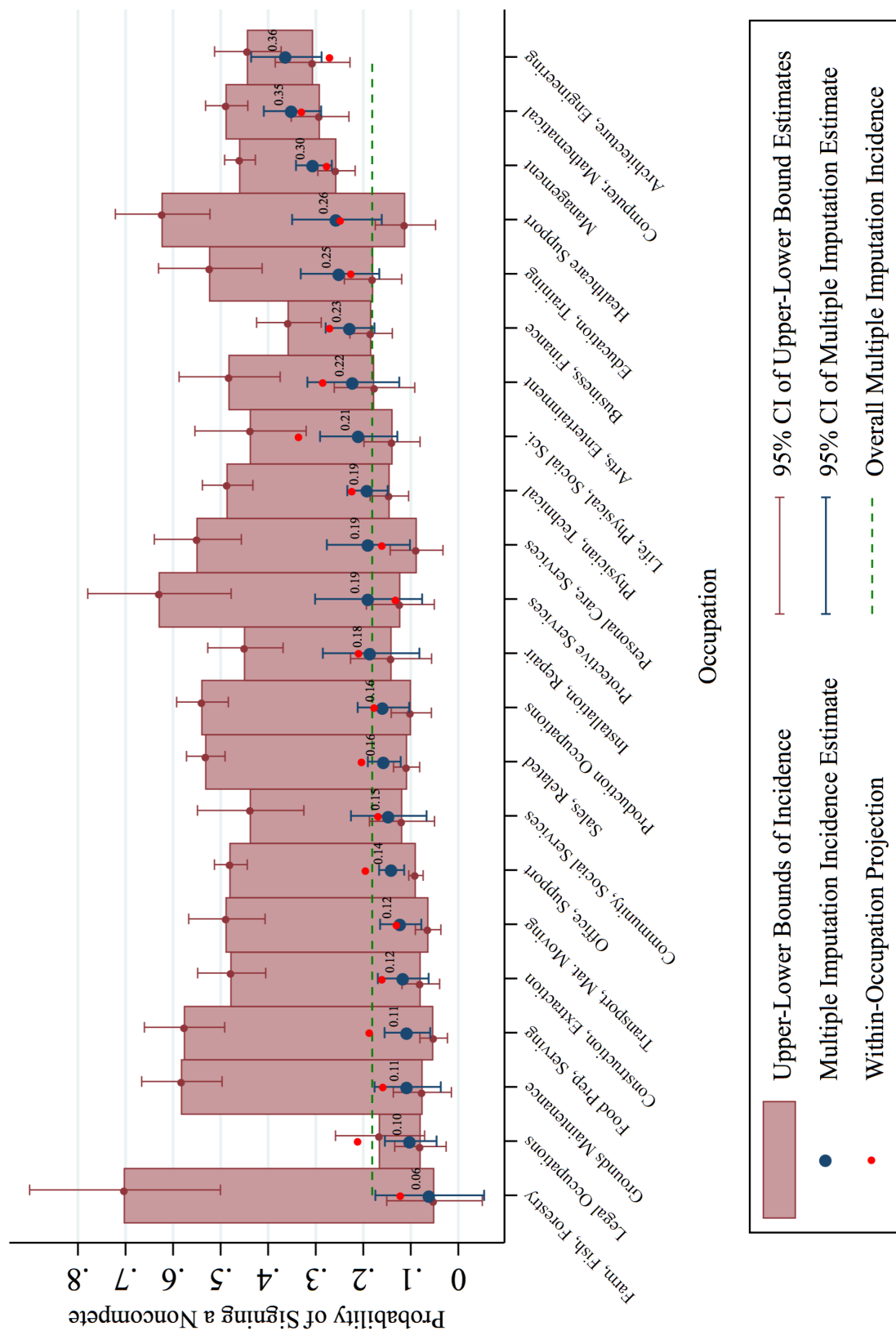
The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively.

Figure A2: Incidence of noncompetes by annual earnings



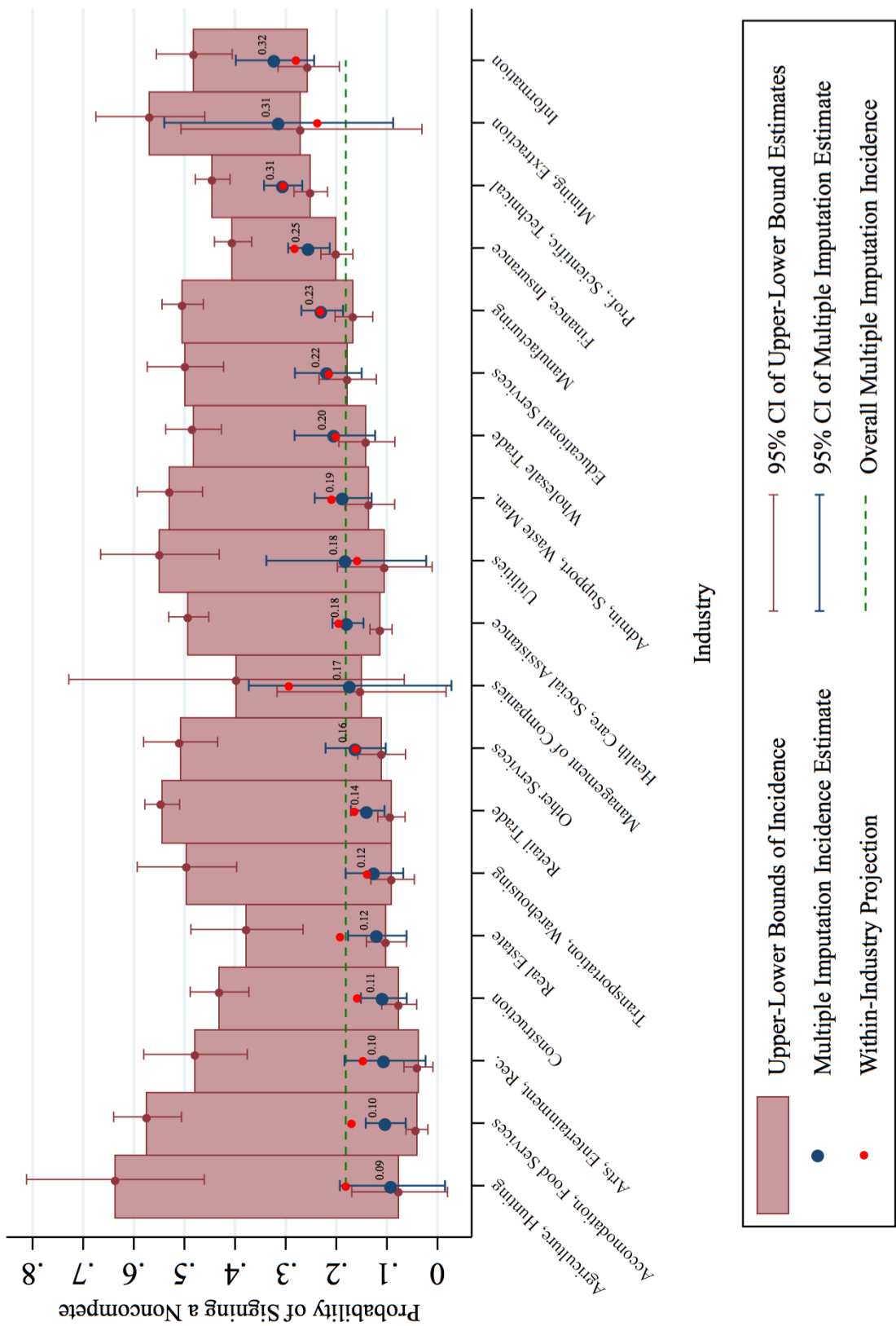
The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively.

Figure A3: Incidence of noncompetes by occupation



The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively. The projections refer to within-occupation average of the projected proportion of noncompete signers.

Figure A4: Incidence of noncompetes by industry



The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively. The projections refer to within-industry average of the projected proportion of noncompete signers.

Figure A5: Incidence of noncompetes by industry and occupation

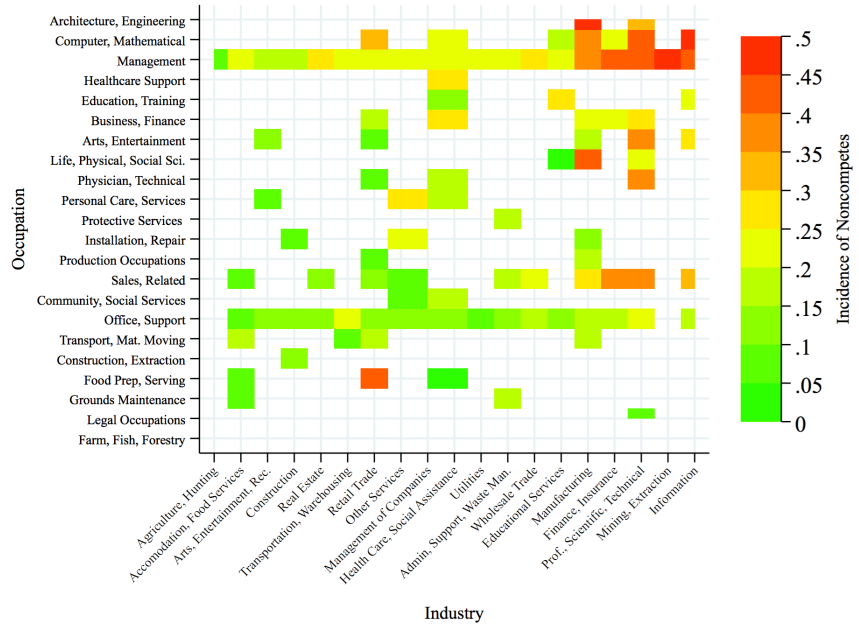
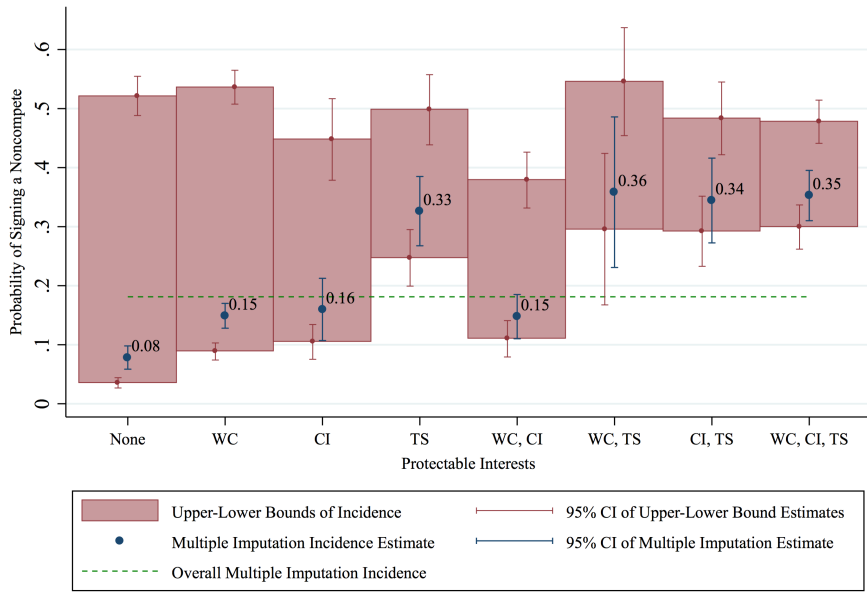
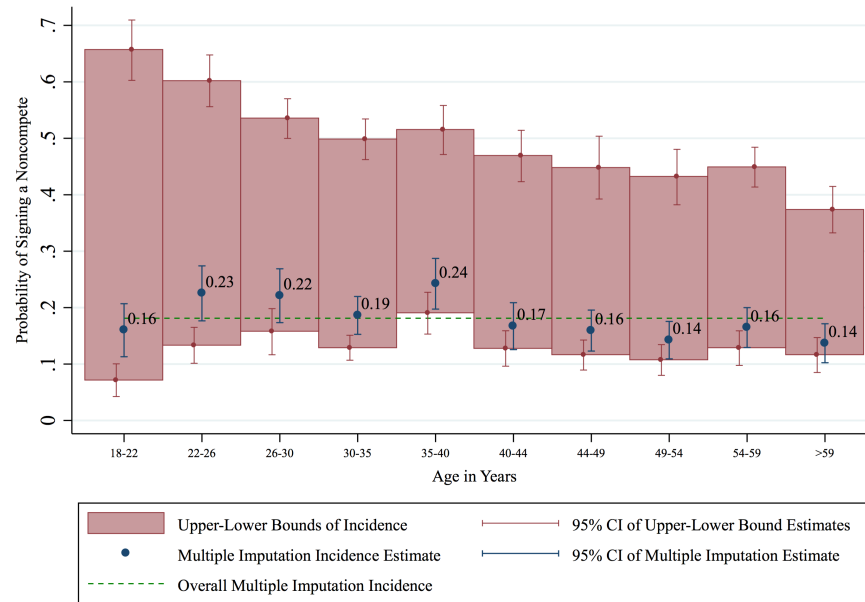


Figure A6: Incidence of noncompetes by legitimate business interest



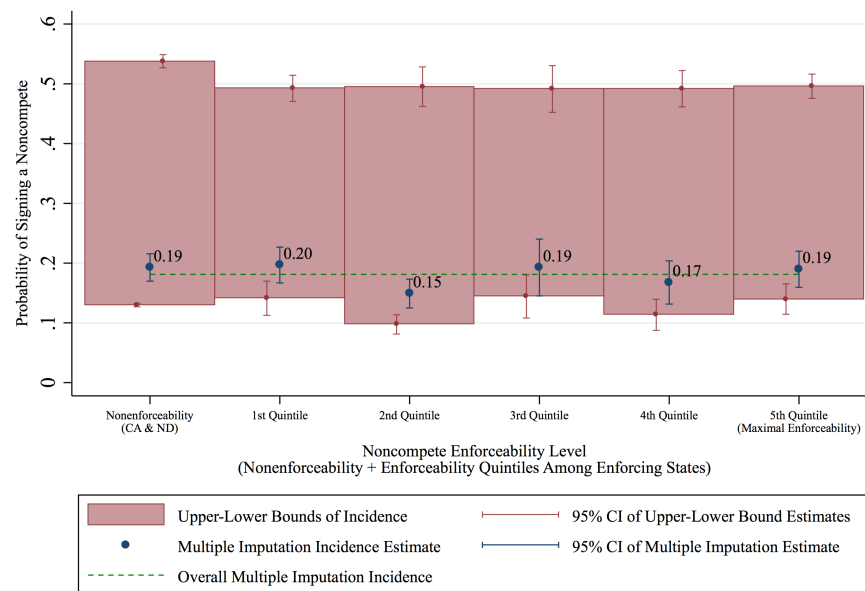
The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively. WC stands for 'Works directly with clients', CI stands for 'Access to client lists or information', TS stands for 'Knowledge of Trade Secrets'.

Figure A7: Incidence of noncompetes by age



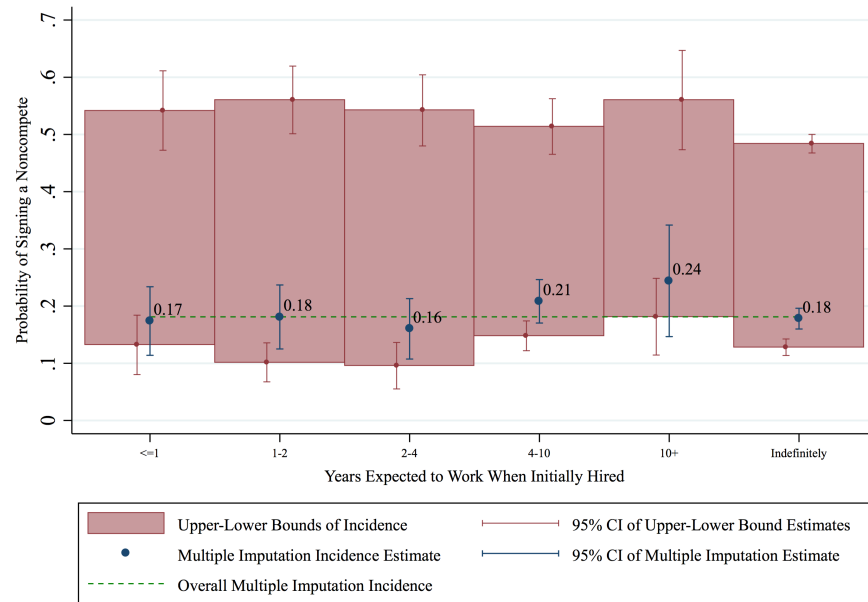
The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively.

Figure A8: Incidence of noncompetes by noncompete enforceability



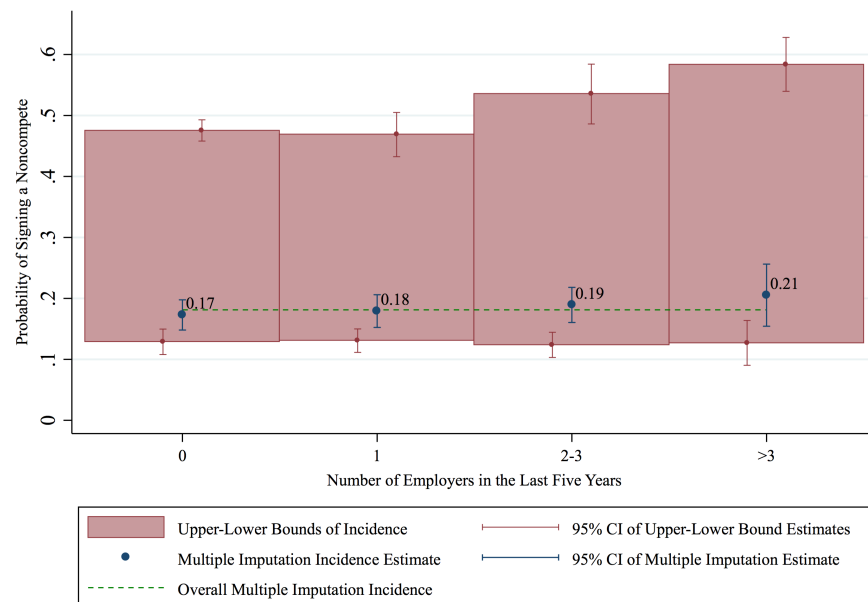
The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively. The noncompete enforceability measure from Starr (2015) is divided into nonenforcing states, and quintiles among enforcing states.

Figure A9: Incidence of noncompetes by expected length of stay



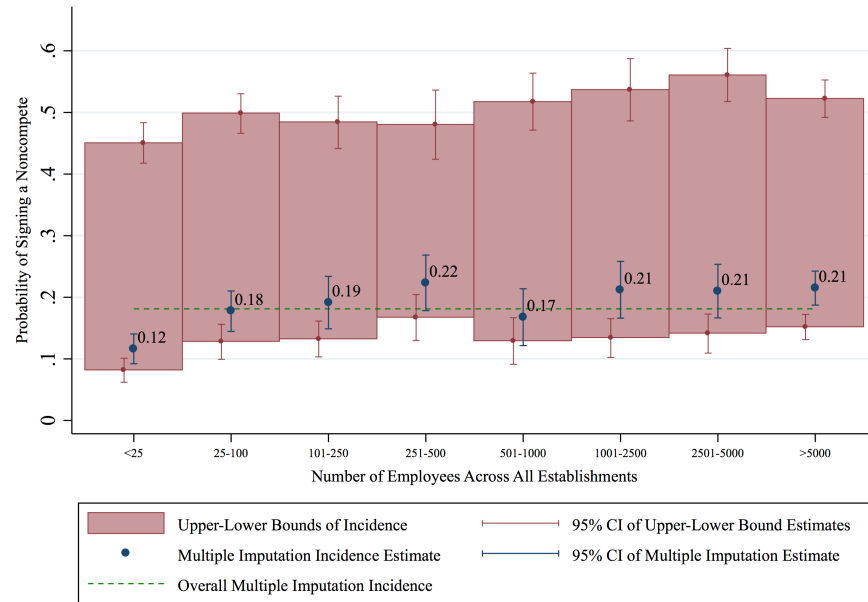
The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively.

Figure A10: Incidence of noncompetes by number of different employers in past 5 years



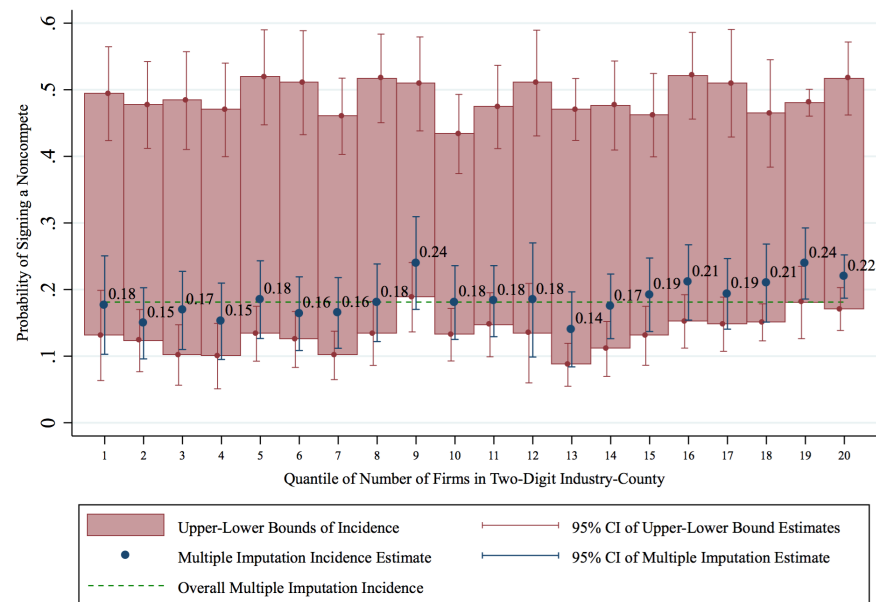
The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively.

Figure A11: Incidence of noncompetes by firm size



The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively.

Figure A12: Incidence of noncompetes by the number of firms in the county-industry



The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively.



## B Additional Tables

Table B1: Predicting noncompete timing and existence of alternative employment opportunities

Model: OLS	(1)	(2)	(3)	(4)
Dependent Variable	<i>1(Asked to sign after accepted job)</i>		<i>1(Had other employment opportunity when asked)</i>	
Ln(State unemp rate when hired)	0.019 (0.050)	0.002 (0.047)	0.105** (0.047)	0.083 (0.050)
Ln(State labor force when hired)	-0.032 (0.027)	-0.030 (0.025)	-0.019 (0.027)	-0.007 (0.027)
1(Salaried)	0.093** (0.045)	0.068 (0.053)	0.153*** (0.037)	0.172*** (0.056)
1(Commission)	0.202** (0.081)	0.125 (0.106)	0.134 (0.096)	0.170* (0.100)
1(Paid in other way)	0.066 (0.164)	-0.104 (0.196)	0.012 (0.125)	-0.019 (0.165)
Age	-0.044 (0.054)	-0.010 (0.047)	0.046 (0.051)	0.077 (0.046)
Age <sup>2</sup>	0.001 (0.001)	0.000 (0.001)	-0.001 (0.001)	-0.002* (0.001)
Age <sup>3</sup>	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000* (0.000)
Hours worked per week	-0.006 (0.012)	0.002 (0.012)	0.010* (0.006)	0.007 (0.007)
Weeks worked per year	-0.004 (0.007)	-0.001 (0.007)	0.002 (0.004)	0.002 (0.004)
Hours*Weeks	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
1(Male)	0.092** (0.040)	0.089** (0.043)	-0.019 (0.029)	-0.034 (0.036)
1(Private non-profit)	0.081 (0.107)	0.061 (0.129)	-0.165*** (0.036)	-0.127 (0.097)
1(Public Health system)	0.106 (0.094)	0.065 (0.117)	-0.113 (0.103)	-0.098 (0.121)
1(Bachelor's Degree)	-0.011 (0.042)	-0.051 (0.050)	-0.021 (0.032)	-0.035 (0.035)
1(> Bachelor's Degree)	-0.002 (0.053)	-0.041 (0.056)	0.059 (0.038)	0.057 (0.050)
1(Multi-Unit Firm)	-0.030	-0.061	-0.002	0.015

Continued on next page

Table B1 – continued from previous page

Model: OLS	(1)	(2)	(3)	(4)
Dependent Variable	<i>1(Asked to sign after accepted job)</i>		<i>1(Had other employment opportunity when asked)</i>	
	(0.061)	(0.072)	(0.044)	(0.050)
<b>1</b> (25-100 employees)	-0.002	0.004	0.007	0.003
	(0.080)	(0.082)	(0.067)	(0.076)
<b>1</b> (101-250 employees)	0.074	0.019	0.046	0.012
	(0.080)	(0.094)	(0.090)	(0.099)
<b>1</b> (251-500 employees)	0.089	0.097	0.149**	0.167*
	(0.088)	(0.095)	(0.074)	(0.090)
<b>1</b> (501-1000 employees)	0.059	0.022	-0.025	0.009
	(0.078)	(0.082)	(0.092)	(0.091)
<b>1</b> (1001-2500 employees)	0.077	0.046	0.034	-0.001
	(0.082)	(0.087)	(0.081)	(0.086)
<b>1</b> (2500-5000 employees)	0.012	0.001	0.063	0.091
	(0.098)	(0.104)	(0.086)	(0.095)
<b>1</b> (>5000 employees)	0.006	-0.002	0.028	0.014
	(0.089)	(0.093)	(0.071)	(0.075)
Log(Number of firms in county-industry)	-0.005	-0.005	0.001	-0.002
	(0.010)	(0.014)	(0.010)	(0.010)
Noncompete Enforceability	-0.025	-0.024	-0.024**	-0.017**
	(0.015)	(0.015)	(0.010)	(0.007)
Observations	1,568	1,568	1,743	1,743
Occupation-Industry FE	No	Yes	No	Yes

Note: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors in parentheses, clustered at the state level. The sample only includes individuals who report signing a noncompete. Columns (1) and (2) exclude those who were asked to sign with a promotion or cannot remember.

Table B2: Noncompetes and the Return to Recruitment

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Log Hourly Wages						
<i>Panel A: Baseline</i>						
1(Recruited in last year)	0.078*** (0.026)		0.075*** (0.027)		0.075*** (0.023)	
1(Recruited by competitor last year)		0.142*** (0.034)		0.131*** (0.033)		0.116*** (0.028)
1(Recruited by noncompetitor last year)		-0.028 (0.030)		-0.023 (0.030)		-0.014 (0.030)
Noncompete	0.035 (0.040)	0.031 (0.039)	0.020 (0.039)	0.015 (0.038)	0.016 (0.039)	0.008 (0.038)
Noncompete*Recruited	0.077 (0.058)		0.073 (0.057)		0.051 (0.056)	
Noncompete*Recruited <sub>Competitor</sub>		-0.070 (0.071)		-0.059 (0.069)		-0.068 (0.067)
Noncompete*Recruited <sub>Noncompetitor</sub>		0.164** (0.066)		0.154** (0.065)		0.141** (0.063)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Occupation-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Flow & Info Controls	No	No	Yes	Yes	Yes	Yes
Benefits & Contract FE	No	No	No	No	Yes	Yes
<i>Return to competitor minus noncompetitor recruitment among those unbound by a noncompete.</i>						
Estimate		0.170		0.154		0.131
P-value		0.006		0.010		0.016
<i>Return to competitor minus noncompetitor recruitment for noncompete signers relative to the unbound.</i>						
Estimate		-0.234		-0.213		-0.209
P-value		0.055		0.074		0.067

Note: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors in parentheses, clustered at the state level. The omitted category in each specification are those who did not sign a noncompete agreement.

Table B3: Noncompetes and Subjective Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Model: OLS							
Dependent variable:		1(Agree or Strongly Agree)			Self-Rated (0-10) Daily		
	Job is secure	Firm committed to upgrading skills	Firm values creativity	Boomerang employee	Effort	Creativity	Performance
<i>Panel A: Baseline</i>							
Noncompete	-0.011 (0.021)	0.013 (0.021)	-0.026 (0.021)	-0.061*** (0.019)	-0.034 (0.080)	0.284** (0.120)	0.031 (0.075)
<i>Panel B: When did you first learn you would be asked to sign a noncompete?</i>							
Before accepted job	0.015 (0.020)	0.057*** (0.020)	0.040* (0.021)	0.017 (0.021)	-0.106 (0.098)	0.448*** (0.107)	0.069 (0.091)
After accepted job	-0.063 (0.040)	-0.099*** (0.031)	-0.125*** (0.031)	-0.179*** (0.038)	-0.163 (0.135)	-0.277 (0.200)	-0.011 (0.109)
Before promotion	0.022 (0.061)	-0.094 (0.148)	-0.051 (0.114)	0.076 (0.057)	0.426 (0.438)	1.333** (0.643)	-0.193 (0.255)
Can't remember	-0.005 (0.037)	0.008 (0.056)	-0.059 (0.072)	-0.102 (0.073)	0.137 (0.280)	-0.105 (0.469)	-0.141 (0.191)
<i>Panel C: Did you have an alternative employment opportunity when you were asked to sign?</i>							
Yes	0.033 (0.026)	0.022 (0.031)	0.030 (0.026)	-0.037 (0.030)	-0.163 (0.132)	0.503*** (0.178)	-0.043 (0.087)
No	-0.031 (0.027)	-0.005 (0.022)	-0.043* (0.022)	-0.057** (0.024)	-0.057 (0.087)	0.056 (0.116)	0.055 (0.084)
<i>Panel D: When were you initially asked to sign and did you have an alternative employment opportunity at the time?</i>							
{Before, Yes}	0.032 (0.028)	0.053 (0.034)	0.077** (0.030)	0.032 (0.031)	-0.191 (0.171)	0.518*** (0.160)	-0.019 (0.098)
{Before, No}	0.001 (0.024)	0.059** (0.024)	0.021 (0.026)	0.012 (0.024)	-0.049 (0.128)	0.433** (0.165)	0.133 (0.111)
{After, Yes}	0.022 (0.055)	-0.034 (0.062)	-0.080 (0.069)	-0.208*** (0.051)	-0.230 (0.272)	0.340 (0.427)	-0.017 (0.189)
{After, No}	-0.097** (0.045)	-0.122*** (0.039)	-0.138*** (0.042)	-0.164*** (0.050)	-0.126 (0.200)	-0.483** (0.220)	0.008 (0.137)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flow & Info Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Benefits & Contract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors in parentheses, clustered at the state level. The omitted category in each specification are those who did not sign a noncompete agreement. The imputed noncompete signers are in the regressions in each Panel, but are not reported in the table for brevity. Those asked to sign a noncompete before a promotion or who cannot remember are in the regressions in Panel B, but are not shown for brevity; they are excluded from the analysis in Panel D.

## C Examples of Noncompetes

Below are two examples of actual covenants not to compete from Amazon and from Girls on the Run of Silicon Valley.

Figure C1: Amazon Noncompete

### 4. RESTRICTIVE COVENANTS.

**4.1 Non-Competition.** During employment and for 18 months after the Separation Date, Employee will not, directly or indirectly, whether on Employee's own behalf or on behalf of any other entity (for example, as an employee, agent, partner, or consultant), engage in or support the development, manufacture, marketing, or sale of any product or service that competes or is intended to compete with any product or service sold, offered, or otherwise provided by Amazon (or intended to be sold, offered, or otherwise provided by Amazon in the future) that Employee worked on or supported, or about which Employee obtained or received Confidential Information.

Figure C2: Noncompete from Girls on the Run of Silicon Valley

### **NON-COMPETE AGREEMENT:**

As a coach and volunteer for Girls on the Run of Silicon Valley, I agree to the following:

- 1.) I will not deliver the Girls on the Run program or any similar program unless I am working as an employee or volunteer of Girls on the Run.
- 2.) I may not create or help develop a program that has similar goals and structure to that of Girls on the Run International within a two-year period of my involvement with Girls on the Run.

## D The Enforceability of Noncompetes

Most of the scholarship on noncompetes revolves around the discussion of whether and to what extent they should be enforced in court (Blake, 1960; Garrison and Wendt, 2008; Marx et al., 2009). In the U.S., noncompetes are governed by state statutes or state case law, with states often coming to markedly different choices (Bishara, 2011). For example, California adopted a policy of nonenforceability in 1870 (Gilson, 1999), which remains today, while Florida adopted a statute in 1996 (Florida Statutes §542.335 (g)) that instructed courts that in determining the enforceability of a contract they “Shall not consider any individualized economic or other hardship that might be caused to the person against whom enforcement is sought.” Most states employ a three pronged test, commonly referred to as the ‘reasonableness’ criterion, in which the court balances the protection needed by the firm, and the harm done to the worker and society (Bishara, 2011). The state-by-state series by Malsberger et al. (2012) provides information regarding when a given state will enforce noncompetes, and many have used these texts to quantify the enforceability of noncompetes. In this paper, we use the 2009 measure developed in Starr (2016), which was built off the initial coding of Malsberger et al. (2012) done in Bishara (2011). We report the table from Starr (2016) below.

## E Data Appendix

This dataset was generated from a survey that the authors wrote and implemented between April and July 2014. The goal of the survey was to understand the use and effects of covenants not to compete, both in the worker’s current job and throughout the worker’s career. In this data appendix, we describe the relevant details of the survey design, sampling frame, and data cleaning and processing. We draw heavily on our technical paper that describes these processes in meticulous detail (Prescott et al., 2016).

### E.1 Sampling Frame and Data Collection Methodology

The sampling frame for this study are U.S. labor force participants aged 18-75, who are working in the private sector (for-profit or non-profit) or for a public health system.<sup>63</sup> We excluded the self-employed, government employees, non-US citizens, and those who are out of the labor force.

To collect the data, we considered numerous possible platforms and collection methods, including using RAND’s American Life Panel, a random digit dial survey, and adding questions to ongoing surveys like the NLSY or the PSID. Ultimately, we decided that we needed a nationally representative sample larger

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<sup>63</sup>We initially thought to focus only on the private sector, but we recognized that public healthcare systems (e.g., those associated with a public university) may also use noncompetes.

Table D1: Noncompete Enforceability Index from [Starr \(2016\)](#)

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

than the ALP would provide, and we needed to write the entire survey ourselves to be able to ask all of the relevant questions. We ultimately settled on Qualtrics, a reputable online survey company with access to more than 10 million *verified* panel respondents.<sup>64</sup>

<sup>64</sup>It is important to highlight upfront the difference between verified and unverified survey respondents. Unverified survey respondents mean that there is no external validation of any information the respondent provides you (e.g., a Google or Facebook survey), while verified survey respondents have had some information verified by the survey company. We signed up with a number of these companies to see how they vetted individuals who signed up to respond to surveys. A typical experience was to fill out an intake form in which you provide a variety of information, including a contact number. A day or so after you filled on the intake form, you would receive a phone call from the survey company on the number that you provided. On the call you would be asked a series of questions related

Our target size for this data-collection enterprise was 10,000 completed surveys. We were able to control the characteristics of the final sample through the use of quotas, which are simply constraints on the numbers of respondents with particular characteristics or sets of characteristics. In particular, we sought a final sample in which respondents were 50% male; 60% with at least a bachelor’s degree; 50% with earnings of at least \$50,000 annually from their current, highest paying job; and 30% over the age of 55. We chose these numbers either to align the sample with the corresponding sample moments for labor force participants in the 2012 American Community Survey (ACS), or to oversample certain populations of interest. Respondents who completed the survey were compensated differently depending on the panel provider: some were paid \$1.50 and entered into prize sweepstakes, others were given tokens or points in online games that they were playing.

The median survey took approximately 28 minutes to complete. Due to the length of the survey, we used three ‘attention filters’ spaced evenly throughout the survey to ensure that respondents were paying attention to the questions. Before we describe the cleaning process of the survey, we briefly describe the costs and benefits of online surveys.<sup>65</sup>

## E.2 Costs and Benefits of Online Surveys

Online surveys come with a variety of benefits. Relative to random-digit-dialing or in-person surveys, the cost per respondent is orders of magnitude lower and the data-collection time is orders of magnitude faster. The interactive survey interface also allows the survey designer to write complicated, nested questions that are easy for readers to answer on the online platform. Online surveys also allow individuals to respond at their leisure and via their preferred method (e.g., computer, phone, tablet, etc.). and place (e.g., at work, home, coffee shop). For these reasons, Reuter’s did all their polling for the 2016 Presidential election online.<sup>66</sup>

These benefits come at a potentially high cost, which is that the sample of online survey takers may not necessarily be representative of the population under study. There are four sample selection concerns in particular. First, not all of the US labor force is online. Second, not all of those online register to take surveys. Third, not all those who register to take surveys received our survey. Fourth, not all those who are invited to take the survey finish it. Among these sample selection concerns, only the second one is unique to online surveys.<sup>67</sup> It bears noting that Kohut et al. (2012) find that survey response to random-

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to the information you provided in the intake form. Verified respondents are those who are reachable by the phone number supplied, and who corroborate the information initially supplied.

<sup>65</sup>The information contained in the following sections can be found in Table 1-18 in (Prescott et al., 2016).

<sup>66</sup>See the ‘About’ tab at <http://polling.reuters.com/>.

<sup>67</sup>For example, random-digit dial surveys miss those without a phone, all those with a phone may not receive a phone call, and those who do get the call may decline the survey.



digit dialing fell from 36% in 1997 to 9% in 2012, thus questioning whether the resulting sample from a random-digit-dial survey is still a random sample of the population. We address each of these selection concerns in (Prescott et al., 2016), and discuss the second concern in particular in Section E.4.

### E.3 Survey Cleaning

The survey was randomly sent to 712,181 panel respondents by e-mail. In the e-mail, there was no indication that the survey would be about covenants not to compete. Among those who received the e-mail, 105,053 acknowledged receipt of the e-mail and 79,328 started the survey. Among those who started the survey, 50,504 were in our population of interest.<sup>68</sup> Among those who started the survey in our population of interest, 57.2% did not finish, the attention filters stopped 11.7%, 2% were kicked out for unreasonable responses, and 29% (14,668) completed the survey.<sup>69</sup>

In the second and third round of cleaning, we recognized that individuals with the same IP address could take the survey multiple times. To address this, we kept only the first attempt from a given IP address and only if it resulted in a completed survey, resulting in a sample of 12,369 respondents. We next recognized, by looking through the raw data, that some individuals appeared to have the exact same responses, even on write-in questions, despite the fact that the IP addresses were different. We thus compared individual responses for those with the same gender, age, and race, living in the same state and zip code, and working in the same county, finding that there were 665 possible repeat survey takers, the majority of whom took the survey with a different panel partner. We went through these potential repeat survey takers by hand and among those identified as repeat takers from different IP addresses, we take the first observation and drop the latter observations, resulting in a sample of 12,090 respondents.<sup>70</sup>

In the fourth round of cleaning, we examined individual answers to identify those who were internally inconsistent or unreasonable. In doing so, we developed a ‘flagging’ algorithm that flagged individuals for making mistakes within or across questions, in addition to manually reading through text entry questions. In analyzing these answers, we recognized that there were some individuals who were intentionally non-compliant (e.g., writing curse words instead of their job title), and some who simply made idiosyncratic errors (e.g., noting that the firm was smaller than their establishment). We decided to drop respondents entirely if they were deemed intentionally noncompliant because their singular responses indicated that they did not take the survey seriously, leaving us with 11,529 responses<sup>71</sup>

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<sup>68</sup>Note that in online surveys like this, it is difficult to calculate a response rate because it is unclear what proportion of the initial 712,181 respondents were in our population of interest.

<sup>69</sup>For a more detailed breakdown, see Tables 1 and 2 from Prescott et al. (2016).

<sup>70</sup>See Tables 3-5 in Prescott et al. (2016) for more details.

<sup>71</sup>See p.412-414 in Prescott et al. (2016) for more details.

In the fifth round of cleaning, we are left with those who either have clean surveys or those who have made some sort of idiosyncratic error. From our flagging algorithm, we identify that 82.2% have no flags and that 16.05% have just one flag (See Table 6 in [Prescott et al. \(2016\)](#)). The most common flag was reporting earnings below minimum wage (often 0), which was true for 1,007 of the 11,529 respondents. The challenge we faced was how to handle these flagged variables. We adopted four approaches: the first was to do nothing — simply, retain all of offending values as they are. The second was to drop all observations with any flag. The third was to replace offending values as missing. The fourth was to impute or otherwise correct offending values. Our preferred method, and the one used in this manuscript, is to impute or correct these offending values.

To accomplish this, we ‘repair’ entries that are marred by idiosyncratic inconsistency by replacing the less reliable offending value with the value closest to the originally submitted value that would not be inconsistent with the respondent’s other answers. When an answer is clearly unreasonable or missing, and there is no workable single imputation procedure, we make use of multiple imputation methods to calculate a substitute value for the original missing or unreasonable survey entry.

We also reviewed by hand the values of reported wages, occupations, and industries, due to their importance. With regards to wages, we manually reviewed all reported wages greater than \$200k and cross-checked them with the individual’s job title and job duties to ensure the attribution was appropriate. We also examined potential typos in the number of zeros (e.g., there is a big difference between \$20,000 and \$200,000, but they might look similar to survey respondents) by comparing the reported annual earnings to the expected annual earnings next year. If a typo was made by omitting a zero or including an extra zero, we would expect to see a ratio of 0.1 or 10. All such entries were corrected by examining the number of zeros reported as expected earnings in subsequent years. Wages that were clearly unreasonable and that we were unable to correct in a reasonable way were left to be imputed.

With regards to occupation and industry, we had respondents self-select 2-digit NAICS and SOC codes within the survey and also report their job title, job duties, and what their employer produced or sold. To ensure that we have the correct 2-digit NAICS and SOC codes – which are crucial for both weighting and fixed effects in any subsequent analysis – we had four sets of RAs independently code the 11,529 responses by taking the job titles, job duties, and employer descriptions and matching them with the appropriate 2 digit NAICS and SOC codes.<sup>72</sup> As part of this process, we found that 24 individuals in the sample were self-employed, worked for the government, or were retired, thus bringing down our total number of respondents to 11,505.

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<sup>72</sup>See p.422 of [Prescott et al. \(2016\)](#) for details.

At the end of this process, we are left with our final sample. Next we examine sample selection concerns, and ultimately weight the impute the missing data using multiple imputation.

## E.4 Sample Selection

As highlighted above, there are four samples selection concerns: (1) not everybody is online, (2) not everybody online signs up for online surveys, (3) not everybody who signs up for online surveys receives the survey, and (4) not everybody who receives the survey takes it. We describe these issues in greater detail in Section IIE in [Prescott et al. \(2016\)](#) and highlight the main issues here. All survey methods must confront issues (1), (3) and (4) – the only unique selection concern is (2). The key question is why individuals sign up for online surveys and whether that reason is associated with the use of noncompetes.<sup>73</sup> To understand why these individuals signed up for surveys we asked them directly, and their responses were tabulated in Table 13 of [Prescott et al. \(2016\)](#). To our surprise, the most common reasons individuals report signing up for online surveys is that they like the rewards (59%) and they like to share their opinion (58%). Only 40% said they wanted the money, and only 23% said it was because they needed money. Taking these responses seriously, the key selection question is, conditional on observables, whether individuals who like to share their opinion or like the rewards are less likely to be in jobs that require noncompetes. It is not obvious to us that this is the case.

## E.5 Weighting and Imputation

In this section we describe our approach to weighting and imputing data that is either missing or marked missing as identified in the cleaning process. The fact that weights need to be included in the imputation step to impute unbiased population values complicates these two steps. Following consultation with survey statisticians, our approach is to weight the non-missing dataset, impute the missing variables (including the weights in the imputation step), and then re-weight given the imputed values so that the resulting dataset is nationally representative. We considered several weighting schemes,<sup>74</sup> including post-stratification, iterative proportional fitting (also called raking), and propensity score weighting. Details on these methods can be found in [Kalton and Flores-Cervantes \(2003\)](#). For each method, we considered a variety of potential weighting variables, and then examined the ability of each weighting scheme to match the distribution of variables within the 2014 American Community Survey (See Table 17 in [Prescott et al. \(2016\)](#).). Iterative proportional fitting, or raking, clearly performs the best in matching the distribution of key variables in the ACS.

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<sup>73</sup>A look at the population of online survey takers (see Table 12 of [Prescott et al. \(2016\)](#)) shows that relative to the average labor force participant they tend to be female and less likely to be in full-time employment.

<sup>74</sup>See p.436-446 in [Prescott et al. \(2016\)](#) for more details.

Using the weights generated by raking, we next turn to the imputation step. We seek to impute multiple variables (see Table 18 in [Prescott et al. \(2016\)](#) for details), some of which have missing values because of the cleaning process described above and others which have missing values because the question was added to the survey while the survey was in the field. In addition, as described in the manuscript, we also impute whether the ‘maybes’ have currently or ever signed a noncompete. Because we seek to impute multiple variables, we use Stata’s chained multiple imputation command, which imputes all variables in one step. As suggested in [Sterne et al. \(2009\)](#), we incorporate all the variables used in the empirical analyses in the imputation model, which would otherwise result in attenuated estimates.

While just doing a single imputation will generate unbiased coefficients, the standard errors will be too small because the predicted value will not capture enough uncertainty in that estimate ([King et al., 2001](#)). To get the standard errors right, [Graham et al. \(2007\)](#) suggest at least 20 imputations when the proportion missing is 30%. We add another 5 to increase power.

The exact mechanics of a given imputation step are as follows. First, we fit a regression model with the non-missing data. Second, we simulate new coefficients based on the posterior distribution of the coefficients and standard errors (this is what gives us variation across the 25 datasets). Third, we apply these coefficients to the observed covariates of the missing observations and generate a predicted value. For continuous variables, we use predictive mean matching in the third step in which we take the average of the 15 nearest neighbors from the predicted value. For binary variables, we use a logit model to create the predicted value. We repeat this process for all missing values to be imputed 25 times.

Once we have the 25 imputed datasets, we re-weight within each dataset using the raking procedure described above, so that each individual dataset is nationally representative.

Estimation via multiple imputation subsequently involves running the regression model on each individual dataset, and then aggregating the 25 different estimates using Rubin’s rules, correcting the standard errors for the variation both within across imputations. Note that standard regression statistics, like the R-squared, are not typically reported in multiply imputed data, because there are 25 estimates of the R-squared. We report the mean of these estimates.