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Noncompete Agreements in the U.S. Labor Force*†

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Abstract

Using nationally representative survey data on 11,505 labor force participants, we examine the use and implementation of noncompete agreements as well as the employee outcomes associated with these provisions. Approximately 18% of labor force participants are bound by noncompetes, with 38% agreeing to at least one in the past. Noncompetes are more likely to be found in high-skill, high-paying jobs, but they are also common in low-skill, low-paying jobs and in states where noncompetes are unenforceable. Only 10% of employees negotiate over their noncompete, and about one-third of employees are presented with their noncompete after having already accepted their job offer. Early-notice noncompetes are associated with better employee outcomes, while employees who agree to late-notice noncompetes are comparatively worse off. Regardless of noncompete timing, however, wages are relatively lower where noncompetes are easier to enforce. We discuss these findings in light of competing theories of the economic value of noncompetes.

Keywords: noncompetes, employment law, transparency
JEL Codes: J4, J6, K31, L41, M5

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†Results from early versions of this paper are discussed in the U.S. Treasury report on noncompetes (U.S. Treasury, 2016) as well as the subsequent White House report (The White House, 2016).

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1 Introduction

Noncompete agreements ("noncompetes") are postemployment restrictions that prohibit departing employees from joining or starting a competing enterprise, typically within time and geographic boundaries.[1] Noncompetes have long faced significant legal hostility because of their often blunt prohibition on employee mobility (Blake, 1960), but they are nevertheless regularly enforced in the United States.[2] Spurred by anecdotes of unpaid interns and minimum wage sandwich makers signing noncompetes, policymakers in recent years have proposed dozens of legal reforms, including banning noncompetes for some or all employees and regulating the noncompete contracting process.[3] Yet relatively little is known about the actual *use* of noncompete agreements by employers because employee-level noncompete data are scarce.[4] In this study, we use nationally representative data from a survey of 11,505 labor force participants to answer three empirical questions: (1) What fraction and which types of employees enter into noncompetes? (2) What is the nature of the noncompete contracting process? And (3) how are noncompetes related to labor market outcomes, like training, wages, and job satisfaction?

Our empirical analysis is motivated by theoretical work in law and economics that considers the costs and benefits of employment contracts that limit an employee's future mobility. The traditional economics perspective has two key tenets. First, due to the inalienability of human capital (Hart and Moore, 1994), employers will be reluctant to invest in developing valuable information or specialized training—given that employees may be unable to compensate employers in advance for access to such information and training (Barron et al., 1999; Acemoglu and Pischke, 1999)—if employees can easily convey the value of any such investments to a competitor simply by taking a new job. Enforceable noncompetes solve this holdup problem by prohibiting departures

[1]Several examples of actual noncompetes are provided in Figures OE1, OE2, and OE3.

[2]All but three U.S. states enforce noncompetes (though to varying degrees) as long as they are protecting legitimate firm interests—such as trade secrets, client lists, or specialized training (Malsberger et al., 2012)—without unduly harming the employee or the public. See Online Appendix C for more on the enforceability of noncompetes.

[3]For a recent summary of noncompete proposals, see `www.https://www.faircompetitionlaw.com/changing-landscape-of-trade-secrets-laws-and-noncompete-laws/`.

[4]See generally Bishara and Starr (2016). Available noncompete data cover executives (Bishara et al., 2012) and engineers (Marx, 2011). There are also two recent papers about the use of noncompetes among physicians (Lavetti et al., 2019) and hair salon employees (Johnson and Lipsitz, 2019). A large literature studies the enforceability of noncompetes, but this work does not use data on actual noncompete use. See, for example, Balasubramanian et al. (2020); Marx et al. (2009); Stuart and Sorenson (2003); Samila and Sorenson (2011); Starr (2019); Starr et al. (2018); Conti (2014); Marx et al. (2015); Younge et al. (2014).

to competitors, which encourages employers to make these fragile but important productivity-enhancing investments (Rubin and Shedd, 1981; Posner et al., 2004; Meccheri, 2009). The second tenet is that employees will not agree to a noncompete unless an employer adequately compensates them (Callahan, 1985; Friedman, 1991), either upfront or through higher future wage growth representing part of the return on the employer's investment in the employee.[5]

In contrast, a more critical perspective recognizes that while noncompetes might solve incentive problems, they can also serve anticompetitive ends, including limiting wage growth by restraining labor-market competition from product-market competitors, retarding product-market competition by reducing information flows to competitors, and preempting future competition from departing employees (Krueger and Posner, 2018; Marx, 2018). Employers might even deploy noncompetes when they are entirely unenforceable (because they are not relying on actual enforceability to align incentives), hoping instead that the *in terorrem* effects of the contract will hold employees to their (unenforceable) promises (Sullivan, 2009; Starr et al., forthcoming; Blake, 1960). This view also challenges the notion that employees will be adequately compensated for entering into a noncompete: employers may impose a noncompete requirement only *after* an applicant has accepted an employment offer, often on the first day of the job, when the employee's bargaining power is much diminished (Arnow-Richman, 2006).

These contrasting views deliver different predictions about the incidence of noncompetes, the noncompete contracting process, and how noncompetes relate to labor market outcomes (Table 1 summarizes the predictions and findings). The more benign view tells us that noncompetes should be confined to occupations and industries that require specialized training or access to valuable information, should exist only in states that enforce noncompetes (because enforceability addresses the holdup problem), should involve negotiation, and should correlate with better employee outcomes (e.g., more training, higher wages), especially in enforcing states. The critical view contends that noncompetes should be common even among employees without access to trade secrets and in nonenforcing states, should follow a contracting process that involves little negotiation or transparency, and should be associated with worse labor market outcomes. In what follows, we describe our data and examine these competing predictions.

[5]There is also the view that noncompetes—a species of within-industry mobility friction—will not matter as long as skills and information are fungible across industries and moving costs are low (Sykuta, 2014).

2 Data

Our data come from a large-scale survey that we developed and administered in 2014 to a panel of verified respondents.[6] The sample population are labor force participants aged 18 to 75 who are employed in the private sector or in a public healthcare system or who are unemployed. The final sample contains 11,505 respondents drawn from all states, industries, occupations, and other demographic categories. We use an online survey instrument to collect these data, which offers several significant research-related benefits, such as the ability to ask technical questions in intuitive ways, easy access to millions of Americans who are comfortable responding to internet surveys, and significantly lower costs (and thus larger sample sizes). Yet surveying people online also comes with several important challenges, such as ensuring respondent reliability and representativeness, addressing item nonresponse, and even calculating the response rate.[7]

With regard to respondent representativeness, we built quotas into the surveying procedure to ensure our unweighted sample would be representative on key demographics. We also created ex post weights using iterative proportional fitting ("raking") to match the marginal distributions of many important variables in the 2014 American Community Survey (ACS).[8] Table 2 presents an unweighted and weighted comparison of our sample and data from the ACS. Our unweighted sample is higher earning, better educated, and more female than the population, but weighting appropriately virtually eliminates these differences. Unfortunately, weighting does not account for any nonrandom selection into our sample on the basis of unobservables.[9] With respect to data quality, we verify the reliability of respondent answers in several ways. In addition to examining long-answer and free-form survey responses directly,[10] we also carefully cleaned our raw data,

[6]We provide a focused discussion of our survey data here, with more details in our Online Data Appendix F. An even more extensive account of our data can be found in Prescott et al. (2016), which describes our investigation into sample-selection issues, hand-coding of occupations and industries, weighting methods, and imputation procedures.

[7]We vetted online panel providers by personally signing up as survey takers with many of these survey firms ourselves. Typically, after we completed an intake questionnaire, a representative called us a few days later at the phone number we listed and asked us questions to confirm the information we had submitted. In later discussions with various online panel providers, we learned that these companies drop applicants who give invalid phone numbers or who are not able to confirm their intake information.

[8]We considered a number of weighting schemes. See Tables 16 and 17 in Prescott et al. (2016) for more details.

[9]As for item nonresponse, note that if only respondents with an axe to grind about noncompetes finish the survey, we may find that noncompetes are associated with negative outcomes. To address this concern, we asked respondents at the end of the survey to indicate why they participated in the exercise, with an option that read: "I wanted to share my experiences with noncompetes." In our robustness checks, we drop these individuals and confirm that our results are robust to their exclusion.

[10]In Table OF1, we reproduce the self-reported job titles, occupational duties, and industries from 15 randomly selected respondents. The entries illustrate how seriously respondents took the survey. The respondent-provided job

identifying and removing repeat survey takers and excluding observations with intentionally non-compliant answers, among many other exhaustive measures that we took to address inconsistent and low-quality survey answers (see Data Online Appendix F).[11]

Respondent willingness to take and complete our survey is comparable to other surveys in the noncompete literature, although response rates are difficult to define and calculate in this setting because panel providers continuously send invitations to a superset of potential respondents—not all of whom are in our population of interest—until they receive a pre-specified number of "complete surveys." We can drop those who are not in our population of interest if they begin the survey (about 40%, see Table 2 of Prescott et al. (2016)), but we do not know and cannot determine whether those who receive an invitation but never start the survey are actually in our population of interest.[12] Given this limitation, the true response rate lies between two extremes: the final sample size over the number who started the survey within our population of interest (23%) and the final sample size over the total number of survey invitations (2%).[13]

3 The Use of Noncompetes

To identify employees bound by noncompetes, our survey instrument first defines a noncompete agreement (explicitly distinguishing a nondisclosure agreement, a common confusion) and asks respondents whether they have ever heard of such provisions (75.2% report yes). Our survey then

descriptions are quite detailed, as are the industry descriptions. We examined all of the survey data comprehensively by reviewing every one of the 11,505 free-form job titles, job duties, and industries by hand in the process of creating occupation and industry codes. It is clear that the vast majority of these respondents took care to write thoughtful responses to these questions.

[11]The final step of our cleaning process was the design and use of a flagging algorithm, which analyzes within-survey responses for internal inconsistencies. The flagging algorithm flags up to 21 different possible inconsistencies, including, for example, whether the respondent reports that the particular establishment or office at which they work is larger in terms of employee numbers than the employer's entire organization, whether there were missing responses, and others (see Table 7 of Prescott et al. (2016) for the full list). Only 1.8% of the final sample was flagged two or more times, with 82.2% receiving zero flags.

[12]The quotas we used to ensure representativeness exacerbate this problem because as the survey stays in the field and quotas begin to bind, respondents who would otherwise qualify for the survey become newly ineligible. Toward the end of the surveying period, when most quotas are full, the online survey company might send out thousands of e-mail invitations when only a handful of respondents satisfy the remaining criteria. In addition, our survey was marketed as a "work experiences survey," and online survey respondents skew toward being out of the labor force (see Table 12 of Prescott et al. (2016)), so it is likely that many who did not respond to the survey invitation were not in our population of interest.

[13]These numbers, while seemingly on the low side, are actually in line with and likely better than response rates to random-digit-dialing surveys, which were around 6% in 2018 (Kennedy and Hartig, 2019). To compare the rates we calculate to response rates for other surveys in this literature, see Table OB1. Moreover, in light of our arguably low response rates, it is important to recall that a low response rate is not problematic per se. Rather, bias results only when the reasons for nonparticipation are correlated with unobservables and outcomes of interest.

asks those who indicate some familiarity with noncompetes whether they have ever agreed to one (25% overall, 42% of those who are aware of them), and, if they answer yes, whether they are currently bound by one. For our 11,505 respondents, the unweighted distribution of those with a noncompetes currently 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%), do not know if they have one (2.2%), do not want to say (0.23%), and cannot remember (2.5%).[14]

A key challenge in calculating noncompete incidence is that many in the "maybe" category may actually be bound by a noncompete. In fact, of those in our data who report having ever entered into a noncompete agreement, 8.8% also acknowledge having unknowingly signed at least one such provision that they discovered only at some later date. We address this uncertainty in two ways. First, we treat the "maybes" as their own category, which allows us to interpret the proportion of respondents answering "yes" as a lower bound on the incidence of noncompetes and the proportion of respondents answering either "yes" or "maybe" as an upper bound. Second, because the overall effect of a noncompete is averaged across those who are and who are not aware of their noncompete status, we use multiple imputation methods (King et al., 2001) to predict which respondents in the "maybe" category have a noncompete.[15]

Overall, our weighted estimates indicate that 38.1% of U.S. labor force participants have agreed to a noncompete at some point in their lives, and that 18.1%, or roughly 28 millions individuals,[16] currently work under one.[17] Table 3 shows the distribution of temporal and geographic restrictions of noncompetes in the U.S.: most noncompetes have durations of 2 years or less, while the geographic scope is frequently the state or the entire country (or there is no geographic limitation),

[14]The unweighted distribution for whether an individual has entered into a noncompete at some point in the past in our full sample is 31.5% "yes," 41.5% "no," and 27% "maybe." Among individuals who answer "yes" or "no" (to the question whether they have ever entered into a noncompete), almost all report being confident in their answer—i.e., either completely (74.2%) or fairly (23%) sure.

[15]We provide a more in-depth discussion in Section II.F of Prescott et al. (2016). To calculate our standard errors properly, we impute noncompete status among the "maybe" category 25 separate times. We then estimate our statistical models on each of the 25 different but complete datasets and follow by using Rubin's Rules to combine the resulting point estimates and correct the standard errors to reflect the variation in the imputed values (see Online Appendix F.5 for details). The benefits of multiple imputation methods are that they allow us to create an overall estimate of the use of noncompetes that accounts for the uncertainty surrounding the "maybe" group.

[16]The Bureau of Labor Statistics (BLS) puts the U.S. labor force at 156 million in July of 2014.

[17]The unweighted multiple imputation estimates signal that relatively few "maybes" are likely to have noncompetes in fact. We calculate that 19.9% of individuals (including 16% of the "maybe" respondents) are bound by noncompetes in 2014. These numbers are similar to two other estimates from smaller but more recent surveys: Krueger and Posner (2018), using a similar online survey methodology of 795 respondents in 2017, find a 15.5% incidence rate, while a 2017 survey in Utah of 2,000 employees reports an 18% incidence rate (Cicero, 2017).

though about 20% of individuals with noncompetes are uncertain as to the precise terms. Table 4 provides means—overall and by noncompete status—of important variables in our sample. Table 5 and Figures 1 to 8 document variation in noncompete use by a range of employee and employer characteristics, with additional calculations presented in Online Appendix Figures OA1 to OA5. The figures report the results of both our bounding approach and our multiple imputation strategy.[18] In Table 6, we also examine multinomial logit (Panel A) and linear probability models (Panel B) of employee noncompete status. We briefly describe variation in noncompete use by demographic characteristics before focusing our discussion on the empirical findings that are most relevant to the theoretical and policy debates over noncompetes.

Noncompete incidence differs widely across types of employees and employers. Table 5 shows that noncompetes are more than twice as common among employees of for-profit employers (19%) than they are among those working for private non-profits (9.8%). Men are slightly more likely than women to have entered into a noncompete at some point (39.7% vs. 36.3%) and to be currently bound by one (18.8% vs. 17.3%). Noncompetes are also a bit more frequent among the young (see also Figure 1) and in areas with greater product market competition (Figure 2). Lastly, while noncompetes are more routine among those with higher levels of education (Figure 3) and among those with greater annual earnings (Figure 4) or receiving a salary (Table 5), they are still prevalent among less-educated and lower-earning employees. For example, among those *without* a bachelor's degree, 34.7% of our respondents report having entered into a noncompete at some point in their lives, while 14.3% report currently working under one. Similarly, of those earning less than $40,000 per year, 13.3% are currently subject to a noncompete, with 33% reporting that they have acquiesced to one at some point. Table 6 confirms that these patterns hold in a multivariate framework. Importantly, these figures and Table 4 also demonstrate that a disproportionate share of the "maybe" category are low-earning with lower levels of education.[19]

Consistent with the traditional case for noncompetes, the provisions are more frequent in certain high-skilled occupations and industries, though they are still common in most other occupa-

[18] The size of the bars in the figures shows the size of the "maybe" category. The lower end of the bar represents the lower bound on the incidence of noncompetes, the upper end represents the upper bound on incidence, and the dark dot marks the multiple imputation estimate.

[19] For example, among those who report having less than a bachelor's degree, nearly 45% indicate that they do not know whether they have agreed to noncompete in the past, compared to approximately 20% of respondents with at least a bachelor's degree.

tions (Figure 5) and industries (Figure 6).[20] Per Figure 5, the occupations in which noncompetes are found most frequently are architecture and engineering (36%) and computer and mathematical vocations (35%). Farm, fishing, and forestry positions have the lowest incidence (6%).[21] With respect to industries, Figure 6 shows that noncompetes are most common in information (32%), mining and extraction (31%), and professional and scientific services (31%). Noncompetes are found least frequently in agriculture and hunting (9%) and the accommodation and food services industries (10%).[22] Relatedly, noncompete incidence is much higher among those who report possessing some type of trade secret or valuable information. Figure 7 breaks down noncompete incidence by type of "legitimate business interest."[23] Those who work with trade secrets are 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%).

Finally, we find very little difference in (unconditional) noncompete incidence between states that will and will not enforce these provisions (Figure 8). This is true even among single-location employers, where we find that the unconditional use of noncompetes in nonenforcing states is only slightly lower than in states that enforce noncompete agreements most zealously (14% vs. 16.5%). By comparison, multivariate results in Table 6 indicate that, comparing two observationally equivalent employees, noncompetes appear to be somewhat more common (4 to 5 percentage points) in the most vigorous enforcing states relative to nonenforcing states. The difference between the unconditional and conditional models suggests some role for geographic selection into the use of noncompetes based on employee and employer observables.

[20] We use two methods to identify the use of noncompetes across occupations and industries: First, we calculate the proportion of respondents who agree to a noncompete within a given occupation or industry. Second, we ask individuals to project how common noncompetes are within their occupations and industries, and then we aggregate those estimates into a single occupation- or industry-specific number. The idea behind using "projected estimates" as a way of estimating noncompete incidence is that an employee's knowledge of their occupation and industry as a whole captures more information than the employee's personal situation alone. See Rothschild and Wolfers (2013) for an example of this method in a voting context.

[21] Two indicia of the quality of our survey data are that legal occupations have the second lowest incidence level (10%) and that employees in these occupations are most likely to know whether they are bound by a noncompete. These facts are reassuring because one would expect that lawyers and legal support staff would be among the most careful readers of contracts and because the practice of law is the only occupation in which noncompetes are unenforceable in all states (Starr et al., 2018).

[22] With respect to the joint occupation-industry incidence distribution, Figure OA5 shows that the use of noncompetes is highest for technical occupations (computer, mathematical, engineering, architecture) in the manufacturing and information industries. Note that in the figure we only analyze occupation-industry cells for which there are at least 20 individuals in the sample in order to ensure that the results are representative.

[23] We define legitimate business interests as trade secrets, relationships with clients, and client information, such as contacts or marketing databases.

To provide some aggregate understanding across all of these characteristics, our simple multivariate model predicts that a salaried employee with a college degree, earning \$100,000 per year, with access to the employer's trade secrets, and in a private for-profit firm, has a 44% likelihood of being a party to a noncompete. As a point of comparison, an employee paid by the hour without a bachelor's degree, in a private for-profit firm, earning \$50,000 per year, and without access to the employer's trade secrets, has a 13% chance of being bound by a noncompete.

4 Negotiation and the Contracting Process

Table 7 presents descriptive statistics regarding the noncompete contracting process, including the extent of negotiation over noncompetes, when employers initially present noncompetes to applicants or employees, and whether employees consult with others before assenting to such a provision. Panel A shows that 61% of individuals with a noncompete first learn they will be asked to agree not to compete before accepting their job offer while more than 30% first learn they will be asked to agree only *after* they have already accepted their offer (but not with a promotion or change in responsibilities). This late notice appears to matter to employees. In a follow-up question to those who received late notice, 26% report that if they had known about their employer's noncompete plans earlier, they would have reconsidered accepting their offer.

Table 7 also shows that only 10% of employees report attempting to negotiate over the terms of their noncompete or asking for additional compensation or benefits in exchange for agreeing to such an employment condition. However, we find that the timing of noncompete notice is correlated with whether an individual makes an effort to bargain: 11.6% report negotiating when given early notice by their potential employer compared to just 6% of those given notice only after they have accepted their offer.[24] When presented with a noncompete, most respondents report just reading and signing it (88%), with a nontrivial fraction not even reading it (6.7%). Consultation with friends, family, or a lawyer is relatively uncommon (17%), but obtaining advice is strongly associated with attempting to negotiate.[25]

[24]By contrast, 31% of those asked to agree to a noncompete before a promotion or raise report negotiating over their noncompete, suggesting such circumstances allow employees a more favorable bargaining position.

[25]In unreported results, we also find that negotiation is twice as likely for those with a bachelor's degree relative to those without (13% vs. 6.2%) and that men are more likely to report negotiating than women (13% vs. 4.5%). Also, negotiation appears to be uncorrelated with noncompete enforceability—even after controlling for a host of characteristics such as employer size and employee age, gender, industry, occupation, and education.

In Table OB2, we report the reasons individuals cite for not attempting to negotiate over the terms of their noncompete (separately by the timing of notice). The top reasons for forgoing the opportunity to negotiate include that the terms were reasonable (52%) and the assumption that noncompetes were not negotiable (41%). Roughly 20% of employees fear creating tension with their employer or simply being fired if they try to negotiate.[26] In terms of heterogeneity by timing, those asked to agree not to compete after they have already accepted their offer are 9 percentage points less likely to report that they felt the terms were reasonable (55% vs. 46%) and are also 10 percentage points more likely to assume they could not negotiate (48% vs. 38%). In unreported tabulations, we also explore respondent beliefs about the consequences of refusing to agree to a noncompete. We asked respondents with noncompetes, "Would you still have been hired if you refused to sign the noncompete?" Only 11.4% answered affirmatively; 61.6% believed not, and 27% did not know. Taken together, the evidence in this section indicates that employers present (or employees receive) noncompete proposals as take-it-or-leave-it propositions.

5 Labor Market Outcomes

The traditional and critical perspectives on noncompetes offer different predictions about the extent to which employees with noncompetes should receive training and valuable information in their employment as well as whether employees who agree not to compete will be better off on the whole. In this section, we examine the conditional relationships between noncompetes and labor market outcomes. Given that contrasting views on noncompetes also highlight the role of late notice (as eroding employee bargaining power),[27] the enforceability of noncompetes (key to resolving the holdup problem), and effects over tenure (perhaps reflecting an upfront compensating differential), we also explore heterogeneity along these dimensions.

5.1 Empirical Approach

We begin by acknowledging that our analysis of the relationships between noncompete use and labor market outcomes (and the heterogeneity of these relationships across various contracting and

[26] For example, in an open text answer to a survey question, one respondent wrote "i needed the job [expletive], i wasn't trying to make any waves on the first day."

[27] We provide summary statistics by early and late notice in Table OB3.

legal dimensions) are best taken as descriptive and should not be interpreted causally. Noncompete use and the moderator variables we examine are endogenous.[28] Accordingly, any associations we observe may be at least partially due to reverse causation or selection on unobservables. To ease some concerns about this important limitation, we use several approaches to assess the sensitivity of our empirical results, including inspecting the robustness of our findings to the inclusion of a rich set of controls in our regression analysis, testing for selection on unobservables,[29] and asking respondents directly about their experiences with noncompetes.

Our investigation focuses on four critical employee outcomes: wages, training, access to information, and job satisfaction. Our main empirical specification takes the form:

$$Y_{iojs} = \beta_0 + \beta_1 Noncompete_i + \gamma X_{ij} + \omega_{o,j} + \alpha_s + \epsilon_{iojs}. \tag{1}$$

$Noncompete_i$ indicates whether the individual is bound by a noncompete. We study those who affirmatively report a current noncompete ("yes"), grouping "maybe" respondents with "no" respondents (and revisiting the robustness of our findings to this choice in our sensitivity analysis). Y_{iojs} refers variously to employment-related outcomes as reported by employee i in occupation o, industry j, and state s. We represent industry (NAICS 2-digit)-by-occupation (SOC 2-digit) fixed effects and state fixed effects with $\omega_{o,j}$ and α_s, respectively. In later models, we disaggregate our noncompete indicator to account for when the employee first learns about the employer's noncompete requirement (early- and late-notice), with individuals who do not have noncompetes

[28] We considered two possibilities for suitable instruments for noncompete status: differences in the enforcement regime and the projected incidence of noncompetes by *others in the same occupation and industry*. Both approaches yield implausible estimates (see Online Appendix D).

[29] Oster (2017) describes the key aspects of the test: If the R-squared statistic rises substantially as additional control variables are added and the estimate of the coefficient of interest remains stable, then there is less residual variation available to explain away a statistically significant estimate. If, however, the R-squared changes very little or the coefficient falls dramatically as controls are added to the model, then we should be less confident in the magnitude and direction of the estimate under review. Oster's (2017) test for selection bias delivers one parameter, δ, which indicates how powerful selection on unobservables would have to be, relative to the selection that occurs with respect to observables, to push the point estimate in question to zero. A value of $\delta = 1$ implies that selection on unobservables would have to be as important as selection on observables to fully account for an estimated nonzero coefficient while a value of $\delta > 1$ indicates that selection on unobservables would need to be even greater than selection on observables. To carry out the selection-bias test, we set the maximum R^2 at 30% higher than the R^2 in our fully saturated model, as Oster recommends. We also examine the reported δ terms by making comparisons (1) between a model with no controls and one with advanced controls and (2) between a model with basic controls (including state and occupation-by-industry fixed effects) and the advanced-controls model. We set the test's δ statistic equal to 1 as a natural cutoff to assess the stability of our results.

serving as the comparison group; we also examine models in which we interact $Noncompete_i$ with state-level noncompete enforceability and with length of tenure.[30]

Controls are given by X_{ij}, which we divide into "basic" and "advanced" groups in our analysis to gauge the sensitivity of our results to potentially confounding variables. Basic controls include demographic characteristics,[31] while the advanced controls address more noncompete-specific concerns.[32] These advanced controls in truth likely include some that are endogenous, potentially obscuring any causal mechanisms linking noncompete use and employee outcomes. Nevertheless, because we do not have reliably exogenous variation in the use of noncompetes to examine, it is informative to explore whether any noncompete-related patterns we observe survive when we condition on these potentially associated employment terms and conditions.[33]

5.2 Results

Table 8 reports the relationships we find between noncompete status and our four employment outcome: logged hourly wages and separate indicators for whether the respondent agrees or strongly agrees that their employer shares all job-related information with them, whether they received training in the last year, and whether they agree or strongly agree that they are satisfied with their

[30] We cluster our standard errors by state, tracking the level at which noncompetes are enforced (Moulton, 1990).

[31] Specifically, indicators for employee type (hourly, salaried, commission), gender, education, employer size, employer's multi-unit status, linear measures of an employee's hours worked per week, weeks worked per year, and their interaction, a third-degree polynomial in employee age, the logged number of employers in the county-industry cell, and the logged unemployment rate and labor force size in the state and year in which the employer hired the respondent (Beaudry and DiNardo, 1991). When necessary, logged variables take a log of the value plus one.

[32] Because noncompetes and other postemployment restrictive covenants (nondisclosure agreements, nonsolicitation provisions, and similar devices) frequently occur together (see Table 4), we disentangle and isolate any relationship between noncompetes and outcomes by controlling for these related provisions. If the use of postemployment provisions generally correlates with employer or employee quality or sophistication, controlling for them also accounts for any residual quality not addressed by our other controls. In addition, we include controls for poaching rates to and from the employer and within the industry generally to address employer heterogeneity in quality and employee-mobility patterns (for example, some employers are more likely to have their employees poached by competitors and so may be more likely to use noncompetes and may also pay different wages). We also control for other HR benefits, such as whether the employer offers a retirement plan, health insurance, paid vacation, sick leave, and life insurance. The inclusion of the HR-type benefits—retirement plan, paid vacation, sick leave, and life insurance—reduces the sample size from 11,462 to 11,010. Excluding these variables produces results that are nearly identical to our reported coefficients in terms of statistical significance and magnitude. See Tables OB4–OB7. Individuals with special access to sensitive information or who are predictable "flight risks" may also be more likely to have both noncompetes and higher earnings, so we control for the number of employers the employee has had in the last 5 years (a baseline measure of employee mobility) and the types of confidential information the employee possesses (for example, access to trade secrets or client information).

[33] In Online Appendix Tables OB4–OB7, we add our advanced controls sequentially so we can more precisely understand which, if any, shift our estimated noncompete coefficients.

employment.[34] Our baseline results in Panel A with our basic controls show that noncompete agreements are associated with positive differentials in wages and training. However, including our advanced controls reduces the training differential to near zero and causes the wages differential to fall from nearly 11% to 6.6%.[35] These results imply that certain advanced controls are strongly correlated with the use of noncompetes and these outcomes.

Panel B of Table 8 demonstrates that our mainly insignificant baseline results in Panel A are driven by heterogeneous associations that run in opposite directions, depending on when an employee receives notice of their noncompete. Focusing first on those who learn of their noncompete before they accept their job offer, our most saturated model indicates that these employees have 9.7% ($e^{0.093}$) higher earnings, are 4.3 percentage points more likely to have information shared with them (a 7.8% increase relative to the sample average), are 5.5 percentage points more likely to have received training in the last year (an 11% increase), and are 4.5 percentage points more likely to be satisfied in their job (a 6.6% increase) relative to those employees without a noncompete. In contrast, those presented with a noncompete after they accept their offer (excluding those furnished with a noncompete following a promotion or a change in responsibilities) appear to receive no observable boost in wages or training, are 13.4 percentage points less likely to have had information shared with them (a 24% reduction), and are 8.5 percentage points less likely to be satisfied in their employment (a 12.5% reduction). In all specifications but one,[36] within-model tests confirm that those who learn of their noncompete from their prospective employer before they accept that employer's offer do statistically significantly better (in terms of compensation, training, access to information, and satisfaction) relative to those who learn of and acquiesce to their noncompete only after they accept their employment offer.[37]

[34]For each dependent variable, we report results with basic controls and fixed effects as well as results with advanced controls. The results of the selection test (δ) for the comparison to a model with no controls is given in square brackets ('[ ]'), while the comparison between the models with basic and advanced controls is given in curly brackets ('{ }').

[35]As expected, given the large coefficient swings across these two models, the selection tests for the model with advanced controls confirm that we ought to be worried about selection on unobservables explaining our results. For example, when our model of logged hourly wages with advanced controls is compared to an otherwise equivalent model with no controls, the δ term is 0.497, implying that selection on unobservables would only need to be half as important as selection on observables to explain away our estimated coefficient on noncompete status.

[36]As we show in Table OB4, the lack of statistical significance on the before-after difference in the association between noncompete status and logged hourly wages only occurs when we control for HR benefits.

[37]The selection tests show that the statistically significant results for the late-notice category all have $\delta > 1$, while the results in the early-notice category are somewhat more sensitive (except in the satisfaction specification). For example, our results regarding hourly wages for the early notification group do appear rather sensitive to our advanced controls ($\delta = 0.275$), signifying that unobservables may more plausibly account for these estimates.

Given the limitations implicit in the cross-sectional nature of our data, we also study employee beliefs about what they were promised by and what they received from their employer for agreeing to their noncompete, as a way to independently—although only tentatively—corroborate the notice-timing differentials that we report in Table 8. The results, which we record in Table OB8, document that employees are rarely promised anything by their employers for agreeing to a noncompete, and, in fact, most of our survey respondents report having received nothing in exchange for their willingness to be bound by one. Moreover, as in Table 8, our findings indicate that employees who enter into late-notice noncompetes are relatively less likely to be promised and less likely to receive anything in exchange for their commitment not to compete.[38]

5.3 Sensitivity Analyses

To probe the robustness of the relationships we observe between noncompete status and employee outcomes, we investigate the consequences for our findings of treating the "maybe" scenarios as a separate contracting category as well as using multiple imputation to reassign members of the "maybe" group. Both approaches yield very similar results with respect to our notice-timing analysis, though the generally positive association that we estimate in Panel A of Table 8 between noncompetes and wages largely disappears when we use multiple imputation (see Online Appendix Tables OB9 and OB10). We also rerun our analysis without incorporating sample weights and find that none of our results is sensitive to weighting (see Table OB11). In Online Appendix Table OB12, we drop the respondents who indicated they took the survey to discuss their noncompete. Our timing results remain robust to this exclusion, though again the average main effect of a noncompete on wages mostly evaporates (as in the multiple imputation analysis). Finally, in Online Appendix Table OB13, we examine a related set of subjective employee outcomes, including perceived job security, the employer's commitment to upgrading the employee's skills, and whether the employee would consider returning to their employer if they were ever to leave. The results are broadly consistent with our earlier findings.[39]

[38] The precise question in Panel A is: "*Which of the following benefits did your employer promise you [beyond employment alone], either explicitly or implicitly, in exchange for signing the noncompete?*" The precise question in Panel B is "*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?*" The survey instrument captures objective outcome measures before it asks these more subjective questions so as not to contaminate the objective measures.

[39] Individuals who become aware of their noncompete upfront are more likely to report that their employer is committed to upgrading their skills relative to those who receive late notice. We also find that those who receive

5.4 Heterogeneity by Tenure and Noncompete Enforceability

In Figure 9, we study whether notice-timing differentials vary by tenure, cognisant that interpreting results later in tenure is troublesome given that tenure itself is endogenous to noncompete status (Starr et al., forthcoming). Within each tenure bin, we rerun our timing specification and report the coefficient and the 90% confidence interval on our early- and late-notice coefficients relative to the baseline outcome for individuals without a noncompete. Early notice is associated with positive compensating earnings differentials early in tenure (Panel A), with higher (but imprecisely estimated) probabilities of receiving training. We also observe negative job-satisfaction and information-access differentials within the first five years for those who agree to their noncompete after accepting their offer of employment (relative to those without noncompetes).

Given the importance of noncompete enforceability for theories justifying noncompetes as a solution to the employer's investment holdup problem, and given that previous empirical work on noncompetes has relied heavily on state-level enforceability,[40] we also estimate models examining the differential relationship of noncompetes in states where such provisions are relatively more or less enforceable.[41] In Table 9, we report estimates with and without state fixed effects (which, when included, subsume the main effect of enforceability). Consistent with prior research examining noncompete enforceability and wages but inconsistent with our main effect of noncompetes, we find that noncompetes in higher enforceability regimes are associated with relatively lower earnings (Balasubramanian et al., 2020; Garmaise, 2009). We also discover that noncompetes in states that are more likely to enforce them are associated with more training, as in Starr (2019). Panel B shows that the negative effects on wages appear invariant to the timing of noncompete notice. By contrast, the relative training benefits we observe in column (6) of Panel B accrue primarily to those who receive early notice of their noncompete.

late notice are less likely (than someone without a noncompete) to consider returning to their employer. Late notice is always associated with statistically significantly worse outcomes relative to early notice.

[40]We discuss noncompete enforceability and measures from a recent study in Online Appendix C.

[41]We use the enforceability measure developed in Starr (2019), which is denominated in standard deviations from a mean enforcement score of zero, and we modify our main timing specification by adding enforceability and its interaction with noncompete status as regressors.

6 Discussion and Conclusion

Motivated by renewed and widespread legislative interest in noncompetes as well as the longstanding debate over their value, our study brings new data and several new findings to the academic and policy conversations about noncompetes and related provisions that regulate employee behavior post-termination: How common is such contracting? What does it look like in practice and what types of employees are bound and to what kinds of employers? How does it relate to employee outcomes? In this section, we consider how the evidence we uncover with respect to noncompete incidence, contracting, and associated labor market success comports with predictions from the traditional and more critical perspectives on noncompetes.

Several of the facts we document are consistent with the traditional economic perspective, which views the noncompete as an efficient contracting device. For instance, our findings that noncompetes are more common in relatively technical jobs and among employees with access to trade secrets aligns with the hypothesis that noncompetes can be effective at protecting valuable information and training, thereby encouraging efficient employer investments. Moreover, our evidence that employees with early notice of a noncompete are compensated—with higher wages, more training, information, and job satisfaction—is compatible with theories that identify noncompetes as a solution to a holdup problem (Rubin and Shedd, 1981; Acemoglu and Pischke, 1999).[42] Our result that employees with early-notice noncompetes have higher wages earlier in tenure is also consistent with an upfront compensating differential (Callahan, 1985).

But the frequency of noncompetes among low-wage employees without access to trade secrets and the lack of negotiation in the contracting process hint at more anticompetitive rationales for the use of noncompetes by employers. We observe, for instance, that late-notice noncompetes are not associated with any additional compensation or training but instead appear to be linked to lower job satisfaction. Heterogeneous associations by noncompete enforceability further challenge the traditional economic perspective. The ability to enforce noncompetes should encourage greater noncompete use, more investment, and higher wages, but employers use noncompetes virtually as often in states where they are clearly unenforceable. Furthermore, while greater enforceability is associated with more training for individuals with early-notice noncompetes, the wage premium

[42] The fact that this noncompete-associated boost in training appears to come earlier in tenure imply that employers may use noncompetes to differentiate "stayers" from "leavers" (Loewenstein and Spletzer, 1997).

for agreeing to a noncompete also diminishes with enforceability, regardless of noncompete timing. This pattern is consistent with the idea that enforceability creates incentives for employers to invest in their bound employees, but it is at odds with the supposition that employees should likewise benefit from agreeing to such a provision.[43] Importantly, these enforceability-specific findings with respect to wages and training also align with prior work studying the effects of noncompete enforceability (Starr, 2019; Balasubramanian et al., 2020).[44]

Our empirical work answers several questions about the use of noncompetes, the contracting process, and labor market associations, but unresolved endogeneity concerns related to noncompete status and timing raise significant questions about how best to interpret our results. For example, we are unable to rule out the possibility that some unobservable association explains our outcome results—such as unobservably "good" employers using early-notice noncompetes and unobservably "bad" employers using late-notice noncompetes. Some of our findings also beg important questions. For instance, if indeed employers can use late-notice noncompetes to avoid compensating employees for giving up their right to compete (and somehow employees do not anticipate this tactic), then why are all employers not springing noncompetes on new employees? Potential explanations include the possibilities that late notice may produce low morale and lower productivity in some contexts and that, if suing to enforce a noncompete is a realistic possibility, judges may look down on any employer giving late notice. We search for determinants of noncompete timing in Table OB14 but find few predictive relationships.

There are several additional limitations to our work that we hope future research will address. First, given the lack of information on the actual use of noncompetes (and related provisions) across the labor force, and the possibility that our online survey approach may not generate data truly representative of the population, future survey efforts to collect longitudinal data on noncompete contracting, which could allow for the study of employee and employer outcomes over time, are sorely needed.[45] Relatedly, our finding that lower-earning employees are less likely

[43]This training finding is also consistent with the idea that employers may use early-notice noncompetes in cases where they may need to convince a judge of an agreement's reasonableness.

[44]While we designed our research to assess the discrepancies between the two main perspectives on noncompetes, we can also rule out the possibility that employers are using noncompetes as a way to sort between committed and uncommitted employees. Figure OA1 shows that employees are no more likely to accept a noncompete if they plan to stay indefinitely versus just a few years, and Figure OA2 similarly finds that noncompetes are only slightly more common when an individual has had many employers in the last 5 years.

[45]Data sets that already collect longitudinal data on employee mobility and entrepreneurship, such as the NLSY or the PSID, would be well suited to undertake this task. Companies such as Glassdoor.com or Indeed.com could

to know whether they are bound by a noncompete raises some uncertainty about our incidence results, and employer-level survey data or actual contracts could help resolve this ambiguity.

These remaining questions notwithstanding, we make several important contributions to our collective understanding of postemployment contractual restrictions and to the related body of work on transparency (Card et al., 2012; Harris, 2018) and labor market frictions (Naidu, 2010). Most concretely, we build on several occupation-specific studies (Marx, 2011; Schwab and Thomas, 2006) to document that noncompetes extend to every corner of the labor market. We also empirically characterize the typically take-it-or-leave-it contracting process surrounding noncompetes, and provide correlational evidence that noncompetes are not uniformly associated with better (or worse) employee outcomes—depending on the timing of notice in the contracting process and a noncompete's enforceability. Overall, the story about noncompetes that emerges from our data is complex and nuanced, drawing on both of the literature's dominant perspectives.

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also add a question on noncompetes in their intake survey and report it to interested job seekers to reduce the information asymmetry regarding the use of noncompetes.

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Figures

Figure 1: Noncompete Incidence by Age

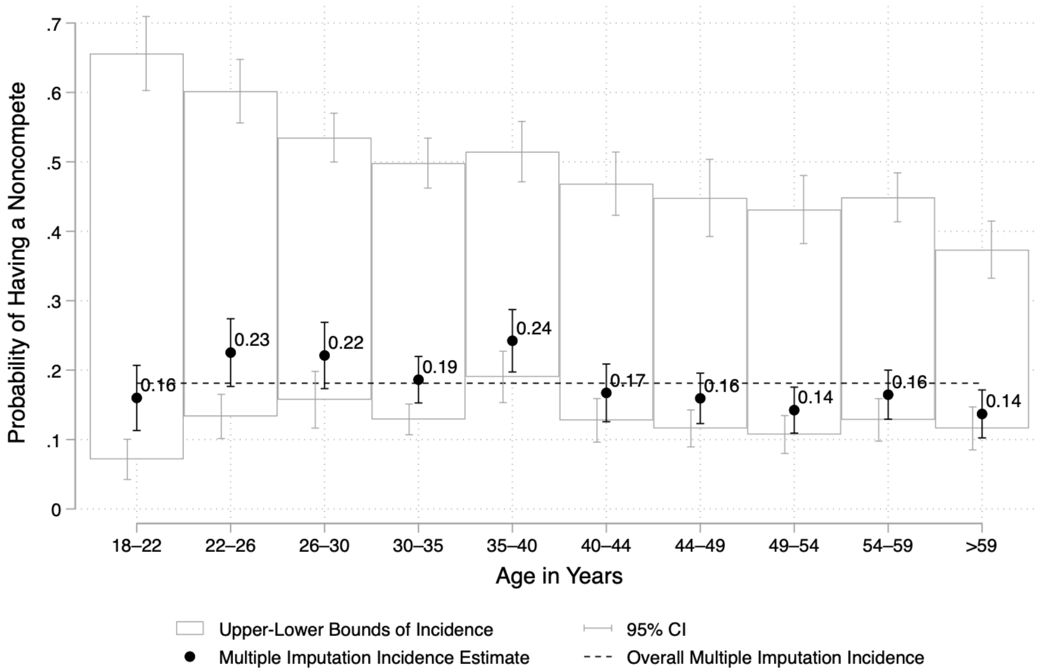


Figure 2: Noncompete Incidence by Number of Employers in County-Industry

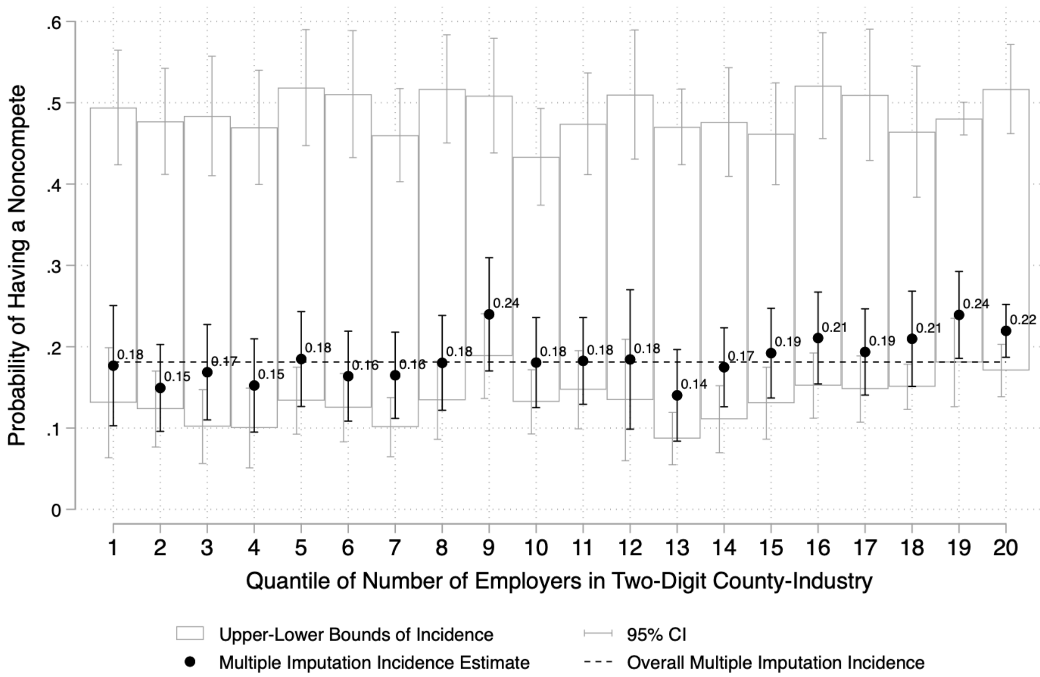


Figure 3: Noncompete Incidence by Education Level

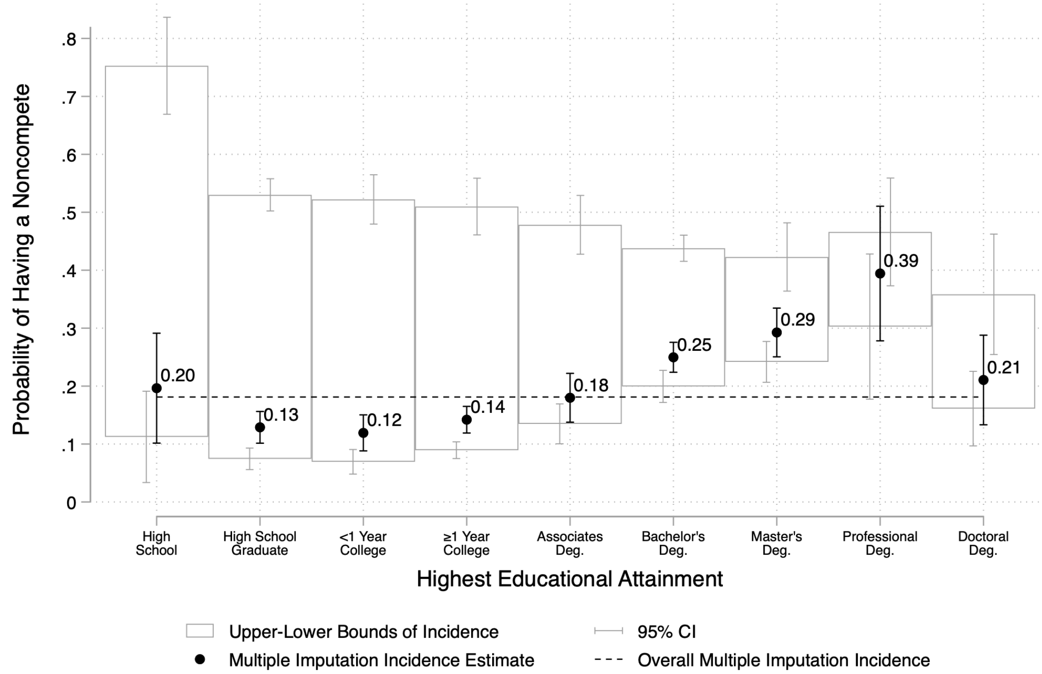


Figure 4: Noncompete Incidence by Employee Annual Earnings

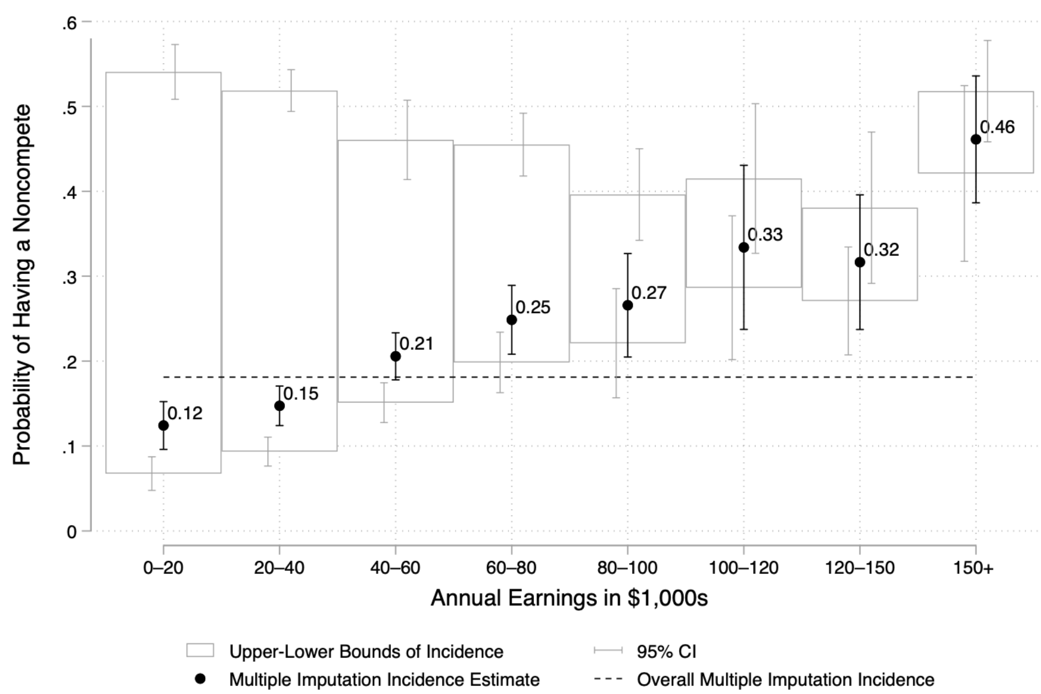


Figure 5: Noncompete Incidence by Occupation

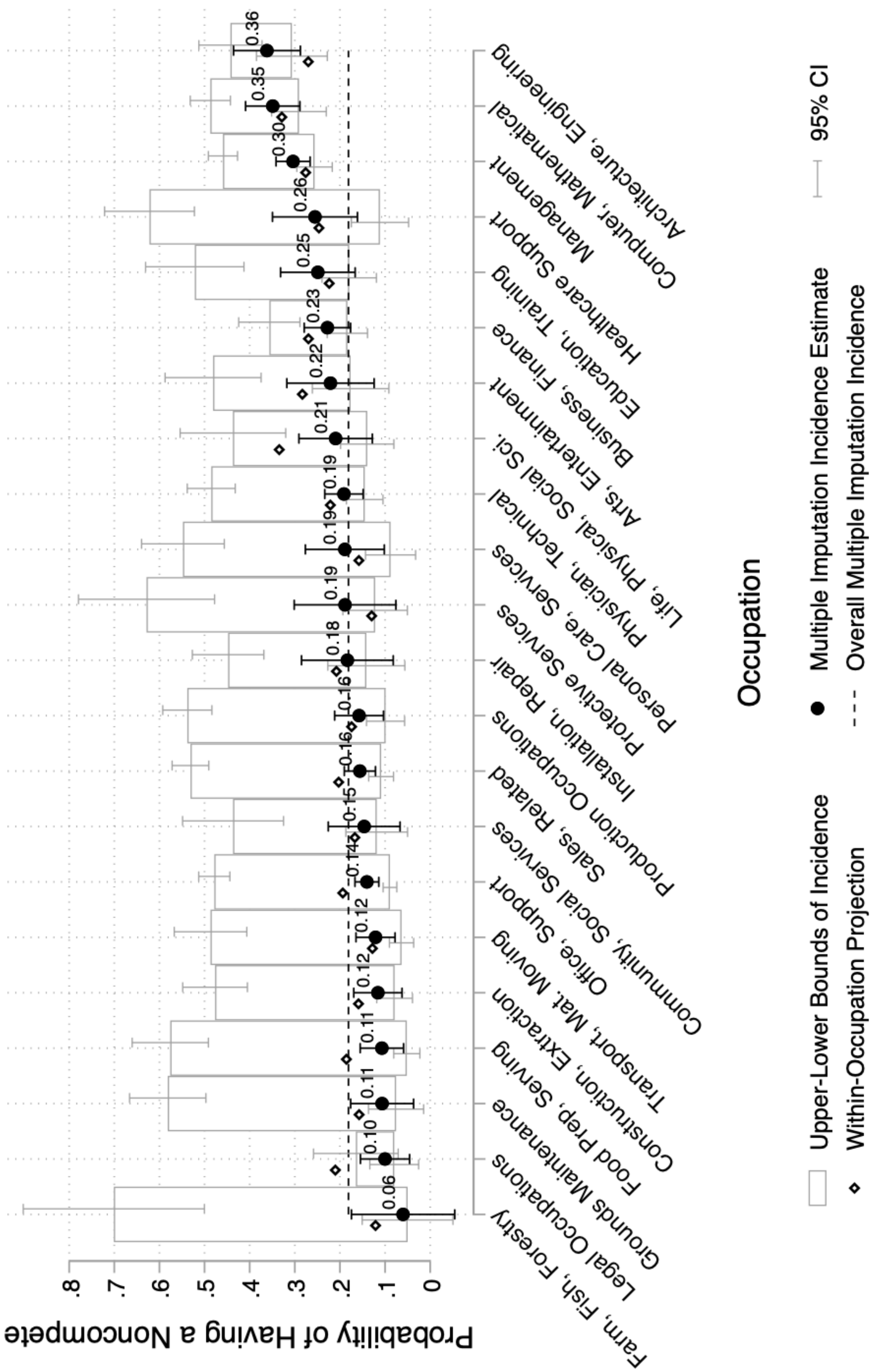


Figure 6: Noncompete Incidence by Industry

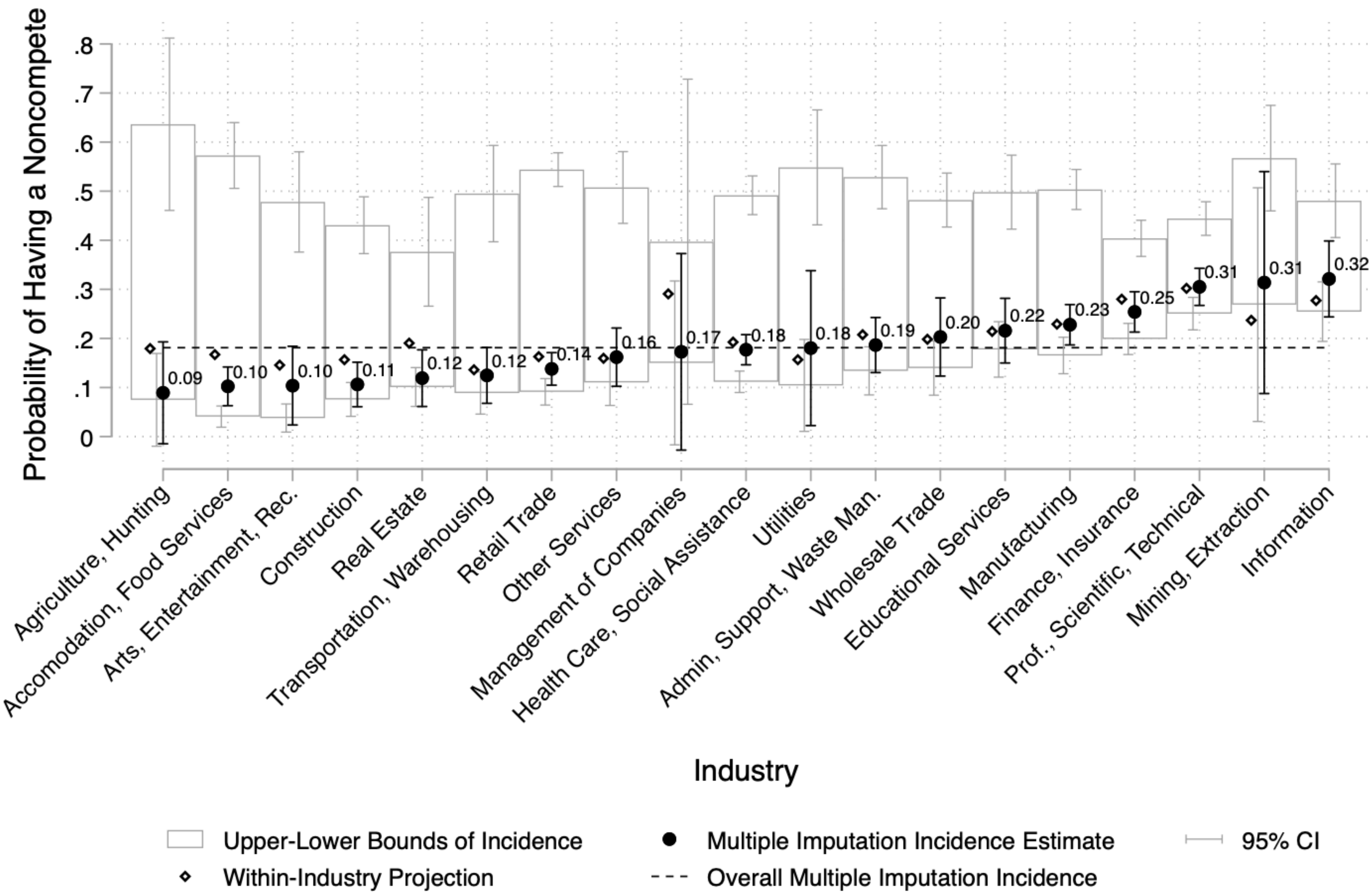


Figure 7: Noncompete Incidence by Legitimate Business Interest

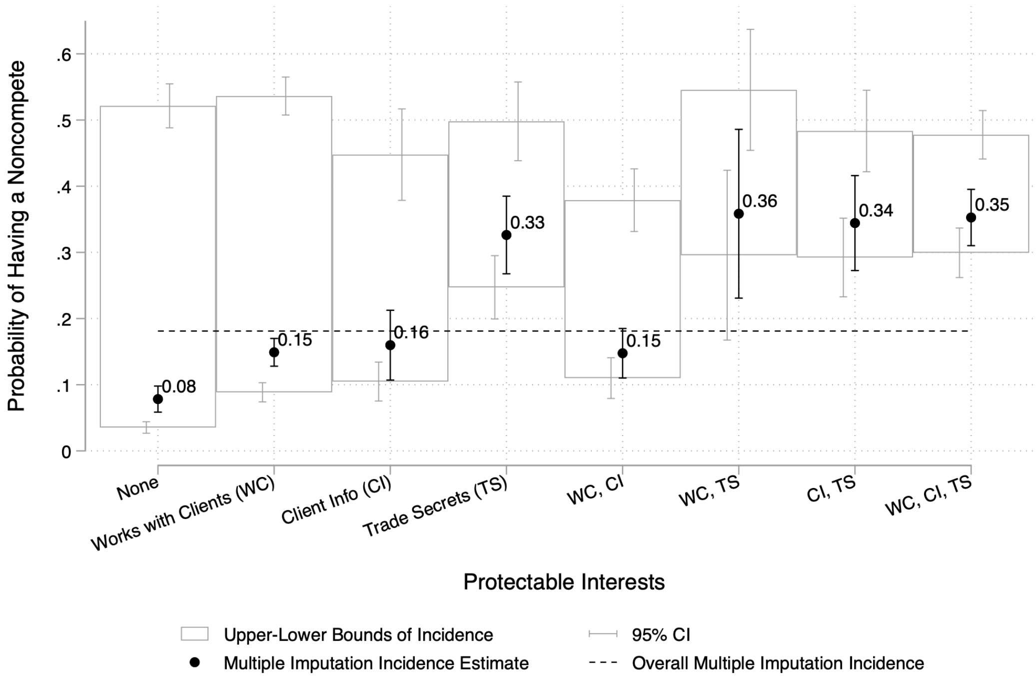


Figure 8: Noncompete Incidence by Noncompete Enforceability

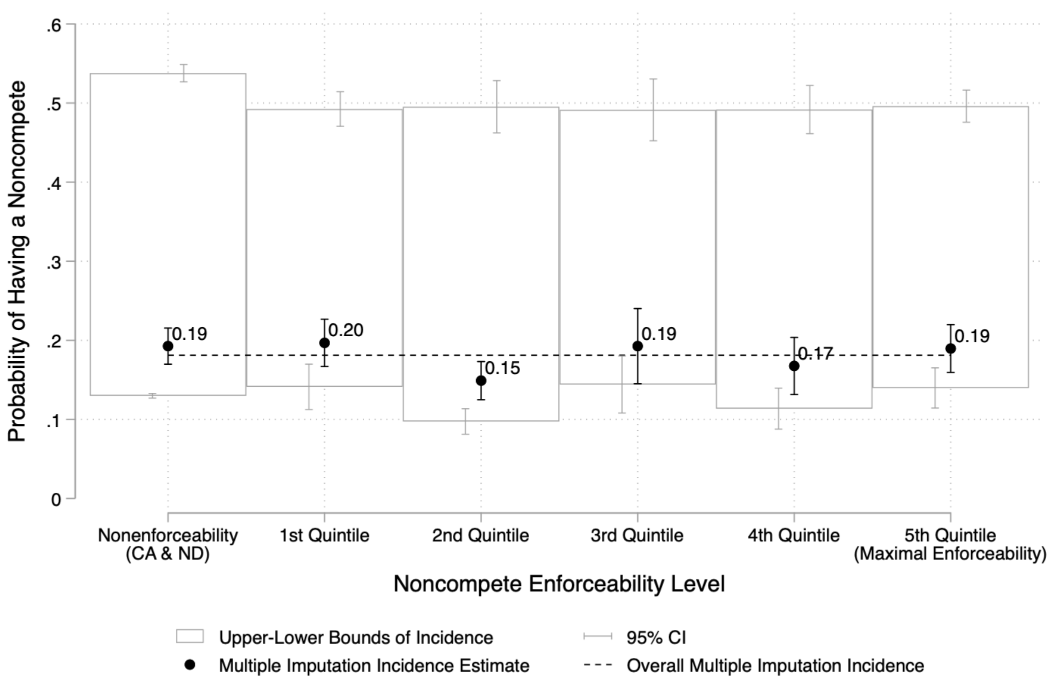
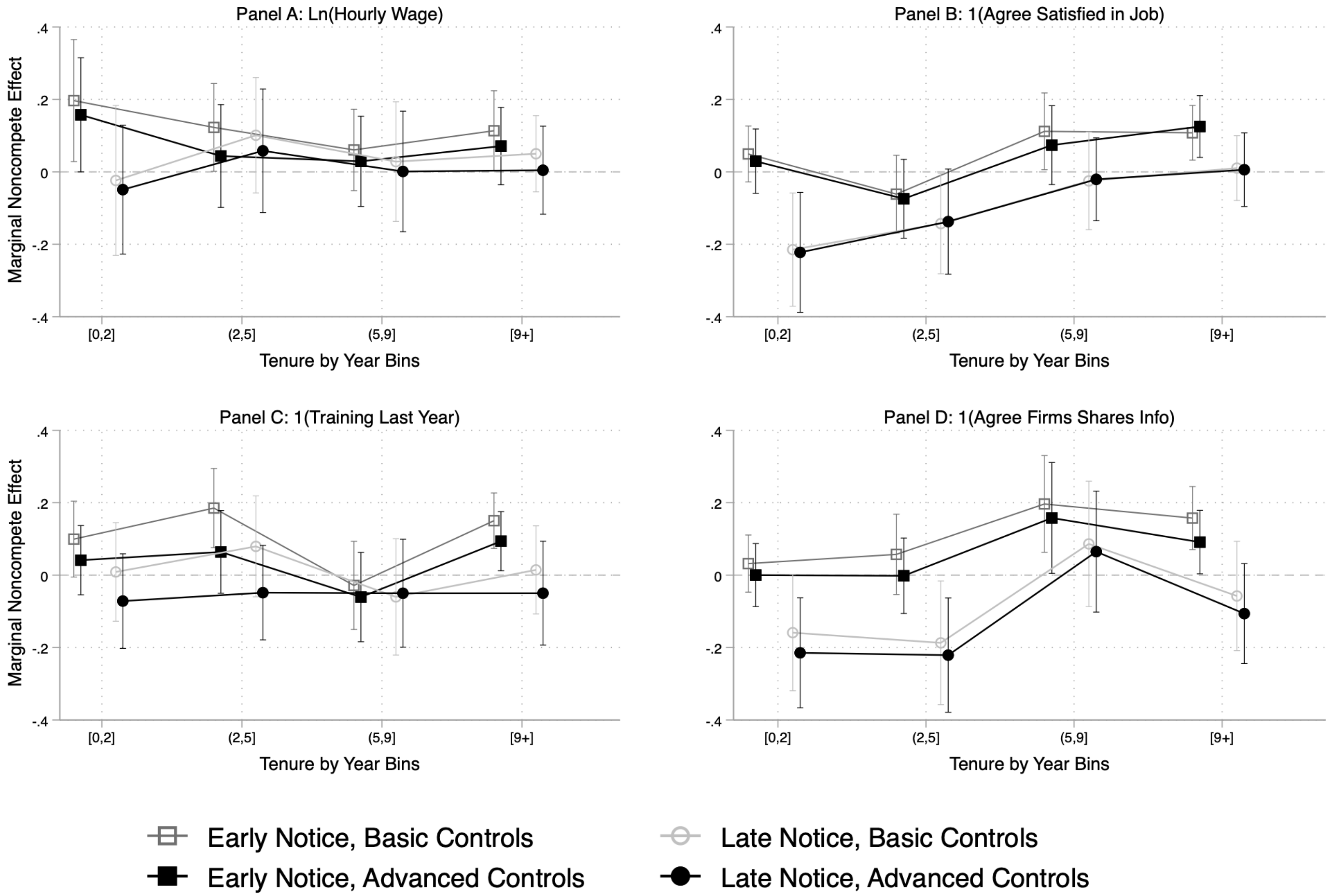


Figure 9: Marginal Effect of Noncompetes over Tenure



Tables

Table 1: Summary of Competing Perspectives on Noncompetes (and Findings)

		Predictions		
	Main Arguments	Use	Contracting Process	Labor Market Outcomes
Traditional Economic Perspective	Employers use noncompetes to solve a holdup problem, protecting "legitimate" employer interests, including trade secrets and client information, which creates incentives for efficient investment. Employees agree not to compete only when doing so makes them better off, either through an upfront compensating differential or through increased growth in wages over time.	Noncompetes should be confined to employees in occupations and industries that involve trade secrets, access to client lists, or other valuable information, or that require specialized industry-specific training. Noncompetes should also be confined to states that enforce noncompetees.	Noncompetes should be negotiated agreements that maximize joint employee-employer surplus and make both parties better off. Alternatively, a compensating differential should be a part of the initial job offer, rendering costly negotiation unnecessary.	Conditional on an employee's job duties, employees who agree to noncompetes should receive more training or valuable information relevant to their employment. Employees should earn higher wages as they capture some of the joint surplus created by the noncompete. Because they capture some of the surplus on average, employees with noncompetes should be more satisfied on job.
Critical Perspective	Noncompetes reduce labor-market competition from product-market competitors and future product market competition from departing employees. Employers can also deploy noncompetes at inopportune times to reduce applicant or employee bargaining power.	Noncompetes should be common for all sorts of employees, regardless of access to confidential information, whenever employers can use noncompetes to limit competition in labor or product markets. Noncompetes should be found where they are unenforceable if employee behavior is affected by noncompetes without regard to the prospect of actual enforcement.	If employers are in a position to impose noncompetes on applicants and employees, noncompetes should rarely be negotiated and employees should rarely seek outside advice since their actual options are few. Employers may opt for "late" noncompetes that reduce employee bargaining power.	Employees may not necessarily see any increase in training or information access since noncompetes may not be deployed to resolve incentive or holdup issues. Employees may also suffer in terms of lower wages and reduced job satisfaction if noncompetes are able to limit competition in labor markets.
		Findings		
		Noncompetes are more common for employees in technical jobs and industries and for employees who have access to valuable, confidential information. However, noncompetes are relatively common in all occupations and industries and bind many employees without access to trade secrets or client information. Noncompetes are also common in states that do not enforce them.	Employees and employers rarely negotiate over noncompetes, and a rarely promised anything in exchange for signing. About one-third of noncompetes are requested by employers after an employee has accepted their job offer (without a change in job title or responsibilities). Few individuals receive outside advice during the contracting process.	Noncompetes are associated with more training, greater access to information, and higher wages and job satisfaction when the noncompete is presented along with the job offer. If a noncompete is presented after the acceptance of a job offer, employees experience no wage or training benefits on average, and they report being less satisfied in their jobs. Higher wages appear to be largest in early tenure for employees receiving early notice, while lower job satisfaction appears early in tenure for late-notice noncompetes. Training associated with noncompetes increases with the enforceability of the noncompete, but wages fall.

Table 2: Comparison of Weighted and Unweighted Sample and 2014 American Community Survey

Variable	Sample Data		ACS	NSP-ACS Difference	
	Unweighted	Weighted		Unweighted	Weighted
Age (in years)	41.98	40.33	40.55	1.43**	-0.22
	(13.23)	(13.63)	(13.64)	(0.16)	(0.27)
Annual Income ($)	49,062	44,001	46,680	2,382**	-2,680
	(42033)	(47378)	(55622)	(769)	(1,748)
1(Work > 40 Hours/Week)	0.70	0.71	0.72	-0.02**	-0.01
	(0.46)	(0.45)	(0.45)	(0.00)	(0.01)
1(Highest Degree < BA)	0.48	0.69	0.70	-0.22**	-0.01
	(0.50)	(0.46)	(0.46)	(0.01)	(0.02)
1(Highest Degree = BA)	0.37	0.21	0.20	0.16**	0.01
	(0.48)	(0.41)	(0.40)	(0.01)	(0.01)
1(Highest Degree > BA)	0.16	0.10	0.097	0.06**	0.00
	(0.36)	(0.30)	(0.30)	(0.00)	(0.01)
1(Male)	0.47	0.53	0.53	-0.07**	-0.00
	(0.50)	(0.50)	(0.50)	(0.01)	(0.01)

Notes: This table shows the distributions of demographic characteristics in our sample data, both weighted and unweighted, and in data from the 2014 American Community Survey. The weighted data use raking weights, as described in the text and in Prescott et al. (2016). Note: ** p<0.01, * p<0.05, + p<0.1. We report standard deviations (first three columns) and robust standard errors (last two columns), clustered at the state level, in parentheses.

Table 3: Temporal and Geographic Scope of Noncompetes

(1) Term Duration	(2) Percent	(3) Geographic Limit	(4) Percent
Duration < 1 Year	30.9	Radius in Miles	7.3
1 < Duration ≤ 2 Years	15.0	City	5.9
Duration > 2 Years	33.8	County	6.1
Don't Know	20.3	MSA	6.0
		Within the State	13.9
		Entire U.S.	15.4
		No limit	23.1
		Other	3.30
		Don't Know	19.0

Notes: Column (2) shows the distribution of noncompete provision duration periods in the sample, while Column (4) shows the distribution of geographic boundaries of the competition prohibitions. The sample includes the 1,747 individuals bound by noncompetes and uses sample weights.

Table 4: Sample Means by Noncompete Use

Variable	Overall	Bound by Noncompete? No	Maybe	Yes	Δ Relative to "No" Group Maybe	Yes
Labor Market Outcomes						
Ln(Hourly Wage)	2.88	2.92	2.70	3.24	-0.23**	0.31**
1(Employer Shares Info)	0.55	0.57	0.50	0.59	-0.06**	0.02
1(Training Last Year)	0.50	0.52	0.44	0.64	-0.08**	0.12**
1(Satisfied in Job)	0.68	0.69	0.65	0.70	-0.04^{+}	0.01
Demographics						
1(Paid Hourly)	0.68	0.65	0.81	0.45	0.16**	-0.12**
1(Paid by Salary)	0.28	0.31	0.16	0.49	-0.15**	0.18**
1(Paid by Commission)	0.03	0.03	0.02	0.04	-0.01*	0.02
1(Paid by Other Means)	0.01	0.01	0.01	0.01	-0.00	-0.00
Age (in years)	40.28	42.33	37.54	40.22	-4.79**	-2.11**
Hours Worked per Week	37.59	37.92	35.87	41.27	-2.05**	3.34**
Weeks Worked per Year	47.81	48.31	46.96	48.33	-1.35**	0.02
1(Male)	0.53	0.56	0.47	0.58	-0.08**	0.03
1(Private For-Profit Employer)	0.90	0.90	0.87	0.96	-0.03**	0.06**
1(Private Nonprofit Employer)	0.06	0.07	0.07	0.02	0.01	-0.05**
1(Public Health System Employer)	0.04	0.03	0.05	0.02	0.02**	-0.01*
1(Highest Degree < BA)	0.69	0.65	0.81	0.48	0.17**	-0.17**
1(Highest Degree = BA)	0.21	0.24	0.14	0.33	-0.10**	0.09**
1(Highest Degree > BA)	0.1	0.12	0.05	0.19	-0.07**	0.08**
Ln(State Unemployment Rate at Hire)	1.9	1.88	1.92	1.89	0.04**	0.01
Ln(Labor Force Size in State at Hire)	15.35	15.33	15.35	15.41	0.02	0.07*
Ln(Establishments in County-Industry)	6.47	6.47	6.4	6.68	-0.07	0.21*
1(Employer Size < 25)	0.23	0.25	0.23	0.15	-0.02^{+}	-0.10**
1(Employer Size 25–100)	0.16	0.16	0.16	0.15	-0.00	-0.00
1(Employer Size 101–250)	0.09	0.1	0.09	0.1	-0.01	0.00
1(Employer Size 251–500)	0.07	0.08	0.06	0.09	-0.01	0.02^{+}
1(Employer Size 501–1,000)	0.07	0.07	0.07	0.07	0.01	0.00
1(Employer Size 1,001–2,500)	0.07	0.06	0.07	0.07	0.01	0.01
1(Employer Size 2,501–5,000)	0.07	0.06	0.08	0.08	0.02*	0.02*
1(Employer Size > 5,000)	0.24	0.23	0.24	0.29	0.01	0.06**
1(Multi-Unit Employer)	0.63	0.61	0.62	0.73	0.00	0.12**
Other Post-Employment Restrictive Covenants						
1(Nondisclosure)	0.36	0.3	0.3	0.75	-0.00	0.44**
1(Nonpoaching)	0.04	0.02	0.02	0.18	-0.00	0.15**
1(Nonsolicit)	0.12	0.08	0.09	0.35	0.01	0.27**
1(Arbitration)	0.08	0.06	0.05	0.19	-0.01	0.13**
1(IP Assignment)	0.09	0.08	0.05	0.28	-0.03**	0.20**
Observations	11,505	6,344	3,414	1,747		

Notes: This table reports the weighted sample means for the full sample as well as for respondents who report working under a noncompete (15.1% of the unweighted sample), respondents who indicate that they were not bound by a noncompete (55.1% of the unweighted sample), and the "maybe" group of respondents (29.7% of the unweighted sample). Recall that 83.5% of the "maybe" category are in that category because they indicate that they have never heard of a noncompete. ** p<0.01, * p<0.05, $^{+}$p<0.1. We use robust standard errors, clustered at the state level, when testing differences between categories.

Table 5: Noncompete Use By Employee Characteristics

Characteristic	% Currently Bound by Noncompete	% Ever Bound by Noncompete
Employer Class		
Private For-Profit	19.0	38.8
Private Nonprofit	9.8	28.6
Public Healthcare	12.4	37.8
Gender		
Female	17.3	36.3
Male	18.8	39.7
Age in Years		
Under Age 40	20.6	38.7
Age 40 or Older	15.6	37.5
Highest Level of Education		
< Bachelor's Degree	14.3	34.7
Bachelor's Degree	25.0	43.8
> Bachelor's Degree	30.0	49.0
Compensation Type		
Hourly	14.0	33.7
Salary	27.5	47.7
Other	23.6	45.9
Annual Earnings		
< $40,000	13.3	33.0
≥ $40,000	25.2	45.6
Confidential Information		
Works with Clients (WC)	14.9	35.6
Access to Client Information (CI)	16.0	36.2
Access to Trade Secrets (TS)	32.6	54.9
WC, CI	14.8	31.3
WC, TS	35.8	53.4
CI, TS	34.4	58.3
WC, CI, TS	35.3	56.2
None	7.8	26.9
Employer Size		
< 25 Employees	11.6	33.6
25–100 Employees	17.7	36.5
101–250 Employees	19.1	40.6
251–500 Employees	22.3	40.9
501–1,000 Employees	16.8	39.1
1,001–2,500 Employees	21.2	42.3
2,501–5,000 Employees	21.0	44.2
> 5,000 Employees	21.5	38.3
Overall	18.1	38.1

Notes: This table shows descriptive statistics related to whether an employee was bound by a noncompete in 2014 ("currently bound') or had ever been bound by a noncompete. The reported incidence statistics we show are from the multiple imputation approach we describe in the text. We weight all estimates.

Table 6: Determinants of Noncompete Status

	Panel A: Compare Yes, No, and Maybe			*Panel B: Yes vs. Maybe or No*
	Multinomial Logit			OLS
	(1) Maybe	(2) No	(3) Yes	(4) **1**(Noncompete)
Ln(Hourly Wage)	-0.037*	0.006	0.031**	0.029**
	(0.017)	(0.018)	(0.011)	(0.010)
1(Private Nonprofit Employer)	0.042	0.039	-0.081**	-0.071**
	(0.033)	(0.033)	(0.014)	(0.015)
1(Public Health System Employer)	0.087*	-0.034	-0.053*	-0.054*
	(0.042)	(0.036)	(0.022)	(0.025)
1(Works with Clients (WC))	-0.058**	0.004	0.053**	0.044**
	(0.020)	(0.023)	(0.008)	(0.009)
1(Access to Client Information (CI))	-0.121**	0.055	0.066**	0.055**
	(0.038)	(0.041)	(0.018)	(0.018)
1(Access to Trade Secrets (TS))	-0.178**	0.021	0.157**	0.161**
	(0.024)	(0.028)	(0.018)	(0.021)
1(WC, CI)	-0.193**	0.132**	0.061**	0.051**
	(0.027)	(0.031)	(0.013)	(0.014)
1(WC, TS)	-0.217**	-0.013	0.230**	0.227**
	(0.042)	(0.046)	(0.049)	(0.056)
1(CI, TS)	-0.194**	0.016	0.178**	0.191**
	(0.031)	(0.029)	(0.022)	(0.026)
1(WC, CI, TS)	-0.240**	0.039	0.201**	0.209**
	(0.027)	(0.026)	(0.015)	(0.017)
1(1st Enforceabilility Quintile)	-0.109**	0.068**	0.041**	0.046**
	(0.026)	(0.018)	(0.013)	(0.015)
1(2nd Enforceability Quintile)	-0.100**	0.073**	0.027*	0.033*
	(0.021)	(0.019)	(0.012)	(0.013)
1(3rd Enforceability Quintile)	-0.136**	0.074**	0.062**	0.066**
	(0.026)	(0.025)	(0.019)	(0.022)
1(4th Enforceability Quintile)	-0.118**	0.084**	0.035*	0.039*
	(0.021)	(0.024)	(0.017)	(0.018)
1(5th Enforceability Quintile)	-0.111**	0.064**	0.047**	0.052**
	(0.026)	(0.017)	(0.015)	(0.017)
1(2 Employers in Last 5 Years)	-0.026	0.027	-0.002	-0.000
	(0.021)	(0.022)	(0.011)	(0.011)
1(3–4 Employers in Last 5 Years)	0.010	-0.014	0.004	0.004
	(0.020)	(0.029)	(0.018)	(0.017)
1($>$ 4 Employers in Last 5 Years)	0.048*	-0.056*	0.008	0.007
	(0.022)	(0.023)	(0.018)	(0.019)
1(**E**[Duration 1–2 Years)	0.064^{+}	-0.014	-0.050	-0.047
	(0.039)	(0.042)	(0.032)	(0.029)
1(**E**[Duration 2–4 Years)	0.038	0.015	-0.053*	-0.048^{+}
	(0.040)	(0.036)	(0.027)	(0.026)
1(**E**[Duration 4–10 Years)	0.056	-0.027	-0.029	-0.024
	(0.035)	(0.035)	(0.028)	(0.026)
1(**E**[Duration $>$ 10 Years)	0.122*	-0.095^{+}	-0.028	-0.009
	(0.062)	(0.051)	(0.037)	(0.038)
1(**E**[Duration Indefinite)	0.059^{+}	-0.015	-0.044^{+}	-0.038^{+}
	(0.032)	(0.035)	(0.025)	(0.022)
1(Paid by Salary)	-0.049**	0.015	0.034*	0.040*
	(0.018)	(0.017)	(0.015)	(0.016)

Continued on next page

Table 6 – continued from previous page

1(Paid by Commision)	-0.116**	-0.006	0.121**	0.111*
	(0.042)	(0.042)	(0.041)	(0.044)
1(Paid by Other Means)	-0.012	-0.018	0.030	0.018
	(0.062)	(0.065)	(0.044)	(0.036)
Age (in years)	-0.005**	0.005**	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Hours Worked per Week	0.000	-0.001	0.001	0.001
	(0.001)	(0.001)	(0.000)	(0.000)
Weeks Worked per Year	-0.003*	0.002^{+}	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
1(Male)	-0.024	0.047**	-0.023*	-0.021*
	(0.015)	(0.016)	(0.011)	(0.010)
1(Highest Degree = BA)	-0.108**	0.076**	0.032**	0.041**
	(0.017)	(0.019)	(0.010)	(0.012)
1(Highest Degree > BA)	-0.119**	0.085**	0.033*	0.051**
	(0.029)	(0.029)	(0.014)	(0.018)
Ln(State Unemployment Rate at Hire)	0.001	-0.023	0.022	0.020
	(0.027)	(0.024)	(0.016)	(0.016)
Ln(Labor Force Size in State at Hire)	-0.008	0.005	0.003	0.002
	(0.010)	(0.009)	(0.007)	(0.007)
1(Multi-Unit Employer)	-0.033	0.000	0.032**	0.034**
	(0.022)	(0.027)	(0.011)	(0.012)
1(Employer Size 25–100)	0.016	-0.046*	0.031^{+}	0.022
	(0.018)	(0.024)	(0.016)	(0.016)
1(Employer Size 101–250)	0.017	-0.038	0.022	0.016
	(0.027)	(0.025)	(0.017)	(0.018)
1(Employer Size 251–500)	-0.003	-0.035	0.038*	0.033
	(0.032)	(0.031)	(0.019)	(0.020)
1(Employer Size 501–1,000)	0.059^{+}	-0.076*	0.017	0.010
	(0.033)	(0.038)	(0.025)	(0.027)
1(Employer Size 1,001–2,500)	0.054	-0.078^{+}	0.024^{+}	0.018
	(0.047)	(0.046)	(0.013)	(0.015)
1(Employer Size 2,501–5,000)	0.088**	-0.106**	0.019	0.013
	(0.028)	(0.027)	(0.018)	(0.019)
1(Employer Size > 5,000)	0.046^{+}	-0.078**	0.032^{+}	0.025
	(0.026)	(0.027)	(0.017)	(0.017)
Ln(Establishments in County-Industry)	0.006	-0.007	0.001	0.002
	(0.005)	(0.005)	(0.003)	(0.003)
Observations	11,462	11,462	11,462	11,462
Mean R-Squared				0.139
Occupation and Industry FE	Yes	Yes	Yes	Yes

Notes: Panel A shows the marginal increase in the probabilty of falling into the "maybe," "yes," or "no" noncompete categories from a unit increase in the variable in the left-hand column. Each row adds to zero in Panel A because increases in the probability of being in one category are offset by lower chances of being in another. Panel B is a linear probability model in which the dependent variable is an indicator for agreeing to a noncompete, where those in the "maybe" category are grouped with those in the "no" category. The omitted enforceability group is the set of nonenforcing states (North Dakota and California) and the measure of noncompete enforceability is taken from Starr (2019). ** p<0.01, * p<0.05, $^{+}$ p<0.1. We report robust standard errors in parentheses, clustered at the state level.

Table 7: The Noncompete Contracting Process

	(1) Distribution (%)	(2) % Negotiate
Panel A: When did you first learn you would be asked to sign a noncompete?		
Before Accepting Job Offer	60.8	11.6
After Accepting Job Offer	29.3	6.3
Before Promotion or Raise	2.2	30.8
Other or Cannot Remember	7.7	6.5
Panel B: What did you do when asked to sign?		
Signed without Reading	6.7	7.9
Read Quickly and Signed	31.2	7.1
Read Slowly and Signed	56.4	11.6
Consulted with Friends/Family	10.4	30.8
Consulted a Lawyer	7.9	48.6
Overall		10.1

Notes: The "Distribution (%)" column (1) shows the percentage of individuals in each category for each question (panel). The "% Negotiate" column (2) records the percentage of individuals in the row who report negotiating over the terms of their noncompete or for other benefits in exchange for agreeing not to compete. The first two rows in Panel A ("before" and "after") refer to noncompetes agreed to without a change in job title or duties, whereas the third row addresses noncompetes signed as part of a promotion.

Table 8: Labor Market Outcomes

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Ln(Hourly Wage)		**1**(Employer Shares Info)		**1**(Training Last Year)		**1**(Satisfied in Job)	
			Panel A: Baseline					
Noncompete	0.109**	0.066**	0.031	-0.020	0.077**	0.006	0.015	0.006
	(0.026)	(0.023)	(0.030)	(0.025)	(0.019)	(0.019)	(0.019)	(0.017)
	[1.033]	[0.497]	[1.361]	[0.715]	[1.180]	[0.104]	[1.463]	[1.399]
		{0.216}		{0.302}		{0.048}		{0.829}
R-Squared	0.503	0.541	0.100	0.146	0.160	0.199	0.099	0.149
			Panel B: Heterogeneity by Timing of Notice					
First Learned of Noncompete								
Before Accepting Job	0.143**	0.093**	0.101**	0.043^{+}	0.131**	0.055*	0.060**	0.045*
	(0.033)	(0.031)	(0.026)	(0.024)	(0.024)	(0.025)	(0.020)	(0.020)
	[1.220]	[0.638]	[4.067]	[1.254]	[1.954]	[0.920]	[4.120]	[3.846]
		{0.275}		{0.518}		{0.406}		{1.972}
After Accepting Job	0.057	0.024	-0.093^{+}	-0.134**	0.017	-0.058	-0.090*	-0.085*
	(0.042)	(0.037)	(0.050)	(0.039)	(0.035)	(0.039)	(0.036)	(0.035)
	[0.759]	[0.316]	[11.830]	[8.474]	[0.089]	[1.112]	[7.862]	[9.004]
		{0.151}		{3.097}		{0.480}		{6.978}
With Promotion	0.202*	0.136	0.039	0.011	-0.060	-0.125	0.070	0.051
	(0.090)	(0.086)	(0.089)	(0.104)	(0.097)	(0.113)	(0.067)	(0.071)
	[1.226]	[0.741]	[0.653]	[0.307]	[0.637]	[2.221]	[1.375]	[2.385]
		{0.269}		{0.186}		{0.850}		{9.855}
Doesn't Remember	0.020	0.010	-0.049	-0.073	-0.076	-0.093	0.043	0.042
	(0.056)	(0.064)	(0.076)	(0.064)	(0.069)	(0.064)	(0.044)	(0.047)
	[1.226]	[0.146]	[0.653]	[4.343]	[0.637]	[4.668]	[1.375]	[4.866]
		{0.506}		{2.164}		{4.559}		{40.34}
P-value: $\beta_{Before} = \beta_{After}$	0.062	0.127	0.000	0.000	0.014	0.021	0.000	0.000
R-Squared	0.503	0.541	0.104	0.150	0.162	0.201	0.102	0.151
Observations	11,462	11,010	11,462	11,010	11,462	11,010	11,462	11,010
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Panel A examines the aggregate association of having a noncompete with the outcome of the column, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Tables OB9 and OB10 in the Online Appendix show the results of treating the "maybe" group as a separate category and imputing "yes" or "no" status for each respondent in the "maybe" group, respectively. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. We define the variables that make up our basic and advanced controls on page 11. We report the selection test relative to a model with no controls in square brackets ('[ ]'), and we report the selection test between the models with basic and advanced controls in curly brackets ('{ }'). In both cases, the selection test statistic is calculated with the Stata command `psacalc`, using as the maximum R-Squared Oster's suggested 30% more than the R-Squared from the model that includes both the basic and advanced controls. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table 9: Labor Market Outcomes: Heterogeneity by Noncompete Enforceability

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Ln(Hourly Wage)		**1**(Employer Shares Info)		**1**(Training Last Year)		**1**(Satisfied in Job)	
			Panel A: Baseline					
Enforceability	-0.014^{+}		0.004		-0.002		0.006	
	(0.007)		(0.006)		(0.006)		(0.005)	
Noncompete	0.106**	0.063*	0.030	-0.020	0.081**	0.010	0.008	0.004
	(0.027)	(0.024)	(0.028)	(0.025)	(0.020)	(0.020)	(0.017)	(0.018)
Enforceability×Noncompete	-0.025*	-0.017^{+}	0.004	0.001	0.021*	0.020**	-0.014	-0.011
	(0.010)	(0.009)	(0.024)	(0.021)	(0.008)	(0.008)	(0.012)	(0.009)
R-Squared	0.491	0.541	0.089	0.146	0.151	0.199	0.0908	0.149
			Panel B: Heterogeneity by Timing of Notice					
Enforceability	-0.013^{+}		0.004		-0.001		0.006	
	(0.007)		(0.006)		(0.006)		(0.005)	
First Learned of Noncompete								
Before Accepting Job	0.139**	0.085**	0.100**	0.044^{+}	0.137**	0.061*	0.055**	0.042*
	(0.032)	(0.032)	(0.026)	(0.024)	(0.026)	(0.026)	(0.020)	(0.021)
After Accepting Job	0.051	0.021	-0.093*	-0.132**	0.020	-0.057	-0.098**	-0.087*
	(0.041)	(0.038)	(0.046)	(0.039)	(0.036)	(0.039)	(0.033)	(0.033)
Enforceability×Before Accepting Job	-0.032*	-0.027*	0.006	0.003	0.025*	0.025*	-0.009	-0.006
	(0.012)	(0.011)	(0.016)	(0.015)	(0.010)	(0.009)	(0.009)	(0.008)
Enforceability×After Accepting Job	-0.039*	-0.030*	0.024	0.015	0.008	-0.000	-0.033	-0.027
	(0.016)	(0.014)	(0.046)	(0.037)	(0.017)	(0.016)	(0.025)	(0.019)
R-Squared	0.492	0.542	0.093	0.150	0.154	0.201	0.093	0.152
Observations	11,462	11,010	11,462	11,010	11,462	11,010	11,462	11,010
State Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Panel A examines the main association of having a noncompete with the column outcomes and the moderating role of noncompete enforceability, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the association of having a noncompete and noncompete enforceability to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. We define the variables that make up our basic and advanced controls on page 11. The Enforceability measure is drawn from Starr (2019), who modifies the initial measure of Bishara (2011). ** $p<0.01$, * $p<0.05$, $^{+}$ $p<0.1$. We show standard errors in parentheses, clustered at the state level.

Online Appendix

A The Incidence of Noncompetes by Other Characteristics

Our data allow us to describe the incidence of noncompetes by a variety of employee, employer, and geographic characteristics. In Figures OA1 through OA4, we present additional statistics using the format of Figures 1 through 8: the top and bottom of each bar bookend the possible range of noncompete incidence for the group in question; we calculate the top by assuming that those in the "maybe" group did agree to a noncompete and the bottom by assuming that they did not. The dark dot within the bar is the multiple imputation estimate, which is our best guess at the overall incidence of noncompetes for the category. The dashed horizontal line is the population average, 18.1%. In Figure OA5, we show the joint distribution of noncompete use in our data by industry and occupation. Note that in the separate occupation and industry figures in the text (Figures 5 and 6), we also report the "projected" use of noncompetes in each 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 the respondent's occupation and industry, respectively.

Figure OA1: Noncompetes Incidence by Expected Length of Stay

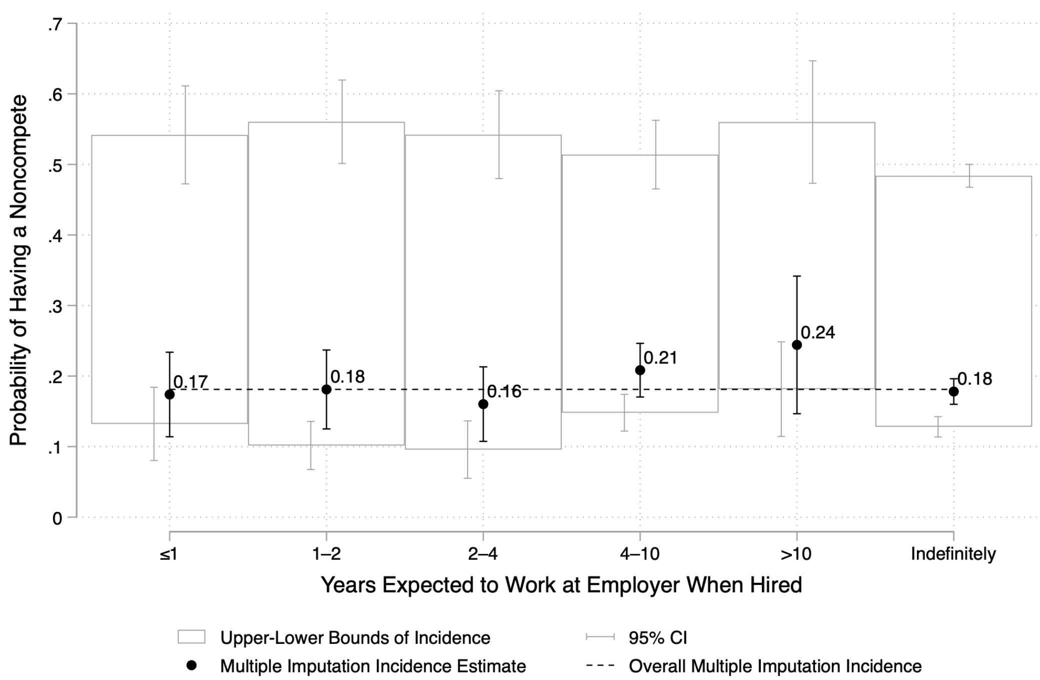


Figure OA2: Noncompete Incidence by Number of Employers in Past 5 Years

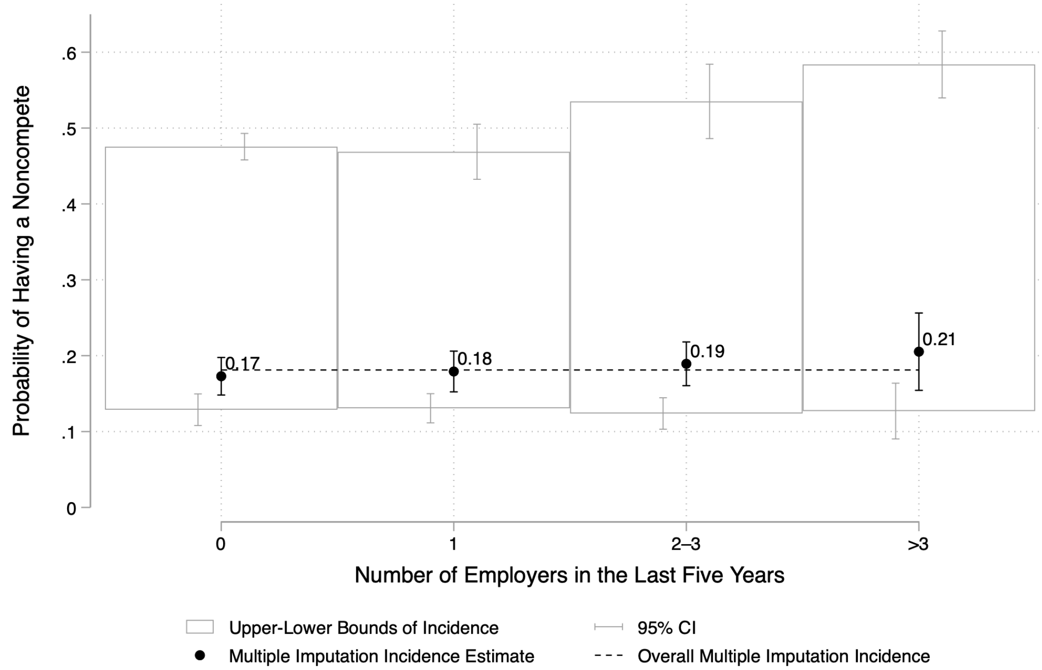


Figure OA3: Noncompete Incidence by Year of Hire
(conditional on staying until 2014)

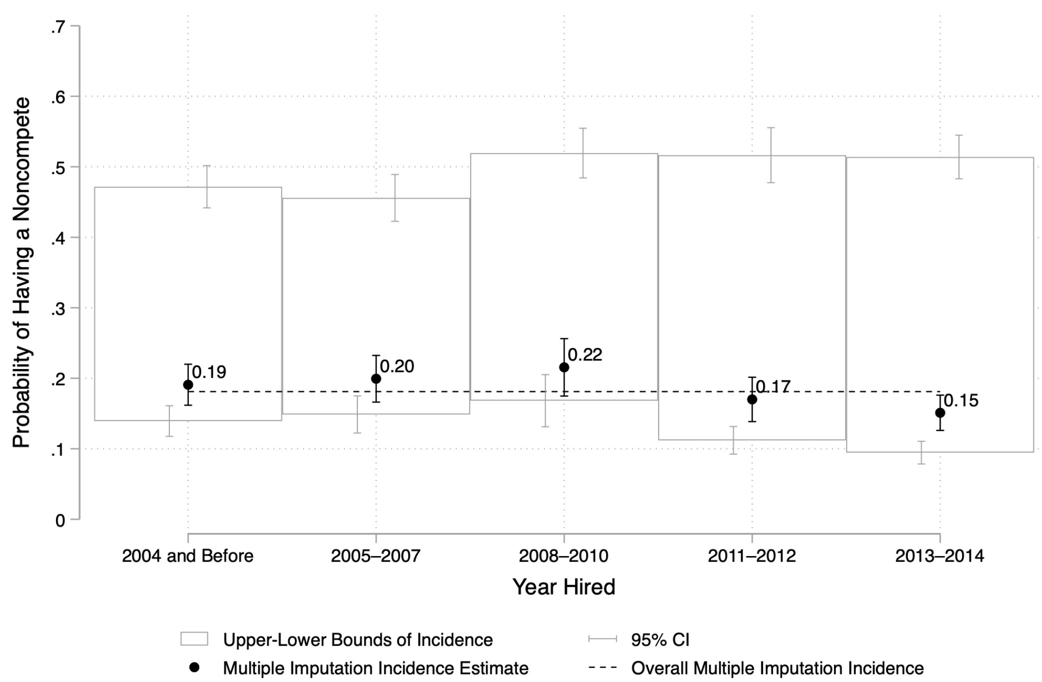


Figure OA4: Noncompete Incidence by Employer Size

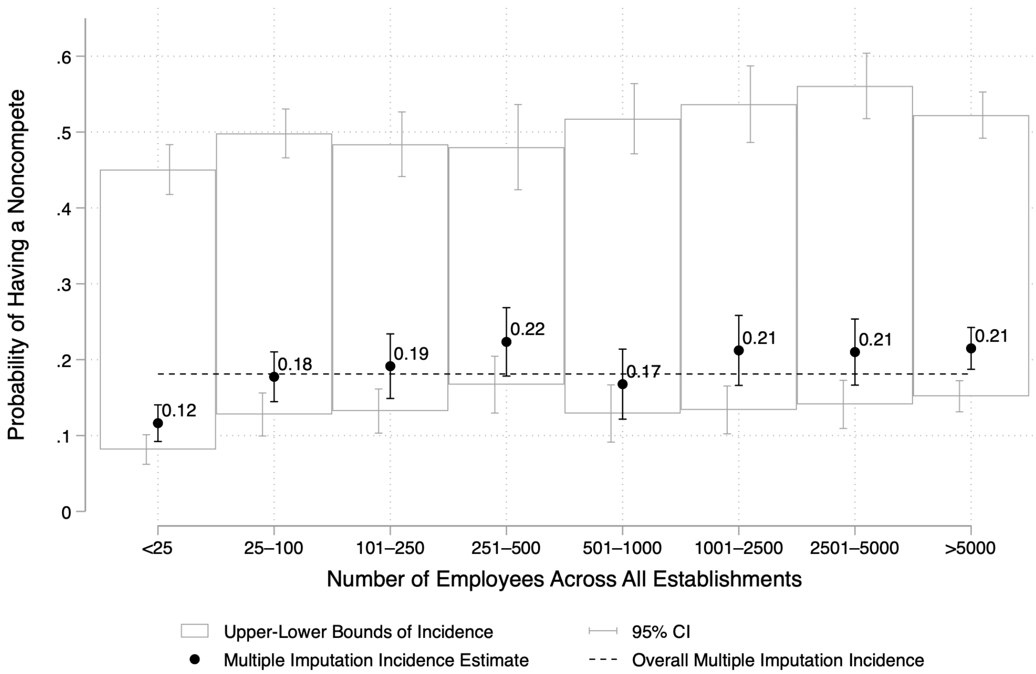
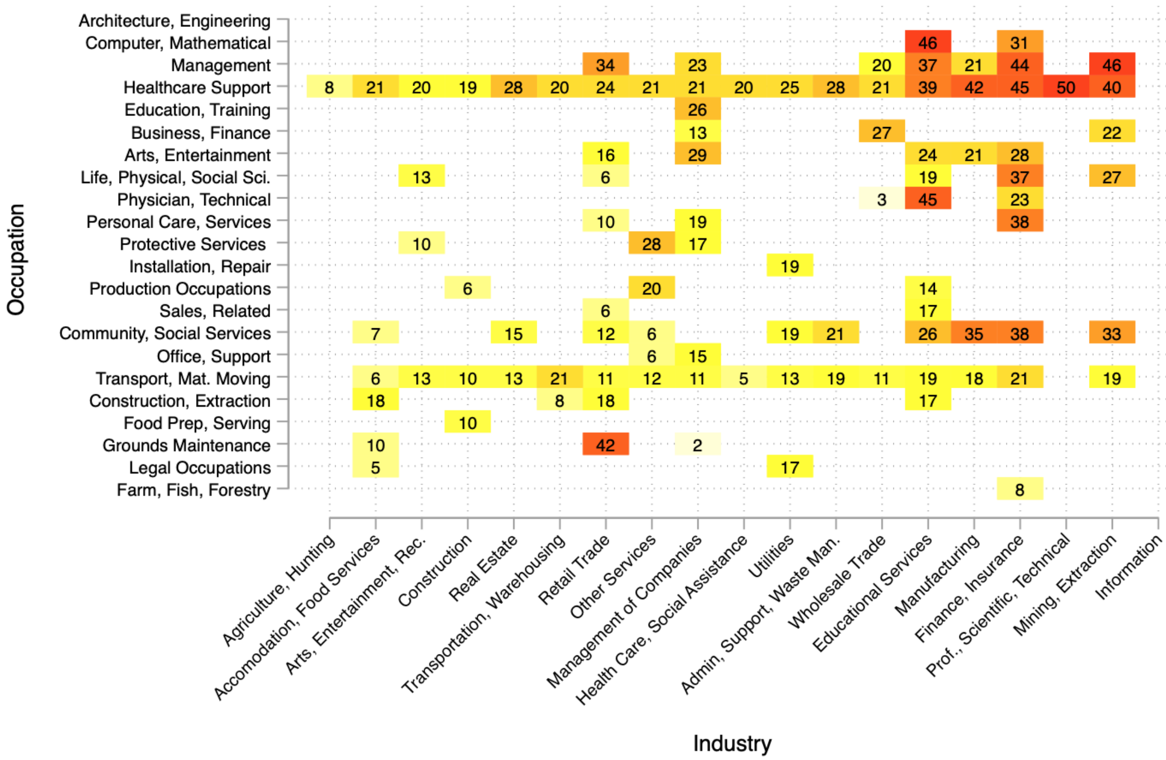


Figure OA5: Incidence of Noncompetes by Industry and Occupation



B Additional Tables

Table OB1: Noncompete Incidence Across Studies and Samples

Study	Population	% U.S. Labor Force	Data Source	Sample Size	Response Rate	Noncompete Incidence
Schwab and Thomas (2006)	Executives	0.18%	The Corporate Library, SEC EDGAR Filings	375 Executives	N/A	67.5%
Garmaise (2009)	Executives	0.18%	Execucomp Firms with 10-K, 10-Q SEC Filings	500 Firms	N/A	70.2%
Bishara et al. (2012)	Executives	0.18%	SEC EDGAR Filings	500 Firms	N/A	78.7%
Marx (2011)	Electrical and Electronics Engineers	0.23%	Survey of Institute of Electrical and Electronics Engineers (professional association)	1,029 Individuals	20.6%	43.3%
Lavetti et al. (2019)	Physicians	0.47%	Survey of American Medical Association Primary Care Physicians in 5 states	1,967 Individuals	69.8%	45.1%
Johnson and Lipsitz (2019)	Hair Stylists	0.25%	Survey of Professional Beauty Association	218 Hair Salons	4%–31%	30.0%
Present Study	U.S. Labor Force	100%	Qualtrics (in conjunction with 7 online survey panel providers)	11,505 Individuals	2%–23%	18.1%

Note: Proportion of U.S. Labor force based on the 2014 BLS Occupational Employment Survey: https://www.bls.gov/news.release/archives/ocwage_03252015.pdf

Table OB2: Reasons for Not Negotiating a Noncompete

	(1)	(2)	(3)
	Presented with Noncompete Before Accepted Job Offer?		*Overall*
	Yes	No	
I Found the Terms Reasonable	0.55	0.46	0.52
I Assumed I Could Not Negotiate	0.38	0.48	0.41
I Wanted to Avoid Creating Tension	0.18	0.19	0.19
I Worried I Would be Fired	0.20	0.22	0.20
I Didn't Think my Employer Would Sue	0.07	0.11	0.08
I Didn't Think a Court Would Enforce	0.08	0.05	0.07
Other	0.04	0.07	0.05

Notes: The table shows the reasons individuals report for not negotiating over their noncompete in response to the question: *"If you did not negotiate over the noncompete, why not?"* Respondents were free to select more than one response. Those who agreed to a noncompete as part of a promotion or who were unable to recall whether they negotiated or why they chose not to negotiate are omitted from the table. Column (3) reports the overall average, and the rows are sorted based on these proportions.

Table OB3: Sample Means by Noncompete Timing

	Noncompete Timing		
	Before Accepted Job Offer	After Accepted Job Offer	Difference
Dependent Variables			
Ln(Hourly Wage)	3.31	3.10	0.214**
1(Firm Shares Info)	0.66	0.46	0.201**
1(Training Last Year)	0.69	0.58	0.116**
1(Satisfied)	0.75	0.57	0.171**
Demographics			
1(Paid Hourly)	0.40	0.55	-0.156**
1(Paid by Salary)	0.53	0.41	0.123*
1(Paid by Commission)	0.05	0.02	0.030^{+}
1(Paid by Other Means)	0.01	0.01	0.002
Age (in years)	40.71	38.35	2.233^{+}
Hours Worked per Week	41.67	39.28	2.405
Weeks Worked per Year	48.27	47.85	0.422
1(Male)	0.63	0.48	0.155**
1(Private For-Profit Employer)	0.95	0.96	-0.010
1(Private Nonprofit Employer)	0.03	0.02	0.009
1(Public Health System Employer)	0.02	0.02	0.002
1(Highest Degree $<$ BA)	0.45	0.50	-0.053
1(Highest Degree $=$ BA)	0.34	0.33	0.007
1(Highest Degree $>$ BA)	0.21	0.17	0.047^{+}
Ln(State Unemployment Rate at Hire)	1.90	1.90	0.000
Ln(Labor Force Size in State at Hire)	15.40	15.41	-0.014
Ln(Establishments in County-Industry)	6.71	6.65	0.056
1(Employer Size $<$ 25)	0.14	0.14	-0.005
1(Employer Size 25–100)	0.14	0.17	-0.032
1(Employer Size 101–250)	0.11	0.08	0.025
1(Employer Size 251–5,000)	0.34	0.27	0.059
1(Employer Size $>$ 5,000)	0.28	0.32	-0.047
1(Multi-Unit Employer)	0.74	0.75	-0.015
Other Post-Employment Restrictive Covenants			
1(Nondisclosure)	0.75	0.77	-0.014
1(Nonpoaching)	0.20	0.14	0.057
1(Nonsolicit)	0.36	0.32	0.039
1(Arbitration)	0.20	0.18	0.025
1(IP Assignment)	0.30	0.24	0.060

Notes: This table reports the weighted sample means for respondents who report working under a noncompete (not including those who are imputed as agreeing to a noncompete). The "After Accepted Job Offer" category does not include those who were asked to sign a noncompete following a promotion or other changes in employment responsibilities. ** $p<0.01$, * $p<0.05$, $^{+}$ $p<0.1$. We use robust standard errors, clustered at the state level, when testing differences between categories.

Table OB4: Ln(Hourly Wage) Results, Sequential Addition of Covariates

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Baseline			
Noncompete	0.407**	0.109**	0.107**	0.084**	0.068*	0.066**
	(0.038)	(0.026)	(0.026)	(0.026)	(0.027)	(0.023)
R-Squared	0.035	0.503	0.507	0.508	0.511	0.541
			Panel B: Heterogeneity by Timing of Notice			
First Learned of Noncompete						
Before Accepting Job	0.483**	0.143**	0.142**	0.118**	0.102**	0.093**
	(0.047)	(0.033)	(0.032)	(0.033)	(0.033)	(0.031)
After Accepting Job	0.269**	0.057	0.053	0.034	0.017	0.024
	(0.064)	(0.042)	(0.041)	(0.039)	(0.041)	(0.037)
With Promotion	0.650**	0.202*	0.186*	0.154^{+}	0.130	0.136
	(0.154)	(0.090)	(0.086)	(0.090)	(0.087)	(0.086)
Doesn't Remember	0.262**	0.020	0.018	0.007	-0.005	0.010
	(0.078)	(0.056)	(0.057)	(0.058)	(0.060)	(0.064)
P-value: $\beta_{Before} = \beta_{After}$	0.007	0.062	0.053	0.065	0.063	0.127
R-Squared	0.038	0.503	0.507	0.509	0.511	0.541
Observations	11,505	11,462	11,462	11,462	11,462	11,010
Demographic Controls		✓	✓	✓	✓	✓
Poaching Flows/Prior Mobility			✓	✓	✓	✓
Other Restrictive Covenants				✓	✓	✓
Access to Confidential Info					✓	✓
Other HR Benefits						✓

Notes: Panel A examines the aggregate association of having a noncompete with Ln(Hourly Wages), where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. Controls are as defined on page 11. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB5: **1**(Employer Shares Information) Results, Sequential Addition of Covariates

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Baseline			
Noncompete	0.050^{+}	0.031	0.031	0.004	-0.017	-0.020
	(0.030)	(0.030)	(0.031)	(0.027)	(0.026)	(0.025)
R-Squared	0.0011	0.100	0.116	0.120	0.127	0.146
			Panel B: Heterogeneity by Timing of Notice			
First Learned of Noncompete						
Before Accepting Job	0.120**	0.101**	0.097**	0.068**	0.048^{+}	0.043^{+}
	(0.025)	(0.026)	(0.027)	(0.025)	(0.024)	(0.024)
After Accepting Job	-0.081	-0.093^{+}	-0.086^{+}	-0.109*	-0.131**	-0.134**
	(0.051)	(0.050)	(0.049)	(0.044)	(0.042)	(0.039)
With Promotion	0.109	0.039	0.059	0.023	-0.002	0.011
	(0.102)	(0.089)	(0.089)	(0.092)	(0.094)	(0.104)
Doesn't Remember	-0.019	-0.049	-0.044	-0.059	-0.081	-0.073
	(0.073)	(0.076)	(0.078)	(0.072)	(0.067)	(0.064)
P-value: $\beta_{Before} = \beta_{After}$	0.000	0.000	0.000	0.000	0.000	0.000
R-Squared	0.00550	0.104	0.119	0.123	0.131	0.150
Observations	11,505	11,462	11,462	11,462	11,462	11,010
Demographic Controls		✓	✓	✓	✓	✓
Poaching Flows/Prior Mobility			✓	✓	✓	✓
Other Restrictive Covenants				✓	✓	✓
Access to Confidential Info					✓	✓
Other HR Benefits						✓

Notes: Panel A examines the aggregate association of having a noncompete with an indicator for whether the employer shares all job-related information with the respondent, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. Controls are as defined on page 11. ** $p<0.01$, * $p<0.05$, $^{+}$ $p<0.1$. We show standard errors in parentheses, clustered at the state level.

Table OB6: **1**(Training Last Year) Results, Sequential Addition of Covariates

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Baseline			
Noncompete	0.152**	0.077**	0.073**	0.030	0.011	0.006
	(0.018)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
R-Squared	0.010	0.160	0.174	0.181	0.190	0.199
			Panel B: Heterogeneity by Timing of Notice			
First Learned of Noncompete						
Before Accepting Job	0.209**	0.131**	0.126**	0.081**	0.063*	0.055*
	(0.022)	(0.024)	(0.024)	(0.023)	(0.025)	(0.025)
After Accepting Job	0.093**	0.017	0.007	-0.033	-0.056	-0.058
	(0.034)	(0.035)	(0.037)	(0.040)	(0.039)	(0.039)
With Promotion	0.016	-0.060	-0.039	-0.107	-0.135	-0.125
	(0.119)	(0.097)	(0.093)	(0.100)	(0.103)	(0.113)
Doesn't Remember	-0.033	-0.076	-0.055	-0.078	-0.091	-0.093
	(0.069)	(0.069)	(0.068)	(0.066)	(0.065)	(0.064)
P-value: $\beta_{Before} = \beta_{After}$	0.006	0.014	0.013	0.017	0.015	0.021
R-Squared	0.0135	0.162	0.176	0.183	0.192	0.201
Observations	11,505	11,462	11,462	11,462	11,462	11,010
Demographic Controls		✓	✓	✓	✓	✓
Poaching Flows/Prior Mobility			✓	✓	✓	✓
Other Restrictive Covenants				✓	✓	✓
Access to Confidential Info					✓	✓
Other HR Benefits						✓

Notes: Panel A examines the aggregate association of having a noncompete with an indicator for whether the respondent received training in the last year, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. Controls are as defined on page 11. ** p<0.01, * p<0.05, + p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB7: **1**(Satisfied in Job) Results, Sequential Addition of Covariates

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Baseline			
Noncompete	0.024	0.015	0.017	0.012	0.007	0.006
	(0.021)	(0.019)	(0.020)	(0.018)	(0.017)	(0.017)
R-Squared	0.000	0.0991	0.129	0.132	0.134	0.149
			Panel B: Heterogeneity by Timing of Notice			
First Learned of Noncompete						
Before Accepting Job	0.072**	0.060**	0.058*	0.052*	0.047*	0.045*
	(0.019)	(0.020)	(0.022)	(0.021)	(0.021)	(0.020)
After Accepting Job	-0.099*	-0.090*	-0.079*	-0.080*	-0.085*	-0.085*
	(0.045)	(0.036)	(0.035)	(0.035)	(0.034)	(0.035)
With Promotion	0.137*	0.070	0.080	0.070	0.065	0.051
	(0.064)	(0.067)	(0.062)	(0.068)	(0.069)	(0.071)
Doesn't Remember	0.077	0.043	0.040	0.043	0.037	0.042
	(0.050)	(0.044)	(0.044)	(0.045)	(0.046)	(0.047)
P-value: $\beta_{Before} = \beta_{After}$	0.001	0.000	0.001	0.002	0.002	0.003
R-Squared	0.004	0.102	0.131	0.134	0.136	0.151
Observations	11,505	11,462	11,462	11,462	11,462	11,010
Demographic Controls		✓	✓	✓	✓	✓
Poaching Flows/Prior Mobility			✓	✓	✓	✓
Other Restrictive Covenants				✓	✓	✓
Access to Confidential Info					✓	✓
Other HR Benefits						✓

Notes: Panel A examines the aggregate association of having a noncompete with an indicator for whether the respondent reports being satisfied in their job, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. Controls are as defined on page 11. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB8: Direct Evidence on the Price of a Noncompete

	(1)	(2)	(3)
	When did you first learn you would be asked to sign?		*Overall*
	Before Accepting	After Accepting	
Panel A: "What did your employer promise, either explicitly or implicitly, in exchange for asking you to sign a noncompete?"			
Nothing	0.84	0.91	0.86
More Compensation	0.09	0.04	0.07
Job Security	0.08	0.04	0.07
More Training	0.07	0.04	0.06
More Trust by Employer	0.07	0.04	0.06
Better Working Conditions	0.05	0.03	0.04
More Responsibility	0.05	0.02	0.04
Promotion	0.03	0.03	0.03
More Access to Confidential Information	0.04	0.03	0.03
More Access to Clients/Lists	0.03	0.02	0.02
More Client Referrals	0.02	0.02	0.02
Other Benefits	0.01	0.01	0.01
Panel B: "What do you believe you received in exchange for signing a noncompete?"			
Nothing	0.45	0.58	0.50
Job Security	0.33	0.25	0.30
More Trust by Employer	0.32	0.24	0.29
More Compensation	0.23	0.11	0.19
More Responsibility	0.17	0.14	0.16
More Access to Confidential Information	0.16	0.12	0.14
More Training	0.17	0.10	0.14
More Access to Clients/Lists	0.13	0.08	0.11
Better Working Conditions	0.13	0.08	0.11
Promotion	0.11	0.05	0.09
More Client Referrals	0.07	0.03	0.05
Other Benefits	0.01	0.02	0.01

Notes: The table shows the proportion of individuals who report receiving or being promised various benefits in exchange for agreeing to a noncompete conditional on when they were asked to agree. Those who signed a noncompete before a promotion or who can't recall are omitted from the columns (1) and (2) for brevity. Column (3) reports the overall average, and the rows are sorted based on these proportions.

Table OB9: Labor Market Outcomes: Treating "Maybes" as Own Category

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Ln(Hourly Wage)		**1**(Employer Shares Info)		**1**(Training Last Year)		**1**(Satisfied in Job)	
				Panel A: Baseline				
Noncompete: Yes	0.096**	0.056*	0.013	-0.034	0.054**	-0.010	0.009	-0.000
	(0.027)	(0.024)	(0.031)	(0.027)	(0.020)	(0.022)	(0.017)	(0.017)
	[1.196]	[0.528]	[1.057]	[1.484]	[1]	[0.190]	[5.288]	[12.11]
		{0.202}		{0.547}		{0.0831}		{0.500}
Noncompete: Maybe	-0.041	-0.035	-0.052**	-0.047**	-0.069**	-0.050*	-0.019	-0.022
	(0.025)	(0.025)	(0.016)	(0.017)	(0.021)	(0.023)	(0.019)	(0.018)
R-Squared	0.503	0.542	0.102	0.148	0.163	0.200	0.0994	0.150
				Panel B: Heterogeneity by Timing of Notice				
First Learned of Noncompete								
Before Accepting Job	0.129**	0.083*	0.084**	0.029	0.108**	0.040	0.054**	0.038^+
	(0.034)	(0.031)	(0.027)	(0.026)	(0.025)	(0.026)	(0.019)	(0.020)
	[1.396]	[0.689]	[6.205]	[0.979]	[1.987]	[0.752]	[17.01]	[4.865]
		{0.265}		{0.359}		{0.310}		{1.716}
After Accepting Job	0.042	0.013	-0.112*	-0.150**	-0.008	-0.074^+	-0.097**	-0.092*
	(0.042)	(0.037)	(0.051)	(0.040)	(0.038)	(0.042)	(0.034)	(0.035)
	[0.900]	[0.355]	[63.12]	[11.65]	[0.436]	[1.547]	[4.847]	[7.744]
		{0.138}		{3.676}		{0.657}		{8.069}
With Promotion	0.188*	0.127	0.021	-0.002	-0.084	-0.139	0.064	0.045
	(0.090)	(0.087)	(0.089)	(0.103)	(0.098)	(0.115)	(0.069)	(0.072)
	[1.327]	[0.779]	[0.388]	[0.194]	[1.379]	[2.676]	[1.481]	[2.394]
		{0.263}		{0.118}		{0.995}		{9.510}
Doesn't Remember	0.007	-0.001	-0.067	-0.089	-0.100	-0.109^+	0.036	0.035
	(0.057)	(0.065)	(0.076)	(0.065)	(0.071)	(0.065)	(0.045)	(0.047)
	[0.286]	[0.168]	[3.612]	[6.218]	[3.527]	[7.089]	[1.358]	[5.530]
		{1.500}		{2.705}		{10.03}		{52.92}
Noncompete: Maybe	-0.041	-0.036	-0.053**	-0.048**	-0.070**	-0.050*	-0.019	-0.022
	(0.025)	(0.025)	(0.016)	(0.017)	(0.021)	(0.023)	(0.019)	(0.018)
P-value: $\beta_{Before} = \beta_{After}$	0.059	0.123	0.000	0.000	0.013	0.020	0.000	0.003
R-Squared	0.504	0.542	0.106	0.151	0.166	0.202	0.102	0.152
Observations	11,462	11,010	11,462	11,010	11,462	11,010	11,462	11,010
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Panel A examines the aggregate association of having a noncompete with the outcome of the column, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are treated as their own category. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, where again the "maybe" group is treated as its own category and those not bound by a noncompete are the omitted category. We define the variables that make up our basic and advanced controls on page 11. We report the selection test relative to a model with no controls in square brackets ('[ ]'), and we report the selection test between the models with basic and advanced controls in curly brackets ('{ }'). In both cases, the selection test statistic is calculated with the Stata command `psacalc`, using as the maximum R-Squared Oster's suggested 30% more than the R-Squared from the model that includes both the basic and advanced controls. ** p<0.01, * p<0.05, + p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB10: Labor Market Outcomes: Multiple Imputation

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Ln(Hourly Wage)		**1**(Employer Shares Info)		**1**(Training Last Year)		**1**(Satisfied in Job)	
			Panel A: Baseline					
Noncompete	0.074**	0.038	0.015	-0.036	0.070**	0.008	0.022	0.017
	(0.027)	(0.029)	(0.027)	(0.025)	(0.020)	(0.024)	(0.020)	(0.019)
	[0.920]	[0.357]	[1.042]	[1.361]	[1.271]	[0.238]	[30.83]	[6.969]
		{0.137}		{0.544}		{0.113}		{1.551}
R-Squared	0.502	0.541	0.0998	0.147	0.160	0.199	0.0994	0.149
			Panel B: Heterogeneity by Timing of Notice					
First Learned of Noncompete								
Before Accepting Job	0.143**	0.091**	0.100**	0.036	0.134**	0.056*	0.063**	0.048*
	(0.033)	(0.031)	(0.026)	(0.025)	(0.025)	(0.026)	(0.021)	(0.021)
	[1.204]	[0.607]	[3.962]	[0.993]	[1.948]	[0.893]	[4.704]	[4.295]
		{0.253}		{0.408}		{0.395}		{2.084}
After Accepting Job	0.057	0.022	-0.094^{+}	-0.140**	0.020	-0.056	-0.088*	-0.081*
	(0.043)	(0.038)	(0.050)	(0.039)	(0.036)	(0.040)	(0.036)	(0.035)
	[0.739]	[0.298]	[12.01]	[7.916]	[0.132]	[1.044]	[6.788]	[7.845]
		{0.136}		{2.842}		{0.452}		{6.257}
With Promotion	0.202*	0.134	0.038	0.005	-0.057	-0.124	0.072	0.054
	(0.089)	(0.086)	(0.089)	(0.104)	(0.096)	(0.112)	(0.068)	(0.071)
	[1.217]	[0.725]	[0.633]	[0.228]	[0.549]	[2.123]	[1.433]	[2.620]
		{0.259}		{0.134}		{0.820}		{45.26}
Doesn't Remember	0.020	0.008	-0.050	-0.079	-0.073	-0.092	0.045	0.045
	(0.056)	(0.064)	(0.076)	(0.064)	(0.070)	(0.064)	(0.044)	(0.047)
	[0.0619]	[0.138]	[1.968]	[4.262]	[1.747]	[4.252]	[1.291]	[5.574]
		{0.393}		{2.033}		{3.855}		{46.74}
Imputed Signer	-0.003	-0.020	-0.019	-0.055	0.046	0.010	0.033	0.032
	(0.054)	(0.059)	(0.041)	(0.042)	(0.043)	(0.049)	(0.039)	(0.039)
P-value: $\beta_{Before} = \beta_{After}$	0.062	0.127	0.000	0.000	0.014	0.021	0.000	0.003
R-Squared	0.503	0.541	0.104	0.150	0.163	0.201	0.102	0.152
Observations	11,462	11,010	11,462	11,010	11,462	11,010	11,462	11,010
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Panel A examines the aggregate association of having a noncompete with the outcome of the column, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are assigned to the "yes" group or the "no" group using multiple imputation. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category, and where those imputed to have noncompetes are treated separately (because they were unaware of their noncompete status and so were not asked the timing question). We define the variables that make up our basic and advanced controls on page 11. We report the selection test relative to a model with no controls in square brackets ('[ ]'), and we report the selection test between the models with basic and advanced controls in curly brackets ('{ }'). In both cases, the selection test statistic is calculated with the Stata command `psacalc`, using as the maximum R-Squared Oster's suggested 30% more than the R-Squared from the model that includes both the basic and advanced controls. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB11: Labor Market Outcomes: Unweighted

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Ln(Hourly Wage)		**1**(Employer)		**1**(Training Last Year)		**1**(Satisfied in Job)	
				Panel A: Baseline				
Noncompete	0.078**	0.052**	0.009	-0.031^{+}	0.065**	0.003	0.014	0.008
	(0.017)	(0.016)	(0.016)	(0.015)	(0.013)	(0.014)	(0.016)	(0.014)
	[0.993]	[0.530]	[0.290]	[1.392]	[1.198]	[0.0831]	[0.848]	[1.235]
		{0.209}		{0.776}		{0.0353}		{1.426}
R-Squared	0.469	0.495	0.0647	0.0992	0.129	0.161	0.0646	0.101
				Panel B: Heterogeneity by Timing of Notice				
First Learned of Noncompete								
Before Accepting Job	0.087**	0.057**	0.075**	0.030^{+}	0.096**	0.032^{+}	0.065**	0.054**
	(0.020)	(0.018)	(0.017)	(0.016)	(0.015)	(0.017)	(0.015)	(0.014)
	[0.959]	[0.542]	[2.599]	[1.040]	[1.738]	[0.674]	[3.185]	[4.054]
		{0.221}		{0.559}		{0.282}		{3.212}
After Accepting Job	0.051^{+}	0.031	-0.104**	-0.138**	0.024	-0.039^{+}	-0.077**	-0.071**
	(0.026)	(0.027)	(0.026)	(0.026)	(0.020)	(0.021)	(0.025)	(0.024)
	[1.090]	[0.544]	[7.038]	[10.39]	[0.171]	[0.985]	[14.89]	[10.33]
		{0.215}		{5.096}		{0.389}		{7.317}
With Promotion	0.283**	0.249**	0.008	-0.013	-0.032	-0.091	-0.010	-0.017
	(0.086)	(0.085)	(0.070)	(0.079)	(0.066)	(0.072)	(0.076)	(0.075)
	[2.587]	[1.857]	[0.221]	[0.451]	[0.112]	[1.979]	[0.235]	[1.080]
		{0.801}		{0.431}		{0.805}		{75.62}
Doesn't Remember	0.029	0.022	-0.096*	-0.102*	0.007	-0.019	-0.046	-0.040
	(0.039)	(0.041)	(0.047)	(0.044)	(0.058)	(0.059)	(0.042)	(0.042)
	[0.383]	[0.393]	[6.928]	[15.11]	[1.062]	[1.245]	[1.922]	[9.682]
		{0.442}		{9.503}		{0.439}		{4.960}
P-value: $\beta_{Before} = \beta_{After}$	0.183	0.339	0.000	0.000	0.005	0.006	0.000	0.000
R-Squared	0.470	0.495	0.069	0.103	0.130	0.162	0.067	0.104
Observations	11,462	11,010	11,462	11,010	11,462	11,010	11,462	11,010
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This table replicates the main labor market outcome results in Table 8, except the analysis is not weighted. Panel A examines the aggregate association of having a noncompete with the outcome of the column, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Tables OB9 and OB10 show the results of treating the "maybe" group as a separate category and imputing "yes" or "no" status for each respondent in the "maybe" group, respectively. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. We define the variables that make up our basic and advanced controls on page 11. We report the selection test relative to a model with no controls in square brackets ('[ ]'), and we report the selection test between the models with basic and advanced controls in curly brackets ('{ }'). In both cases, the selection test statistic is calculated with the Stata command `psacalc`, using as the maximum R-Squared Oster's suggested 30% more than the R-Squared from the model that includes both the basic and advanced controls.
** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB12: Labor Market Outcomes: Dropping Noncompete-Focused Respondents

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Ln(Hourly Wage)		**1**(Employer Shares Info)		**1**(Training Last Year)		**1**(Satisfied in Job)	
	Panel A: Baseline							
Noncompete	0.079*	0.045	0.036	-0.009	0.078**	0.011	0.010	0.009
	(0.032)	(0.028)	(0.035)	(0.031)	(0.026)	(0.028)	(0.021)	(0.021)
	Panel B: Heterogeneity by Timing of Notice							
First Learned of Noncompete								
Before Accepting Job	0.122**	0.083*	0.092*	0.040	0.126**	0.055	0.055*	0.048
	(0.043)	(0.040)	(0.037)	(0.035)	(0.035)	(0.035)	(0.026)	(0.030)
After Accepting Job	-0.008	-0.037	-0.087+	-0.123**	0.026	-0.047	-0.107*	-0.092*
	(0.055)	(0.048)	(0.051)	(0.042)	(0.046)	(0.047)	(0.044)	(0.041)
With Promotion	0.249**	0.196*	0.066	0.045	-0.082	-0.153	0.127*	0.113+
	(0.090)	(0.085)	(0.109)	(0.123)	(0.114)	(0.135)	(0.062)	(0.059)
Doesn't Remember	0.031	0.038	0.071	0.044	-0.023	-0.041	0.066	0.068
	(0.070)	(0.080)	(0.096)	(0.075)	(0.067)	(0.064)	(0.059)	(0.059)
P-value: $\beta_{Before} = \beta_{After}$	0.054	0.058	0.001	0.000	0.081	0.067	0.005	0.012
Observations	8,982	8,623	8,982	8,623	8,982	8,623	8,982	8,623
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This table replicates the main labor market outcome results in Table 8, except that it excludes respondents who answered the survey question (which appears at the end of the survey) "Why did you decide to take this survey?" with "I wanted to share my experiences with noncompetes." Panel A examines the aggregate association of having a noncompete with the outcome of the column, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. We define the variables that make up our basic and advanced controls on page 11. ** $p<0.01$, * $p<0.05$, $^{+}$ $p<0.1$. We show standard errors in parentheses, clustered at the state level.

Table OB13: Related Dependent Variables

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	**1**(Job is Secure)		**1**(Employer Committed to Upgrading Skills)		**1**(Boomerang Employee)	
	Panel A: Baseline					
Noncompete	0.008	-0.005	0.049*	0.004	-0.011	-0.041*
	(0.021)	(0.023)	(0.019)	(0.017)	(0.023)	(0.020)
	[1.709]	[0.748]	[1.271]	[0.176]	[1.132]	[3.260]
		{0.272}		{0.109}		{1.436}
R-Squared	0.103	0.135	0.116	0.171	0.080	0.120
	Panel B: Heterogeneity by Timing of Notice					
First Learned of Noncompete						
Before Accepting Job	0.038^{+}	0.022	0.111**	0.059**	0.058*	0.027
	(0.020)	(0.020)	(0.020)	(0.020)	(0.022)	(0.021)
	[96.02]	[2.938]	[2.746]	[1.603]	[3.792]	[1.388]
		{0.736}		{0.865}		{0.617}
After Accepting Job	-0.055	-0.063	-0.059^{+}	-0.100**	-0.142**	-0.173**
	(0.035)	(0.041)	(0.033)	(0.032)	(0.042)	(0.038)
	[6.924]	[15.77]	[2.300]	[4.884]	[171.5]	[53.05]
		{18.66}		{3.443}		{13.39}
With Promotion	0.068	0.041	-0.043	-0.067	0.097	0.109^{+}
	(0.050)	(0.058)	(0.122)	(0.139)	(0.062)	(0.059)
	[1.988]	[1.606]	[0.500]	[1.391]	[8.684]	[7.501]
		{1.886}		{1.670}		{3.915}
Doesn't Remember	-0.004	-0.003	0.004	-0.006	-0.089	-0.098
	(0.036)	(0.037)	(0.067)	(0.055)	(0.077)	(0.071)
	[0.335]	[0.437]	[0.113]	[0.377]	[4.398]	[10.06]
		{1.293}		{0.692}		{6.871}
P-Value: $\beta_{Before} = \beta_{After}$	0.005	0.019	0.000	0.000	0.000	0.000
R-Squared	0.104	0.136	0.119	0.174	0.0852	0.125
Observations	11,462	11,010	11,462	11,010	11,462	11,010
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes

Notes: This table shows the relationship between noncompete status and timing and other dependent variables of interest. The dependent variable in columns (1) and (2) is an indicator for whether the employee agrees or strongly agrees that their job is secure. The dependent variable in columns (3) and (4) is an indicator for whether the employee agrees or strongly agrees that their employer is committed to upgrading their skills. The dependent variable in columns (5) and (6) is an indicator for whether the employee would consider returning to their employer if they were ever to leave (i.e., become a "boomerang" employee). Panel A examines the aggregate association of having a noncompete with the outcome of the column, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. We define the variables that make up our basic and advanced controls on page 11. We report the selection test relative to a model with no controls in square brackets ('[ ]'), and we report the selection test between the models with basic and advanced controls in curly brackets ('{ }'). In both cases, the selection test statistic is calculated with the Stata command `psacalc`, using as the maximum R-Squared Oster's suggested 30% more than the R-Squared from the model that includes both the basic and advanced controls. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB14: Predicting Timing of Noncompete Notice

Model: OLS	(1a)	(2a)		(1b)	(2b)
Dependent Variable:	***1**(Before Accepted Job)*				
Ln(State Unemployment Rate at Hire)	0.019	0.002	**1**(Highest Degree = BA)	-0.011	-0.051
	(0.050)	(0.047)		(0.042)	(0.050)
Ln(Labor Force Size in State at Hire)	-0.032	-0.030	**1**(Highest Degree > BA)	-0.002	-0.041
	(0.027)	(0.025)		(0.053)	(0.056)
1(Paid by Salary)	0.093*	0.068	**1**(Multi-Unit Employer)	-0.030	-0.061
	(0.045)	(0.053)		(0.061)	(0.072)
1(Paid by Commission)	0.202*	0.125	**1**(Employer Size 25–100)	-0.002	0.004
	(0.081)	(0.106)		(0.080)	(0.082)
1(Paid by Other Means)	0.066	-0.104	**1**(Employer Size 101–250)	0.074	0.019
	(0.164)	(0.196)		(0.080)	(0.094)
Age (in years)	-0.044	-0.010	**1**(Employer Size 251–500)	0.089	0.097
	(0.054)	(0.047)		(0.088)	(0.095)
Age^2	0.001	0.000	**1**(Employer Size 501–1,000)	0.059	0.022
	(0.001)	(0.001)		(0.078)	(0.082)
Age^3	-0.000	-0.000	**1**(Employer Size 1,001–2,500)	0.077	0.046
	(0.000)	(0.000)		(0.082)	(0.087)
Hours Worked per Week	-0.006	0.002	**1**(Employer Size 2,500–5,000)	0.012	0.001
	(0.012)	(0.012)		(0.098)	(0.104)
Weeks Worked per Year	-0.004	-0.001	**1**(Employer Size > 5,000)	0.006	-0.002
	(0.007)	(0.007)		(0.089)	(0.093)
Hours∗Weeks	0.000	0.000	Ln(Establishments in County-Industry)	-0.005	-0.005
	(0.000)	(0.000)		(0.010)	(0.014)
1(Male)	0.092*	0.089*	Noncompete Enforceability	-0.025	-0.024
	(0.040)	(0.043)		(0.015)	(0.015)
1(Private Nonprofit Employer)	0.081	0.061			
	(0.107)	(0.129)			
1(Public Health System Employer)	0.106	0.065			
	(0.094)	(0.117)			
Observations	1,568	1,568		1,568	1,568
Occ-Ind FE	No	Yes		No	Yes

Notes: The sample only includes individuals who report signing a noncompete (no imputed individuals). Those who were asked to sign with a promotion or cannot remember are excluded. Column (1a) and (1b) are the same regression, without Occupation by Industry FE, while Column (2a) and (2b) are the same regression, with Occupation by Industry FE. ** $p<0.01$, * $p<0.05$, $^{+}$ $p<0.1$. Standard errors in parentheses, clustered at the state level.

C The Enforceability of Noncompetes

Most noncompete scholarship revolves around whether and to what extent noncompetes 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 and state case law, with states often coming to markedly different conclusions (Bishara, 2011). For example, California adopted a policy of nonenforceability in 1872 (Gilson, 1999), which remains the policy of the state today, while Florida adopted a statute in 1996 (Florida Statutes §542.335 (g)) that instructed courts to "... 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 employer and the harm done to the employee 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 this information to quantify the enforceability of noncompetes. In this paper, we use the 2009 measure developed in Starr (2019), which was built off the initial coding of Malsberger et al. (2012) conducted by Bishara (2011). We report the table from Starr (2019) below.

Table OC1: Noncompete Enforceability Index (Starr, 2019)

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

D Potential Instruments for Noncompetes

Exogenous variation in the use of noncompetes is a traditional prerequisite for rigorously identifying the causal effects of such provisions. Unfortunately, at a minimum, noncompete use may be endogenous to outcomes driven by (potentially unobservable) employer and employee characteristics. The most natural instrument for noncompete use is the set of laws that govern their enforceability or changes in those laws over time. Employers that reside in states where noncompetes are exogenously easier to enforce have greater incentives to use noncompetes, but these laws may not affect employee outcomes except through the higher incidence of noncompetes. In this section, we explore what scrutiny of these instruments reveals, both in terms of predicting the use of noncompetes and in terms of second-stage effects.

We consider four potential instruments related to the enforceability of noncompetes. The first two are simply cross-sectional measures of noncompete enforceability. The second two exploit recent policy changes in noncompete enforceability at the state level. The first of these latter two variables, "Changes in Enforceability," is set to 1 if the respondent was hired after an increase in noncompete enforceability and to -1 if the respondent was hired after a reduction in enforceability. The value is set to 0 if there were no changes in the state in the last 20 years. These variables are subsequently decomposed into states that increased enforceability in the last five years and states that reduced enforceability in the last five years. All of these policy changes are gathered from Ewens and Marx (2017), who examined the state-by-state treatises of noncompete case law and statutes in Malsberger et al. (2012). The logic of these latter two instruments is to compare an employee who was hired before the regulatory change to an employee in the same state who was hired just after the regulatory change. The first-stage and second-stage results for these instruments are shown in Table OD1.

The first two instruments show that enforceability is positively associated with the use of noncompetes, but the second-stage estimates are implausibly large. Further analysis suggests that the exclusion restriction is violated as the enforceability measure appears to have a negative main effect on wages, as we show in column (1) in Table 9. The second set of instruments does not produce any statistically significant first-stage results, and indeed they point in opposite directions.

Table OD1: Potential Instruments for Noncompete Use

	(1)	(2)	(3)	(4)
First Stage				
Dependent Variable: Indicator for Noncompete Provision				
Noncompete Enforceability	0.008^{+}			
	(0.004)			
1st Quartile of Enforceability		0.046*		
		(0.017)		
2nd Quartile of Enforceability		0.028		
		(0.019)		
3rd Quartile of Enforceability		0.050^{+}		
		(0.026)		
4th Quartile of Enforceability		0.040^{+}		
		(0.022)		
5th Quartile of Enforceability		0.035^{+}		
		(0.020)		
Changes in Enforceability			-0.027	
			(0.021)	
Increased Enforceability Last 5 Years				-0.017
				(0.024)
Decreased Enforceability Last 5 Years				0.040
				(0.047)
Second Stage				
Dependent Variable: Ln(Hourly Wage)				
Noncompete	-2.690	-0.887	1.949	0.631
	(2.576)	(1.112)	(8.198)	(15.751)
Basic Controls	Yes	Yes	Yes	Yes
Occupation-Industry FE	Yes	Yes	Yes	Yes
Flow & Info Controls	Yes	Yes	Yes	Yes
Benefits & Contract FE	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes

Notes: The table shows our analysis of potential noncompete enforceability instruments. The instrument in column (1) is a linear measure of enforceability at the state level, while the instruments in second column are indicators for nonenforcing states and quartiles of enforcing states (the omitted category in column (2) is the set of nonenforcing states). Increased enforceability (column 3) is a variable that equals 1 if the respondent was hired after the state increased noncompete enforceability, 0 if the respondent was hired with no change in enforceability over the previous 20 years, and –1 if the respondent was hired after the state reduced noncompete enforceability. Column (4) repeats this analysis but separates out increases and decreases and focuses on changes only in the last 5 years. Columns (3) and (4) condition on tenure and have state fixed effects to compare the likelihood of having a noncompete to others who were hired in the state before the policy change. ** $p<0.01$, * $p<0.05$, $^{+}$ $p<0.1$. We show standard errors in parentheses, clustered at the state level.

E Examples of Noncompetes

Below are examples of actual covenants not to compete that we believe were recently deployed by four organizations: Amazon.com, Inc. (e-commerce company); Jimmy John's Franchise, LLC (fast food company); Blackbaud, Inc. (software company), and Girls on the Run of Silicon Valley (nonprofit). Note: with the exception of the Girls on the Run Noncompete, which we received when we applied for a job, we received the examples we reproduce here from third parties or obtained them online and so we do not vouch for their legal authenticity.

Figure OE1: Example of 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 OE2: Example of Jimmy John's Noncompete

3. **Non-Competition Covenant.** Employee covenants and agrees that, during his or her employment with Employer and for a period of two (2) years after either the effective date of termination of his or her employment for any reason, whether voluntary or involuntary and whether by Employer or Employee, or the date on which Employee begins to comply with this paragraph, whichever is later, he or she will not have any direct or indirect interest in or perform services for (whether as an owner, partner, investor, director, officer, representative, manager, employee, principal, agent, advisor, or consultant) any business which derives more than ten percent (10%) of its revenue from selling submarine, hero-type, deli-style, pita and/or wrapped or rolled sandwiches and which is located within three (3) miles of either (1) 9641 N Milwaukee Ave , Niles IL 60714 [insert address of employment], or (2) any such other JIMMY JOHN'S® Sandwich Shop operated by JJF, one of its authorized franchisees, or any of JJF's affiliates.

Employee also acknowledges and agrees that, for at least twelve (12) months after the effective date of termination of his or her employment for any reason, whether voluntary or involuntary and whether by Employer or Employee, Employee may not become a partner of or investor/owner with, or work for, another JIMMY JOHN'S® Sandwich Shop franchisee. Employee acknowledges that other JIMMY JOHN'S® Sandwich Shop franchisees are contractually prohibited by JJF from recruiting Employee as a partner or investor/owner, or from hiring Employee, for at least twelve (12) months after Employee leaves his or her employment with Employer (regardless of the reason for his or her departure).

Figure OE3: Example of Blackbaud Executive Noncompete

7.1 Noncompetition Provisions. Executive recognizes and agrees that the Company has many substantial, legitimate business interests that can be protected only by Executive agreeing not to compete with the Company or its subsidiaries under certain circumstances. These interests include, without limitation, the Company's contacts and relationships with its customers, the Company's reputation and goodwill in the industry, the financial and other support afforded by the Company, and the Company's rights in its confidential information. Executive therefore agrees that during his employment with the Company and for the twelve (12) month period of time following the termination of such employment by either party for any reason, he will not, without the prior written consent of the Company, engage in any of the following activities in the United States (the "Protected Zones"), relating to the Protected Businesses (as defined below):

a. engage in, manage, operate, control or supervise, or participate in the management, operation, control or supervision of, any business or entity which provides products or services directly competitive with those being actively developed, manufactured, marketed, sold or otherwise provided by the Company or its subsidiaries as of the date hereof (the "Protected Businesses") in the Protected Zones;

b. have any ownership or financial interest, directly or indirectly, in any entity in the Protected Zones engaged in the Protected Businesses, including, without limitation, as an individual, partner, shareholder (other than as an owner of an entity in which Executive owns less than 5% of the economic interests), officer, directly, executive, principal, agent or consultant;

c. solicit, acquire or conduct any Protected Business from or with any customers of the Company or its subsidiaries (as defined below) in the Protected Zones;

d. solicit any of the employees or independent contractors of the Company or its subsidiaries or induce any such persons to terminate their employment or contractual relationships with any such entities; and/or

e. serve as an officer or director of any entity engaged in any of the Protected Businesses in the Protected Zones.

Figure OE4: Example of Girls on the Run of Silicon Valley Noncompete

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.

F Data Online Appendix

This article's data derive from a labor force (i.e., employee) survey that we designed and implemented between April and July 2014. Our goal in conducting the survey was to understand the use and effects of covenants not to compete ("noncompetes"), both in a respondent's current job and over the course of a respondent's career. In this appendix, we describe the survey's origin, design, and sampling frame as well as our cleaning and processing of the data to clarify important aspects of this article's analysis. We draw heavily on an earlier technical article that describes these issues in meticulous detail (Prescott et al., 2016), and virtually identical content can be found in the appendix of Starr et al. (forthcoming).

F.1 Sampling Frame and Data Collection Methodology

The sampling frame for this study are U.S. labor force participants aged 18–75 years who are working in the private sector (for profit or nonprofit), working for a public health system,[46] or unemployed and looking for work. We excluded individuals who reported being self-employed, government employees, non-U.S. citizens, or out of the labor force. To collect the data, we considered a few possible survey platforms and collection methods, including using RAND's American Life Panel (ALP), conducting a random-digit-dial survey, and adding questions to ongoing established surveys like the NLSY or the PSID. Ultimately, we concluded that our work required a nationally representative sample that was larger than the ALP could provide. We also determined that, to obtain a complete picture of an employee's noncompete experiences, we needed to collect too many different pieces of new information to build on existing surveys. Instead, it made more sense to design and draft a noncompete-specific survey ourselves so that we would be able to ask all of the potentially relevant questions. In the end, we settled on using Qualtrics, a reputable online survey company with access to more than 10 million *verified* panel respondents.[47]

The target size for this data-collection project 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

[46] We initially considered focusing only on the private sector, but we recognized that public health systems (e.g., those associated with public universities) also use noncompetes extensively.

[47] The difference between verified and unverified survey respondents is important. The use of unverified survey respondents means that there is no external validation of any information the respondent provides (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 agreed to respond to online surveys. A typical experience involves filling out an intake form and providing fairly detailed demographic information, including a contact number. A day or so after completing the intake form, the applicant receives a phone call from the survey company at the number the applicant provided. On the call, the applicant is asked a series of questions related to the information previously provided on the intake form. Verified respondents are those who are reachable at the phone number supplied and who corroborate the information initially supplied.

earnings of at least $50,000 annually from their current, highest paying job; and 30% over the age of 55 years. We chose these particular thresholds 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. Respondents took a median time of approximately 28 minutes to complete the survey. 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 for our survey data, we briefly outline the costs and benefits of using online surveys.[48]

F.2 Costs and Benefits of Online Surveys

Online surveys come with a variety of benefits. Relative to random-digit-dial 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 respondents to answer through an online platform. Online surveys also allow individuals to respond at their leisure via their preferred method (e.g., computer, phone, tablet, etc.) from wherever they wish (e.g., work, home, or coffee shop). For these reasons, Reuters, the well-known national polling company, has conducted all of its polling since 2012 online, including its 2016 Presidential election polling.[49]

However, these benefits come at a potentially high cost: a sample of online survey takers may not be representative of the population of interest to researchers or policymakers. There are four sample selection concerns in particular. First, not all people in the U.S. labor force are online. Second, not all of those online register to take surveys. Third, not all of those who register to take surveys receive any particular survey. Fourth, not all of those who are invited to take a survey finish it. Among these sample selection concerns, only the second one is unique to online surveys.[50] With respect to the fourth, alternatives seem unlikely to be better. Kennedy and Hartig (2019) find that survey response to random-digit dialing fell to 6% in 2018, raising the very important question whether a sample resulting 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 F.4.

[48]The information contained in the following sections can be found in Tables 1–18 in Prescott et al. (2016).

[49]See the "About" tab at `http://polling.reuters.com/`.

[50]For example, random-digit-dial surveys miss those without a phone, those who have a phone but do not receive the survey call, and those who receive the call but decline to take the survey.

F.3 Survey Cleaning

Qualtrics fielded the survey and obtained 14,668 completed surveys. When we began to review this initial set of responses, we recognized that individuals with the same IP address may have taken the survey multiple times given there were incentives. To address this, we retained only the first attempt to take the survey from a given IP address and only if that attempt resulted in a completed survey, which produced a sample of 12,369 respondents. We next detected, by inspecting the raw data by hand, that some individuals appeared to have the exact same responses, even for write-in questions, despite the fact that the IP addresses recorded in the survey data were different. To weed these out, we 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. We found 665 possible repeat survey takers; the majority of these respondents took the survey with two different panel partners. We reviewed these potential repeat survey takers by hand, and, among those identified as repeat takers from different IP addresses, we kept the first observation and dropped all others, leaving us with a sample of 12,090 respondents.[51]

In the next round of cleaning, we examined individual answers to identify any that were internally inconsistent or unreasonable in substance. 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 answers. In analyzing these answers, we discovered that some individuals were intentionally non-compliant (e.g., writing curse words or gibberish instead of their job title), while others simply made idiosyncratic errors (e.g., noting that their entire employer was smaller than their establishment—that is, their particular office or factory). We dropped respondents entirely if we deemed them to be intentionally noncompliant because their singular responses indicated that they did not take the survey seriously. This step left us with 11,529 survey responses. [52]

In the last round of cleaning, we began with those who had clean surveys and those who had made some sort of idiosyncratic error. From our flagging algorithm, we determined that 82.2% had no flags and that 16.05% had just one flag (see Table 6 in Prescott et al. (2016)). The most common flag was reporting earnings below the 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 were. 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 we use in this article (although our findings are not very sensitive to this choice), is to impute or correct these offending values. Specifically, we "repaired" entries that were marred by idiosyncratic inconsistency by replacing the less reliable, offending value with

[51] See Tables 3–5 in Prescott et al. (2016) for more details.
[52] See pp.412–14 in Prescott et al. (2016) for more details.

the value closest to the originally submitted value that would not be inconsistent with the respondent's other answers. When an answer was clearly unreasonable or missing, and there was no workable single imputation procedure, we applied multiple imputation methods to calculate substitute values for the original missing or unreasonable survey entries.

We also reviewed by hand the values of reported earnings, occupations, and industries, due to their importance in our work. With regard to compensation, we manually reviewed all reported earnings greater than $200,000 per year and cross-checked them with the individual's job title and duties to ensure the amount seemed appropriate. We also examined potential typos in the number of zeros (e.g., the sizable real-world difference between $20,000 and $200,000 may be missed on a screen by survey respondents) by comparing reported annual earnings to expected annual earnings in subsequent years. If a typo was made by omitting a zero or by including an extra zero, we would expect to see a ratio of 0.1 or 10. We imputed earnings that were unreasonable if we were unable to correct the entry in a reliable way. With regard to occupation and industry, we had respondents self-select two-digit NAICS and SOC codes within the survey and also report their job title, occupational duties, and employer's line of business. To verify the two-digit NAICS and SOC codes—which are crucial for both weighting and fixed effects in our empirical work—we had four sets of RAs independently code the 11,529 responses by taking job titles, occupational duties, and employer descriptions and matching them with the appropriate two-digit NAICS and SOC codes.[53] As part of this process, we found that 24 individuals in the sample were self-employed, worked for the government, or were retired, thus reducing our total number of respondents to 11,505.

F.4 Sample Selection

As we observe above, there are four primary sample selection concerns with an online survey like ours: (1) not everybody is online; (2) not everybody online signs up for online surveys; (3) not everybody who signs up for online surveys receives a particular survey; and (4) not everybody who receives a survey manages to complete it. We describe these issues in greater detail in Section II.E in Prescott et al. (2016). All survey research must confront issues (1), (3) and (4)—the only unique selection concern for online surveys is (2). The key question is why individuals sign up to take online surveys and whether that reason is associated with their noncompete status or experiences.[54] To understand why the individuals who responded to our survey agreed to take online surveys, we asked them directly, and their responses were tabulated in Table 13 in Prescott et al. (2016). The two most common reasons individuals report to explain their interest in taking online surveys are that they enjoy the rewards (59%) and sharing their opinions (58%). Only

[53] See p.422 of Prescott et al. (2016) for details.

[54] 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.

40% indicated that they wanted money, and only 23% claimed that they needed money. Taking these responses seriously, the crucial selection question is, conditional on observables, whether individuals who like the available rewards or sharing their opinions are less likely to be in jobs that require noncompetes. We believe it is certainly plausible that there is no such relationship.

A related sample selection concern is that individuals who participate in a survey may for some reason lie or otherwise provide inaccurate information in a systematic way. We designed our cleaning strategy with the explicit goal of weeding out such individuals. But of course in any surveying effort legitimate concerns remain about the validity of the responses of the individuals who remain in the sample. To assuage these concerns, we present in Table OF1 the self-described job title, self-described occupational duties, and self-described industries for 15 randomly selected observations. These randomly selected respondents include a sales rep, a nurse, an analyst, a pizza delivery driver, an optometrist, and a programmer analyst. Reading their job-duty descriptions reveals a striking amount of detail, suggesting not only that these respondents answered the survey's questions carefully but also that they were responding truthfully.

Table OF1: Randomly Selected Self-Described Job Titles, Job Duties, and Industries)

	Self-Described Job Title	Self-Described Job Duties	Self-Described Industry
1	Associate Analyst	My current job duties are to review and evaluate telephone recordings between our customers and customer contact representatives.	My current employer is a regional utilitiy company which provides/sells electricity and natural gas to residential and commercial customers.
2	project manager	Design and staff community health clinics, write proposals, seek funding, evaluate and educate	Ensure children of low income families get preventive health and treatment if necessary
3	Quality Assurance Director	Review reports before going to our clients	Insurance Inspection Services
4	optometrist	Care for patient's ocular health	Optometry
5	purchasing clerk	I have receptionist duties including purchasing office supplies and filing the shipping department's paperwork.	retail art gallery
6	sales rep	account manager for a sales base	sells office supplies and equipment
7	Sales Associate	Sell phones and other communication devices, assist customers and resolve issues.	Retail sales company for cell phone business
8	Programmer analyst	Software developer	IT Consulting
9	Customer Service	I take phone calls from Customers.	My employer provides Health Insurance.
10	Certified Medical Assistant	Assist the doctor in the office and minor office procedures while making sure the office runs efficiently.	Healthcare provider
11	Analyst	researching our site's traffic	Publishing
12	Registered Nurse	I am responsible for providing dialysis services to current inpatients	It is a rehabilitation hospital
13	Title Coordinator	Process recorded deed of trust	Issue title policies
14	LEGAL ASSISTANT	INTERACT W/STATE BOARD OF WORKERS'COMP, PROVIDE PERSONAL INJURY REPRESENTATION, INVOLVES HIPAA LAWS	PERSONAL INJURY/WORKERS' COMP ATTORNEY
15	delivery driver	deliver food to people	pizza

F.5 Weighting and Imputation

In this section, we describe our approach to 1) weighting our survey data and 2) imputing values that are missing in our data or that we identified as problematic and marked as missing during the data cleaning process. The fact that weights need to be incorporated into the imputation step to impute unbiased population values complicates these two tasks. In line with current survey methods, we generated our analysis data by weighting our nonmissing data elements, imputing the missing variables (including the weights in the imputation step), and then reweighting the data given the imputed values so that the resulting analysis data are nationally representative. Below, after discussing our weighting approach, we explain how we combined weighting and multiple imputation methods to assemble our data.

With respect to weighting, we considered and compared several candidate approaches,[55] 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 evaluated a variety of potential weighting variables, and then we examined the ability of each weighting scheme to match the distributions of variables within the 2014 American Community Survey (ACS) (see Table 17 in Prescott et al. (2016)). Iterative proportional fitting, or raking, clearly performed better than alternatives in matching our data to the distributions of key variables in the ACS.

To assemble our analysis data, we began by using raking to calculate weights for our original nonmissing survey data. Next, we imputed our missing data. Our goal was to impute values for many different variables (see Table 18 in Prescott et al. (2016) for details), some of which were missing because of the cleaning process we describe above in Section F.4 and others because we added the relevant question to the survey while the survey was in the field. In addition, as we explain in the article, we also aimed to impute whether the "maybe" individuals are currently or have ever been bound by a noncompete. Because we sought to impute missing values across multiple variables, we employed Stata's chained multiple imputation command, which imputes missing values for all variables in one step. As suggested in Sterne et al. (2009), we incorporated all of the variables that we planned to use in our empirical analyses into our imputation model. Doing otherwise would have produced attenuated estimates.[56]

While imputing missing values just one time will allow for unbiased coefficient estimates, the associated standard error estimates will be too small because the predicted values will not convey the uncertainty implicit in those estimates (King et al., 2001). To generate unbiased standard error estimates, Graham et

[55] See pp.436–46 in Prescott et al. (2016) for more details.

[56] Dependent variables should be included as controls in the imputation of an independent variable to avoid attenuation in the imputed estimates (Sterne et al., 2009). See also `http://thestatsgeek.com/2015/05/07/including-the-outcome-in-imputation-models-of-covariates/`. Indeed, a general rule of thumb is that all variables involved in the analysis should be included in the imputation model.

al. (2007) recommend conducting at least 20 imputations when the proportion missing is 30% (relevant for our "maybe" group). We added another 5 to increase power.

The exact mechanics for a given imputation step are as follows: First, we fit a regression model with our initial nonmissing data. Second, we simulated new coefficients based on the posterior distribution of the estimated coefficients and standard errors—this step is what gives us variation across the 25 datasets. Third, we combined these coefficients with the observed values of the covariates for the missing observations to generate a predicted value. For continuous variables, we used predictive mean matching in the third step. Specifically, we took the average of the 15 nearest neighbors to the predicted value. For binary variables, we employed a logit model to create the predicted value. We repeated this process 25 times for all missing values, creating 25 separate datasets.

Once we had 25 imputed datasets in hand, we reweighted within each dataset using the raking procedure we discuss above, so that each individual dataset is nationally representative. In Table 2 in the article, we present a comparison of the distribution of demographics between the 2014 ACS and our weighted and unweighted data. The table shows that the weighted data quite accurately match the distribution of contemporaneous ACS data and that the unweighted data indicate a much more skilled workforce, one that does not align closely with the U.S. labor force. This occurs because we employed quotas to ensure that more than 50% of our sample was composed of respondents with a bachelor's degree.

Estimation of our main analysis via multiple imputation involves running the regression model in question on each individual dataset and then aggregating the 25 different estimates using Rubin's rules, combining the within-imputation variance and the between-imputation variance into our standard error calculations. Specifically, for $i = 1, \ldots, M$ imputations, for a given estimate in a given imputation $\hat{\beta}_i$ and within-imputation standard error $\hat{se}_i$, the formula for combining the within and between variance is:

$$Var_{Total} = Var_{Within} + Var_{Between} + \frac{Var_{Between}}{M},$$

where

$$Var_{Within} = \frac{\sum_{i=1}^{M} \hat{se}_i}{M}$$

and

$$Var_{Between} = \frac{\sum_{i=1}^{M} (\hat{\beta}_i - \bar{\beta})^2}{M - 1}.$$

We note that standard regression statistics, like R-Squared, are not typically reported for regressions conducted with multiple-imputation data because there are 25 distinct estimates of each statistic. To give a rough approximation of fit, we report the mean of our R-Squared estimates.