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Evan Starr
University of Maryland
JJ Prescott
University of Michigan Law School, jprescott@umich.edu
Norman Bishara
University of Michigan

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The Behavioral Effects of (Unenforceable) Contracts*

Evan Starr
University of Maryland
estarr@rhsmith.umd.edu
Corresponding Author

J.J. Prescott
University of Michigan
jprescott@umich.edu

Norman Bishara
University of Michigan
nbishara@umich.edu

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Abstract

Do contracts influence behavior independent of the law governing their enforceability? We explore this question in the context of employment noncompetes, using nationally representative data for 11,500 labor force participants. We show that noncompetes are associated with reductions in employee mobility and changes in the direction of that mobility (i.e., toward noncompetitors) in both states that do and do not enforce noncompetes. Decomposing mobility into job offer generation and acceptance, we detect no evidence of differences in job search, recruitment, or offer activity associated with noncompetes. Rather, we find that employees with noncompetes—even in states that do not enforce them—frequently point to their noncompete as an important reason for declining offers from competitors. Our data further show that these employees' beliefs about the likelihood of a lawsuit or legal enforcement are important predictors of their citing a noncompete as a factor in their decision to decline competitor offers.

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1. Introduction

The economic usefulness of contracting is predicated on the idea that when disputes arise courts will enforce valid agreements to hold parties to their promises. If one party breaches or threatens to breach a valid contract, other parties can bring a lawsuit to collect appropriate damages or compel performance—and will do so if litigation is likely to result in enforcement and is not overly costly. The predictability of contract law and the availability of courts make promises costly to break, which in turn allows parties to depend on and invest in reliance upon these promises. An unstated corollary of this understanding is that invalid or unenforceable contractual provisions are of little economic value to either party because such agreements will have few if any effects on behavior. If an agreement cannot be enforced, parties will only comply with it if doing so is in their individual best interest at the time—but we usually assume that economic actors pursue their best interests absent any contractual requirement. An important further implication of this corollary is that unenforceable contracts should be rare or nonexistent in the real world.

Recent research, however, shows that this latter implication is not true—unenforceable contracts appear to be common in practice, reflecting a surprising decoupling of the use of contractual provisions and whether those provisions are legally enforceable. For example, as many as 73% of rental leases contain unenforceable clauses (Furth-Matzkin 2017), and employers regularly ask employees to sign noncompetes in states that will not enforce them (Starr et al. forthcoming, Bishara et al. 2015, Sanga 2018). The widespread use of unenforceable contract terms appears to challenge the longstanding presumption that a contract's enforceability is crucial to its ability to influence a party's behavior. But while scholars have reported the systematic use of unenforceable provisions, raised ethical concerns about their application (Kuklin 1988, Sullivan 2009), and hypothesized about their likely behavioral consequences (Blake 1960), we know very little about whether unenforceable contractual terms produce changes in *real world* behavior. An important question, therefore, is whether contracts matter independent of the law governing their enforceability.

An affirmative answer to this question would raise many new questions about the economic motivations for contracting and about how best to regulate the contracting process and substantive contract language to prevent potential abuse and manipulation.

We investigate this question in the context of employment-related covenants not to compete ("noncompetes), which prohibit employees from joining or starting competing enterprises within geographic and temporal limits. The noncompete context is appealing from a research perspective for several reasons: First, there is significant variation in the legal enforceability of noncompetes across U.S. states. Some states will not enforce them at all and others enforce them vigorously (Bishara 2011). Second, the intuition that noncompetes in particular might matter independent of their legal enforceability dates back to at least Blake (1960), who wrote:

> For every covenant that finds its way to court, there are thousands which exercise an *in terrorem* effect on employees who respect their contractual obligations and on competitors who fear legal complications if they employ a covenantor, or who are anxious to maintain gentlemanly relations with their competitors. Thus, the mobility of untold numbers of employees is restricted by the intimidation of restrictions whose severity no court would sanction.

An *in terrorem* effect occurs when individuals "voluntarily" adhere to contractual or statutory language in anticipation of legal consequences (e.g., a lawsuit) were they to choose not to comply. In theory, such an effect may result even with respect to illegal or unenforceable contract terms. In such cases, adherence might arise from, for example, inaccurate beliefs about the content of the law (Fisk 2002, Arnow-Richman 2006) or simply the financial and professional costs of responding to a frivolous lawsuit (Sullivan 2009). Yet we are aware of no empirical evidence of *in terrorem* effects resulting from unenforceable agreements, particularly in the employment contracting environment.[1] In fact, evidence to date on the important behavioral consequences of noncompetes derives almost entirely from enforcement-related policy differences across states and over time (Marx et al. 2009,

[1] For example, Arnow-Richman (2006) notes that "the effect of noncompete agreements on worker perceptions and behavior is an under-explored empirical question" (p. 966).

Garmaise 2009, Starr et al. 2018) and not from the use of noncompetes themselves. Existing research leveraging differences in state law (without data on contract use) simply cannot address whether noncompetes matter independent of the law (Bishara and Starr 2016).

To assess the extent to which noncompetes matter for employee mobility independent of the law and to provide direct evidence on the existence and scope of any *in terrorem* effects, we use data on 11,505 labor force participants and their experiences with noncompetes to analyze how noncompetes influence the rate and direction of employee mobility, along with its constituent subprocesses (e.g., search, recruitment, job offer receipt, and job offer acceptance). As a baseline, we first establish that individuals with noncompetes have, on average, a 17% higher competitor-specific reservation wage, 11% longer tenures, and a 54% greater likelihood of reporting that they will leave for a noncompetitor relative to a competitor compared to an observationally equivalent employee not subject to a noncompete. While we caution against a causal interpretation of our baseline mobility results because our analysis does not exploit any purely exogenous variation in the use of noncompetes, our estimates are robust to an extensive set of controls that include, among other novel variables, various restrictive covenants (e.g., nondisclosure, nonsolicitation, etc.) and employer-level poaching flows. We also use a within-individual differencing strategy to mitigate selection on employee- or employer-level unobservables, and diagnostic tests indicate that any selection on unobservables would need to be implausibly severe to overturn the relationships we identify (Oster 2017). Finally, direct survey evidence also supports the causal role of noncompetes for a significant share of our sample.[2]

We next address our primary research question: whether the employee mobility patterns associated with noncompetes depend on noncompete enforceability. Importantly, we find that the

[2] Among those who have entered into a noncompete in the past, 33% report that a noncompete has materially influenced a past mobility decision, with the two most common reported effects being delay in leaving the employer (11.6%) and departure from the industry entirely (12%).

relationship between noncompetes and mobility holds even in states where noncompetes are unenforceable. In fact, we discern no statistical differences in our baseline mobility patterns between states where noncompetes are enforceable and states where they are not.

To investigate the mechanisms underlying our mobility results and to explore how contracts might operate independent of law, we adopt a search and matching perspective in which employee mobility is contingent on the resolution of two subprocesses: (1) the acquisition of a job offer through recruitment by or search for an alternative employer, and (2) the employee's determination whether to accept that job offer. We detect no evidence that employees with noncompetes receive less recruitment attention or fewer job offers from competitors, but we do find that employees with noncompetes are recruited by and search for jobs at noncompetitors more frequently than competitors relative to individuals not bound by noncompetes. Tellingly, these results on mobility subprocesses are largely statistically indistinguishable across states with higher and lower levels of noncompete enforceability, with the exception that employees who sign noncompetes in high-enforceability states appear to be relatively more likely to report receiving job offers in the last year from noncompetitors—a finding that suggests employers may be more informed than employees about the law surrounding noncompetes.

The fact that employees with noncompetes do not search less, do not attract less recruitment attention, and do not receive fewer job offers seems inconsistent with our evidence that these same individuals have longer tenures—until one recalls that people can decline offers of employment. Approximately 40% of respondents bound by noncompetes—in both enforcing and nonenforcing states—report that a noncompete has been a factor in their declining a job offer from a competitor. We explore why this might be so, and we find that respondent beliefs about the likelihood of court enforcement of a noncompete and the likelihood of their employer suing them over their noncompete, as well as reminders about the existence of their noncompete by their employer, strongly predict whether individuals will cite their noncompete as influencing their reaction to an offer from a competitor. In contrast, we find no indication that the enforceability of noncompetes deters employees from accepting such offers. To our knowledge, our results constitute the first rigorous evidence of the *in terrorem* effect of noncompetition agreements.

The central contribution of this study is to highlight and unpack the possibility that even unenforceable contractual provisions may influence behavior. In the context of noncompetes, our analysis complements the existing literature on mobility, which focuses almost entirely on the role of the state-level enforceability of noncompetes, by stressing the important possibility that the existence of a noncompete itself can be critical to how long employees stay at their jobs and where they go when they leave, whether or not the noncompete is enforceable.

One important policy implication of our results derives from our evidence that noncompete-bound employees appear to refuse job offers due to their *beliefs* about the likelihood of a resulting lawsuit and about court enforcement—as opposed to whether a court would *actually* enforce their noncompete. This finding implies that some employers may use noncompetes even when they are fully aware that such provisions are unenforceable, cognizant of the fact that employee beliefs may be inaccurate and open to manipulation, for example, by reminding employees of their (unenforceable) noncompetes or by visibly pursuing frivolous litigation against former employees (Ganco et al. 2015). Thus, policymakers wishing to reduce mobility barriers should consider enacting laws that limit the *use* of noncompetes—perhaps by increasing the cost of using clearly unenforceable provisions—as opposed to supporting reform proposals that restrict noncompete enforceability in court, as they now tend to do. Furthermore, given that employee beliefs appear to play such an important role and that many noncompetes are actually unenforceable, future research should examine whether employee beliefs are unfounded and whether disabusing employees of any mistaken beliefs might encourage them to accept attractive offers from competitors.

The rest of this paper proceeds as follows: In Section 2, we outline our conceptual framework, detail the specific facets of the mobility process, and relate our research to prior scholarship on noncompetes, mobility, and *in terrorem* effects. In Section 3, we briefly describe our data and data preparation work. In Section 4, we explain our empirical strategy, report our results, investigate the robustness of our findings, and explore the mechanisms underpinning the patterns we observe. We discuss and conclude in Section 5.

2. Conceptual Framework and Prior Scholarship

In this section, we integrate the idea that noncompetes typically influence employee mobility choices via an *in terrorem* channel—i.e., employees comply with a noncompete because they anticipate legal consequences if they breach it, not because a court has already ordered them to comply with the agreement—with a canonical search and matching view of the labor market. We use this combination to understand whether and how noncompetes may differ in their mobility effects based on the enforceability of such provisions. Assuming endogenous search and recruitment effort (Burdett and Mortensen 1998, Manning 2003, Postel-Vinay and Robin 2002), we posit that employee mobility is contingent on the resolution of two subprocesses: First, through on-the-job search by the employee or employer recruitment (i.e., identifying and developing potential candidates, including those not presently engaged in on-the-job search), another employer must make an employment offer. Second, the offer's terms must reach some reservation threshold (wages, benefits, etc.) for the employee to accept it.

By definition, noncompetes that employees believe can be legally enforced will impose moving costs on any bound individual, and these costs will raise their competitor-specific reservation threshold, all else equal. If an employee with a noncompete receives an offer from a competitor that is below the individual's (higher) reservation threshold but above their counterfactual (lower) reservation threshold (i.e., if they were not bound by a noncompete), then the employee will turn down an offer they otherwise would have accepted. Fewer accepted offers produce lower mobility levels. Alternatively, if competing employers can discern which employees are bound by noncompetes, and the net return of attempting to hire someone who is or is more likely to be bound by a noncompete is lower than recruiting an individual who is less likely to be bound by a noncompete, these employers should make fewer offers to individuals bound by noncompetes (sometimes simply to avoid anticipated rejection), also resulting in less employee mobility.

Noncompetes only raise the costs of moving to "competitors," however, and therefore the mobility effects of noncompetes will be attenuated when employees can more easily search for employment opportunities with noncompetitors and when employers can freely recruit people in noncompeting industries. In an extreme situation, when noncompetitors and competitors are perfectly interchangeable as employers, noncompete-based moving costs should have no effect on overall mobility—though we would still expect employees bound by noncompetes to be more likely than other employees to move to noncompetitors. By contrast, if employment offers from noncompetitors are less forthcoming or those offers often propose something below an employee's reservation threshold—perhaps because noncompetitors are unwilling to pay for an employee's industry-specific human capital (Parent 2000)—we should observe negative mobility effects.

All of these predictions are based on the notion that noncompetes are legally enforceable. After all, it is the threat of the employer enforcing the contract in court that forces employees to internalize the costs of any breach (typically, in the form of an injunction against working for the poaching competitor along with the possibility of money damages). However, in the U.S., noncompete enforceability varies dramatically across states, with California and North Dakota courts refusing to enforce them at all in the context of job-to-job moves, while other states enforce noncompetes even when the employee is involuntarily terminated (Bishara 2011). Add to this fact the recent and unexpected empirical finding that noncompetes are at best only slightly less common in states that refuse to enforce noncompetes relative to states that enforce them vigorously (Bishara et al. 2015; Starr et al. forthcoming), and it seems vital to examine whether the mere existence of a noncompete—whether it is enforceable or not—may play an important role in explaining employee mobility patterns.

A few recent studies notwithstanding (Gurun et al. 2019, Carlino 2017), research generally indicates that mobility rates are somewhat lower in states where noncompetes are more rather than less enforceable in court (Fallick et al. 2006, Marx et al. 2009, Garmaise 2009, Balasubramanian et al. 2020). However, because they lack data on who is actually "bound" by a noncompete, these studies do not and cannot show that any effects of noncompete enforceability are driven by the behavior of employees actually subject to noncompete provisions, although this is an understandable way to interpret their findings. This implicit inference is in line with the canonical view: unless a noncompete is enforceable, it will necessarily have limited behavioral implications for the mobility choices employees make, and we can assume these away. Yet these studies cannot explain why noncompetes usage is so prominent in states that do not enforce them, and, perhaps more important, they cannot address the potentially distinctive effects that noncompetes might have themselves, independent of the law. One interesting implication of this logic is that existing state law-based studies may actually *understate* the combined effects of noncompete use and enforceability as opposed to the effects of enforceability alone (given noncompete prevalence).

In contrast, some legal scholars have long assumed that even unenforceable contracts can produce important behavioral changes in parties—i.e., even invalid or unenforceable contracts can have *in terrorem* or chilling effects on employee mobility (Blake 1960, Fisk 2002, Arnow-Richman 2006). With respect to noncompetes, one commonsense basis for this conjecture is employee "fear" of a (possibly noncredible) lawsuit, perhaps attributable to employees being uninformed about the law (Fisk 2002).[3] Sullivan (2009) expands on this point by arguing that employees may also be

[3] Merriam Webster defines *in terrorem* as "by way of threat or intimidation: serving or intended to threaten or intimidate." Coincidentally, the example given by Merriam Webster for using the phrase *in terrorem* is "overbroad covenants not to compete which have *in terrorem* effect on employees."

"unwilling to risk the resources needed to establish [a noncompete's] invalidity" even when they are well informed. Employees may know the law, be confident about how the law *ought* to apply to their noncompete, and yet still abide by the provision's terms to avoid the potential financial and opportunity costs of a protracted legal battle that they cannot afford or may (erroneously) lose (Arnow-Richman 2006).[4] Such costs may also be moral or reputational, if the employee feels bound to keep their "promise" not to compete or if they are worried about developing a reputation as being disloyal in their chosen industry or profession. Taken together, this alternative view suggests that the costs of even frivolous enforcement litigation and erroneous employee beliefs about the law and the likelihood of a lawsuit may virtually eliminate the practical importance of the actual enforceability of noncompetes as it relates to mobility choices.[5]

3. Data: The 2014 Noncompete Survey Project

We analyze nationally representative employee-level data from a large online panel of verified U.S. labor force participants. The data include extensive information on employee experiences with and beliefs about noncompetes along with detailed information about their current and past employment as well as relevant demographic information. The sample population are individuals aged 18 to 75 who are either employed in the private sector or in a public healthcare system or who are unemployed. The final sample comprises 11,505 respondents drawn from all states, industries, occupations, and other demographic categories. We briefly discuss key elements of the data in what follows. We offer more details in our appendix and an even more extensive

[4] A few legal scholars suggest that these rationales may lead employers to use the threat of noncompete litigation coercively (Bishara and Westerman-Behaylo 2012).

[5] A natural response to these arguments is to question why employees remain uninformed about the law if competitors interested in poaching them are legally sophisticated and well-informed about the enforceability of noncompetes. One might reasonably hypothesize that if competitors are informed as to enforceability, and can communicate what they know about noncompete-related enforcement to the people they recruit, we might observe mobility patterns in the receipt of offers and, particularly, in how employees respond to those offers (i.e., following contact with a competitor) that differ from mobility patterns in employee search behavior and exploratory recruiting activity (i.e., prior to employee contact with the competitor in question).

examination in Prescott et al. (2016), which is a technical paper that describes the collection, cleaning, and validation processes, an investigation into sample-selection issues, and our hand-coding of occupations and industries, weighting methods, and imputation procedures.

Due to the potential for noncompliance among online survey takers, we carefully cleaned our raw data before arriving at our final sample. These steps included identifying and removing repeat survey attempts (both within and across IP addresses), reviewing all free-form responses to questions to exclude respondents who were intentionally noncompliant, and dropping individuals who failed to pass attention filters (among other exhaustive measures—see the data appendix for more details). We used a flagging algorithm to identify up to 21 different possible inconsistencies or errors within an individual's set of survey responses, including, for example, whether respondents report their establishment (e.g., office) as larger than their entire employer and whether there were critical missing responses (see Table 7 of Prescott et al. (2016) for the full list). Only 1.8% of the final sample were flagged two or more times, with 82.2% receiving zero flags.

Although there are many benefits to using an online survey for this sort of study, one challenge is that there is no easy-to-calculate response rate. In contrast to surveys of members of a specific industry or organization where all possible respondents are in the population of interest, our survey's panel providers continuously sent invitations to a superset of potential respondents, many of whom were not in the population of interest or otherwise eligible, until they reached a previously agreed upon number of "complete surveys." Among those who start the survey, we can exclude anyone not in our population of interest (about 40%, Prescott et al. (2016) Table 2), but among those who receive an invitation but never start the survey, we simply cannot know whether they were in our population of interest. Our survey was marketed as a "work experiences survey" and online survey respondents skew toward being unemployed or out of the labor force (Prescott et al. (2016) Table 12), so it seems very likely that more than 40% of nonstarters were not in the population of interest. In addition, we used quotas within the survey to ensure representativeness of the sample by income, education, gender, age, and geography. These quotas mechanically reduce the response rate because, as a survey stays in the field and quota constraints begin to bind, respondents who would otherwise qualify for the survey become newly ineligible. Toward the end of a survey, when most quotas are full, thousands of e-mail invitations may be sent when only a handful of respondents satisfy the remaining criteria.

We can report two helpful response-related statistics: the final sample divided by all of those who started the survey who were within our population of interest (23%) and the final sample over the total number of invitations (2%). The true response rate lies in between these extremes. Our view is that these numbers, while perhaps on the low side, are in line with and likely better than response rates to random-digit-dialing surveys, which were typically 6% in 2018 (Kennedy and Hartig 2019) and are similar to other noncompete surveys (see comparison in Table A1). Importantly, and especially in this context, a low response rate is not problematic per se; bias results only when reasons for nonparticipation are correlated with unobservables and our outcomes of interest.

To give more context as to whom our survey respondents are and how seriously they took the survey, we randomly chose 15 observations from the dataset and report their self-described job titles, job duties, and industries in Appendix Table A2. The randomly chosen job titles include a few sales representatives, a pizza delivery driver, an optometrist, a nurse, a programmer, a legal aid provider, and others. The respondent-provided job descriptions are quite detailed, as are the industry descriptions. In cleaning the data, we reviewed all 11,505 self-described job titles, job duties, and industries by hand in the process of creating occupation and industry codes, and this review confirmed that the vast majority of respondents wrote thoughtful responses to these questions.

While our various respondent quotas ensured that we collected a sufficiently large sample of highly skilled employees (to account for their potential lack of representation in the online survey-taking population), one important question is whether our survey data are representative of the U.S. labor force as a whole. Appendix Table A3 compares the demographics in our raw survey data to the demographics of our population of interest in the 2014 American Community Survey (ACS). As Table A3 shows, our raw sample is more educated and more female than the ACS sample. To account for the quotas and to make sure that our data accurately reflect our population of interest, we created weights for our analysis using iterative proportional fitting ("raking") to match the marginal distributions of key variables in the 2014 ACS. Table A3 shows that the weighted data closely align with the American Community survey. In the end, our key findings are not sensitive to our weighting strategy, and results from analyzing unweighted data are consistent with those we present here in terms of magnitude and statistical significance.

One difficulty of studying noncompetes and their role in the employment setting is that some employees are uncertain about whether they have a noncompete. In response to a survey question asking (after defining a noncompete) whether the participant was currently bound by a noncompete, respondents could effectively respond "yes," "no," or "maybe." The unweighted distribution of answers is 15.2% "yes," 55.1% "no," and 29.7% "maybe," where the "maybe" category includes those who have never heard of a noncompete (24.8%), those who do not know if they have signed one (2.2%), those who do not want to say (0.23%), and those who cannot remember (2.5%).[6] Around 9% of labor force participants who have had a noncompete at some point in their lives report having unknowingly entered into a noncompete in the past only to discover at a later time that they had in fact indicated an intention to be bound (Starr et al. forthcoming). Thus, some fraction of individuals in the "maybe" category (and probably a much smaller fraction of individuals in the "no" category) are actually a party to a noncompete even though they are unaware of their status.

[6] Starr et al. (forthcoming) report that the unweighted distribution among the full sample for whether an individual has ever signed a noncompete in the past is 31.5% "yes," 41.5% "no," and 27% "maybe." We asked those individuals who answered "yes" or "no" how confident they were in their answer, and 74.2% report that they were completely sure, while 23% reported that they were fairly sure.

We address the uncertainty regarding whether an individual is subject to a noncompete in two ways, although, importantly, our main conclusions are insensitive to how we treat “maybe” respondents. First, to capture any effects of noncompetes when an employee is uncertain about their noncompete status but when an employer is nevertheless presumably aware, we use standard multiple imputation methods (e.g., King et al. 2001) to estimate whether individuals in the “maybe” category had likely entered into a noncompete, based on the characteristics of the individuals in the “yes” and “no” categories.[7] Effectively, we allocate the “maybe” respondents to either the “yes” or “no” groups and proceed as if there were just two groups. Our main tables below report results based on multiple imputation methods, except in those instances where respondents had to answer “yes” to be able to answer the question of interest. One concern with this approach, however, is that “maybe” respondents seem less likely to behave as if they are bound, especially if they have never heard of a noncompete or cannot remember signing one—even if they are actually bound by one. For this reason, we also separately analyze the mobility patterns of the “yes,” “no,” and “maybe” categories. The results from using this way of coding noncompete status are substantively similar to our primary results, and so we report them only in the appendix. Table 1 provides summary statistics for our variables of interest in our sample of the U.S. labor force. The table presents mean differences between those respondents who are and who are not presently subject to a noncompete.

<<COMP: Place Table 1 About Here>>

[7] Specifically, we use chained multiple imputation to create 25 different datasets. For each one, we impute whether the “maybe” respondents have ever signed a noncompete and, if so, whether they are currently subject to one, along with a variety of other variables with missing observations (see Prescott et al. 2016 pp.446–55). Per standard practice, we use dependent variables as controls in the imputation of the independent variable (e.g., noncompete status) (Stern et al. 2009) to avoid attenuation of our imputed estimates. We create our multiple imputation estimates of the effects of noncompetes by estimating each model on the 25 different but complete datasets, combining the point estimates, and correcting the standard errors to reflect the variation in the 25 estimates. The result is an overall estimate that accounts for the uncertainty surrounding the “maybe” group. Our unweighted multiple imputation estimates indicate that 19.9% of all individuals (which includes 16% of the “maybe” respondents) are currently bound by a noncompete.

Using this data, Starr et al. (forthcoming) provide the first systematic evidence regarding the use of noncompetes across the labor force, finding that roughly one in five U.S. labor force participants were bound by a noncompete in 2014. They establish that noncompetes are common for all types of employees, with even 19% of individuals in nonenforcing states bound by one, equivalent to the percentage of employees with noncompetes in the most vigorously enforcing states. For more information on the differences between employees with and without noncompetes, see Starr et al. (forthcoming).

4. Empirical Analysis

Identifying whether noncompetes matter for mobility in both enforcing and nonenforcing states is challenging, requiring both exogenous variation in the use of noncompetes and exogenous variation in noncompete enforceability. Prior studies relying on panel data to identify the effects of noncompetes have exploited exogenous differences or changes in state policies (Marx et al. 2009, Balasubramanian et al. 2020, Lipsitz and Starr 2020), but they do not employ information on noncompete use; such data are generally unavailable. In the handful of studies with any data on noncompete use, researchers have been unable to identify good instruments for the presence of a noncompete (Marx 2011, Lavetti et al. 2020, Johnson and Lipsitz 2020).[8] Our unique data on noncompete use are ample in terms of sample size, representative of the U.S. labor force, and offer comprehensive individual-level detail, but they are nevertheless cross-sectional. Accordingly, we deploy an ensemble of different but mutually reinforcing methods to study the relationship between noncompete use and mobility outcomes and subprocesses.

4.1 Empirical Strategy

[8] Probably the most natural candidate instrument for noncompete use is the relative enforceability of noncompete provisions—but as shown in Starr et al. (forthcoming), state-level noncompete enforceability is virtually unrelated to actual noncompete use in a state.

Our empirical approach is fourfold: First, we make use of the rich information in our data to estimate highly saturated models that incorporate a comprehensive set of controls, including many that are new to the noncompete literature and, to our knowledge, not present in any other data. Second, whenever possible, we use within-individual between-competitor-and-noncompetitor models, which net out common employee or employer fixed effects.[9] Third, adopting the method proposed in Oster (2017), which extends the method in Altonji et al. (2005), we explicitly test whether our results are likely to be driven by selection on unobservables.[10] Finally, we examine many distinct mobility-related outcomes and, when it matters most, we ask individuals directly about the effects of noncompetes on their behavior and their predictions for the future. In the end, our results—consistent across multiple approaches and many robustness checks—point in the same direction and tell a coherent story.

To measure the relationship between the existence of a noncompete agreement and various measures of mobility, we use OLS to estimate the following equations:

$$(1) \qquad Y_{iojs} = \beta_0 + \beta_1 Noncompete_i + \gamma X_{ij} + \omega_{oj} + \alpha_s + \varepsilon_{iojs};$$

$$(2) \qquad Y_{iojs} = \beta_0 + \beta_1 Noncompete_i + \beta_2 Noncompete_i * Enforceability_s + \gamma X_{ij} + \omega_{oj} + \alpha_s + \varepsilon_{iojs},$$

where Y_{iojs} represents various mobility-related outcomes including length of tenure, the competitor-specific reservation wage premium, and the likelihood of leaving for a competitor or a noncompetitor

[9] We intentionally left the terms "competitor" and "noncompetitor" in the survey undefined, in line with the vagueness and variety of actual noncompete contracts and consistent with allowing respondents to apply the individualized interpretation of these terms that they would use to construe the scope of any noncompete. For example, the survey asked respondents to estimate the likelihood that they would depart for a competitor in the next year and separately asked respondents for the likelihood that they would leave for a noncompetitor. The survey also used this approach to ascertain, by category of prospective employer (i.e., competitor versus noncompetitor), a respondent's recruitment experience, search effort, and offer receipt activity.

[10] There may be other objections to a causal interpretation. For instance, simultaneity bias or reverse causation may affect our estimates—e.g., in the tenure context, employers might ask employees who stay longer and rise through the ranks to sign a noncompete midcareer. As to this particular example, any concern should be small. In our sample, noncompetes are almost uniformly proposed at the outset of employment (Starr et al. forthcoming).

(and later, job search effort, recruitment experience, and the receipt of offers) for employee i in occupation o, industry j, and state s.[11] $Noncompete_i$ is an indicator for whether the employee is currently subject to a noncompete agreement. The set of controls ω_{oj} accounts for any 2-digit industry (NAICS) by 2-digit occupation (SOC) fixed effects while α_s captures state fixed effects.

To measure the enforceability of covenants not to compete, we leverage a continuous measure from Starr (2019), which reweighted a measure originally developed in Bishara (2011). As a robustness check, we also use the continuous enforceability measure Garmaise (2009) constructs in addition to simply dividing the sample into "enforcing" and "nonenforcing" states and re-running our baseline specification within each group using the categorization in Beck (2014). The continuous measures of enforceability (Bishara 2011, Starr 2019, Garmaise 2009) are constructed from primary sources as well as the well-regarded state-by-state treatises edited by Malsberger (2012), which describe the cases and statutes related to noncompete enforcement in each state. Malsberger scrutinizes all key legal issues, including whether a noncompete can be enforced when an employee is fired, whether additional contractual consideration is required beyond continued employment, and whether and when overbroad provisions are valid, among many others.

Ideally, to estimate the enforceability of noncompetes in a state, one would like to know both the conditions under which courts will enforce noncompetes and how likely those conditions are to occur. The Malsberger series provide information on the former, but no study, to our knowledge, possesses information on the latter. This distinction is important because if, for example, a state declines to enforce noncompetes when an employee is involuntarily terminated, but involuntary terminations are rare for employees in the state, then such a limitation on enforceability will have

[11] In our analysis, we cluster the standard errors at the state level to account for any within-state correlations in the disturbances, which is the appropriate strategy given the state-level enforcement of noncompetes (Moulton 1990; Bishara 2011). In specifications using data only from respondents in nonenforcing states, our results are mostly driven by California; few individuals represent North Dakota and Oklahoma in our survey data.

minimal practical significance. Thus it is theoretically and practically important to weight the dimensions of enforceability by their significance. Bishara (2011) does re-weight 7 dimensions of enforceability (measured from 0–10), though in a subjective way. Starr (2019) improves on this approach by using factor analysis to reweight the same scores as Bishara (2011) does. In contrast, Garmaise (2009) assesses 12 dimensions of enforceability (scoring each dimension with a 0 or 1) and adds them up, assuming each has the same influence on total enforceability. Given the potential importance of weighting, we prioritize the Starr (2019) index and use the Garmaise (2009) score as a robustness check, but all such measures are highly correlated and give similar results.[12]

We divide our individual-level controls, X_{ij}, into two distinct categories that we refer to as basic controls and advanced controls. Our basic controls include employee gender, a third-degree polynomial in employee age, employee education, hours worked by the employee per week, weeks worked by the employee per year, the interaction of hours worked and weeks worked, class of the employer (e.g., for profit, nonprofit), how the employee is paid (e.g., salary, hourly), employer-size indicators, an indicator for multi-unit employers, the log of the number of establishments in the employee's county-industry, the log of the state unemployment rate as well as the log of the size of the labor force in the state when the employee was hired. Our advanced controls add separate indicators for whether the employee has a nondisclosure agreement, a nonpoaching agreement, a nonsolicitation agreement, an IP assignment agreement, an arbitration agreement, and various employee benefits including a retirement plan, employer-sponsored health insurance, paid vacation, sick leave, and life insurance as well as indicators for how many employers the individual has had in the last five years, the types of confidential information the employee currently possesses (e.g., trade

[12] We normalize all continuous enforceability measures to have a mean of zero and a standard deviation of one (giving each state equal weight).

secrets), and measures of employee flow from the employee's employer to competitors, to the employer from competitors, and between competitors in the industry generally.[13]

We include this extensive set of controls not only to improve the precision of our estimates but also to help separate treatment from selection by controlling for potentially omitted variables.[14] For example, consider the possibility that high-quality employers also tend to be legally sophisticated and therefore more likely to use noncompetes, and that employees might especially enjoy working for high-quality employers relative to other employers. Such a scenario would produce a spurious negative relationship between noncompetes and mobility. To account for this possibility, our model controls for measures of employer quality such as employer size, whether the employer is a multi-unit entity, the use of other postemployment restrictive covenants and benefits (that legally sophisticated, high-quality employers might also use), and poaching flows to and from the employer (to capture the employer's relative attractiveness to the focal employee). Selection concerns may push in the opposite direction as well: if employees vary in their underlying propensity for mobility and employers use noncompetes in an attempt to "root" their most valuable or mobile employees, noncompetes may prove to be associated with more rather than less mobility. We address this possibility by including controls for the number of employers an employee has had in the last five years (i.e., a measure of previous mobility) and for the types of confidential information the employee currently possesses (e.g., trade secrets, client information).

[13] The inclusion of these advanced controls reduces the sample size from 11,462 to 11,010 because some of the questions were not presented to all respondents. Unreported results from analysis that excludes the offending controls (which are benefit-type indicator variables) does not materially change the magnitude or statistical significance of our estimates. We omit earnings as a control from our regressions because compensation is also an outcome and an employee's noncompete status may directly affect their salary or wages.

[14] We acknowledge that some of these advanced controls may also be "bad controls" in that a noncompete may affect mobility outcomes *through* these covariates (Angrist and Pischke 2008). Our view is that our advanced controls are more likely to eliminate omitted variable bias than operate as bad controls, but for sake of completeness, we report estimates from specifications with and without both basic and advanced controls (below and in the appendix).

We complement this analysis with an additional, more powerful empirical strategy: using within-individual differences as outcome variables in our regressions. Specifically, we subtract each employee's particular mobility outcomes (e.g., the probability of leaving within the next year) with respect to "competitors" from the same outcomes with respect to "noncompetitors." With this approach, we eliminate any individual- or employer-level fixed effects that have equally strong mobility implications for both competitors and noncompetitors. Consequently, in these specifications, only factors that might *differentially* privilege competitors or noncompetitors remain as sources of omitted variable bias. Such concerns could arise, for example, if employers who are more likely to lose employees to competitors (as opposed to noncompetitors) are also more likely to use noncompetes. This particular scenario seems realistic, so we include controls for an employer's poaching flows (both hires and departures). More important, a bias of this sort would cause us to find that noncompetes are associated with an increased propensity for moving to a competitor over a noncompetitor, the opposite of what we actually find in our data.

Notwithstanding our extensive set of controls and our within-individual differencing strategy, we cannot control for every possible determinant of employee mobility that might correlate with noncompete status. Thus, as another check, we assess the stability of our baseline results to potential selection on unobservables (Oster 2017). The essence of this idea is that we can learn about selection on unobservables by analyzing selection on observables—that is, by comparing coefficient and R-squared movements between less saturated (by purposely omitting observables) and more saturated models.[15] Applying this approach, we report a test statistic, δ, in our regression results that captures

[15] The intuition of the our selection test is straightforward: when the R-squared rises significantly when moving from a less to a more saturated model, but the magnitudes of the estimated coefficients in question remain the same, we can be relatively confident that selection on unobservables does not account for our results because there is comparatively too little unexplained variation remaining (relative to the maximum possible R-squared) to plausibly explain away the sign of the estimates. If, in contrast, the coefficient estimates fall dramatically as we add additional controls, or the R-squared does not change at all with the inclusion of the new controls, then we should be relatively less confident in the results. We use Oster's (2017) Stata command "psacalc" in our work.

how severe selection on unobservables would need to be relative to selection on (our extensive set of) observables to drive our estimated treatment effect to zero. A value of $\delta = 1$ indicates that selection on unobservables would have to be just as strong as selection on observables to explain away the direction and magnitude of our results. Oster (2017) recommends a threshold of $\delta > 1$ as reliable measure of estimate stability if the regression in question controls for all first-order outcome determinants, and we use this cutoff as our touchstone.[16]

4.2 Baseline Mobility Patterns and Enforceability Effects

In Table 2, we present our baseline results on the relationship between noncompetes—precisely, having a noncompete—and employee mobility. Panel A examines three dependent variables: an indicator that the employee believes they will never leave their current employer for a competitor, the log of the reported wage premium minimally required for a competitor to poach the employee (conditional on the respondent being open to leaving their current employer for a competitor in the first place),[17] and the employee's tenure with their current employer. Employee tenure is not a traditional measure of mobility in that it does not track job-to-job transitions, but it does capture the absence of prior mobility. Theoretically, the probability of a move is tightly linked to observed tenure: for instance, a 10% probability of departure in each year results in an expected tenure of 10 years. For each dependent variable, we estimate separate models with just our basic and with both our basic and advanced controls, and we report the corresponding δ value from the selection-on-unobservables test that compares these two models.

<<COMP: Place Table 2 About Here>>

[16] The other parameters that we must choose to calculate this selection statistic are the maximum plausible R-squared for the regression, which, per Oster's recommendation, we set at 30% higher than the R-squared in the most saturated model, as well as the two sets of controls to allow us to compare changes in the R-squared and coefficient estimates. We implement the test by comparing the results of the model with our "basic" controls and fixed effects to the same model with our "advanced" set of controls.

[17] We operationalize the required wage premium in our empirical work as follows: If an individual reports that they would need a 10% raise to join a competitor, the dependent variable would take on a value of *ln(10)*.

Our data indicate that employees with noncompetes are as much as 5.7 percentage points more likely to say that they will never leave their employer for a competitor, a 69% increase relative to the sample mean of 8.3%. Among those who are open to joining a competitor, noncompetes are associated with a 17% (=$e^{0.16}$) increase in the required competitor-specific wage premium (i.e., not wage level). For tenure, noncompetes are associated with an additional 0.74 years of tenure on average, which is an 11.3% increase relative to the sample mean of 6.6 years. The δ statistic is greater than one in each of these specifications, suggesting that it would take implausibly serious selection on unobservables for our results to be due entirely to selection bias.

Panel B of Table 2 examines three dependent variables related to the direction or type of employee mobility: the subjective probability (on a scale from 0–100) that the employee will leave for a competitor in the next year, the subjective probability that the employee will leave for a noncompetitor in the next year, and the within-individual difference between these two outcomes. The specification with advanced controls suggests that employees with noncompetes are 4.4 percentage points less likely to leave for a competitor (a 35% reduction relative to the sample mean of 12.7%) and 0.53 percentage points less likely to leave for a noncompetitor, with only the former estimate statistically significant. The estimate of the within-individual difference between a departure to a competitor versus a noncompetitor indicates that employees with noncompetes are 3.9 percentage points less likely to move to competitors versus noncompetitors relative to an individual not bound by a noncompete, which is 54% of the sample mean. The δ selection-test terms are greater than one for the competitor model and the within-individual difference model.

This baseline analysis documents two important empirical relationships. First, noncompetes result in longer employment tenures, which translates to lower overall employee mobility. Second, noncompetes redirect employees who move from competitors toward noncompetitors. While other research has anticipated these relationships by studying noncompetes in particular professions (Marx

2011, Lavetti et al. 2020) or using state-level legal variation (Balasubramanian et al. 2020), our results are the first to emerge from an analysis of a representative sample of the U.S. labor force that incorporates information on the actual use of noncompetes and comprehensive employee-level data.

Before turning to whether noncompetes matter independent of the enforceability of such provisions, we explore the robustness of these baseline conclusions by addressing a number of potential data and methodological objections. To begin with, one natural concern is that our findings may somehow be driven by our use of imputation or the precise way in which we impute the status of our "maybe" respondents. As we note in Section 3, however, we address this concern directly by separately analyzing those who report having agreed to noncompetes ("yes") and those who were uncertain about their noncompete status ("maybe") in Appendix Table A4. We find that "yes"-respondent behavior explains the key estimates we present in Table 2.

Doubts may also remain about our identification strategy. We use an extensive set of controls, present within-individual estimates that should difference out most employee-level and employer-level selection effects, and confirm that any selection on unobservables would need to be implausibly severe to account for our results. Furthermore, our redirection results, in particular, seem at odds with selection stories in which (1) the "best" employees are required to sign noncompetes and/or (2) the "best" or more sophisticated employers disproportionately use noncompetes. Given our results, these stories would imply that the "best" employees or employees at the "best" employers are relatively *more* likely to leave their industry altogether absent a noncompete, which seems counterintuitive at the very least. Together, these strategies and the consistency of our results across outcomes should alleviate at least some of these lingering identification concerns.

Nevertheless, we also confirm our primary mobility results using a different and rarely available empirical approach, at least when studying a sample as large as ours: asking individuals directly. In particular, we ask survey respondents who have agreed to a noncompete at some point in the past whether a noncompete has ever affected their prior mobility decisions and, if so, how. Table

3 summarizes their reactions to this question and shows that a large fraction of these respondents do report that noncompetes have limited their mobility (11.6% implicate noncompetes in their longer tenures) and have redirected them toward different industries (12% report noncompete-based redirection) in the past. Overall, 33% of those who have signed a noncompete in their lifetime (and 40.7% of the same group with more than a bachelor's degree) acknowledge that noncompetes have materially influenced their mobility decisions in some way.[18]

<<COMP: Place Table 3 About Here>>

Given the evidence that noncompetes affect employee mobility generally, we now investigate whether the relationship between noncompete status and our mobility measures changes in important ways when we allow noncompetes to have differential effects based on their enforceability. If the enforceability of a contract accounts for most or all contract-related behavior, we should expect to see significant differences in employee mobility across distinct enforcement regimes. To test for this possibility, we repeat our regression analysis but add to our specification an interaction term between state-level noncompete enforceability and noncompete use (see equation (2)). We report results, only for the most saturated models, in columns 3, 6, and 9 of Table 2.[19] In each specification, the main effect we estimate for noncompete use maintains a similar magnitude and statistical significance

[18] Interestingly, noncompete experiences and behavior vary across education levels. For instance, 8–9% of individuals with a noncompete and at least a bachelor's degree felt "obligated" not to join a competing employer, compared to only 5.6% of those with less than a bachelor's degree. Relatedly, the most educated individuals with noncompetes were more likely to remain with an employer out of fear of subsequently facing litigation if they were to leave, hinting at an employee belief that jilted employers may target higher-skilled individuals with enforcement actions. Lastly, the highly educated are more likely to leave and wait for a noncompete to expire before joining a competitor, potentially because they can afford the opportunity cost of such a choice. They are also the most likely to geographically relocate or go back to school in response to a noncompete's constraints. In line with this pattern in the data, we recognize that prior noncompetes may have affected the existing distribution of occupations and geography of employees. To understand the extent to which this sorting may occur, we asked respondents directly if a prior noncompete (which could be theirs, a spouse's, etc.) was a factor in determining where they currently live or work. Overall, 11% of those who report ever signing a noncompete also report that a prior noncompete was a factor in determining where they currently work, though there exists a substantial difference between those with (14%) and without (8.3%) bachelor's degrees. Furthermore, 6.9% of those who have ever signed a noncompete report that a prior noncompete was a factor in determining where they live. This latter figure is somewhat larger for those with a bachelor's degree (8% vs. 6%).

[19] We report less saturated models without state fixed effects in Table A5.

relative to our earlier results. In contrast, the estimated coefficient on the noncompete enforceability interaction term is small in magnitude in all specifications and statistically insignificant in all but one case (in which it points in the "wrong" direction). Moreover, across models, the sign on the interaction coefficient oscillates. To visualize these effects, Figure 1 shows the marginal effect of a noncompete in an entirely nonenforcing state relative to an average enforcing state. As the interaction analyses suggest, the marginal effect of a noncompete is largely similar in states that do not enforce noncompetes and in states that do.

<<COMP: Place Figure 1 About Here>>

To provide more evidence on this point, Table A6 splits out the sample by the states that enforce noncompetes at least at some level and those where noncompetes are virtually unenforceable. Consistent with the results in Table 2, the estimates point in the same direction and maintain similar levels of statistical significance regardless of noncompete enforceability. That is, even in states that do not enforce noncompetes, employees who agree to be bound by such provisions show generally lower mobility levels and evince redirection toward noncompetitors.[20] Table A7 repeats the main analysis in Table 2 but uses Garmaise's (2009) measure of noncompete enforceability as an alternative approach to probe the robustness of our conclusions. Again, the results we estimate are substantively similar, with the exception that one measure—willingness to leave for a competitor—appears to be marginally higher in states that more vigorously enforce noncompetes.

4.3 Mechanisms, the Mobility Process, and the In Terrorem *Effect*

Our evidence that noncompetes are associated with longer tenures (retention) and more departures to noncompetitors relative to competitors (redirection), regardless of the law, says little about precisely how noncompetes produce these mobility consequences. From a policymaking

[20] Note that with only three nonenforcing states, per Beck (2014), clustering on state in the nonenforcing sample effectively reduces the sample size to three. Accordingly, we report robust standard errors in the nonenforcing specifications, which yield more conservative results.

perspective, designing welfare-enhancing interventions requires understanding how noncompetes operate at a behavioral level to influence employee decisions. In this section, we explore empirically the mechanisms that might underlie the two noncompete-mobility relationships—retention and redirection. We accomplish this by building on the search and matching literature's recognition that mobility derives from the generation of job offers from alternative employers and from employee decisions to accept these offers. In theory, noncompetes can affect mobility outcomes by modifying the function of one or both of these subprocesses.

4.3.1 Employment Offer Generation

We proceed by estimating variants of equation (1), replacing our previous dependent variables to reflect the discrete elements that together constitute the mobility process (e.g., search) rather than mobility itself (i.e., an employee receiving an offer, accepting an offer, and departing to a new employer). Table 4 gives our main estimates for the relationship between the presence of a noncompete and each of three different dimensions of the offer generation process: recruitment, search, and offer receipt.

<<COMP: Place Table 4 About Here>>

Panel A examines whether an employee's noncompete status is associated with their having been recruited in the last year, one obvious precursor to an offer. The advanced controls are important to this analysis: when we include only our basic controls, those bound by a noncompete appear to be more attractive hiring prospects to alternative employers. Respondents with noncompetes report receiving more recruitment attention all else equal. But this difference disappears once we include our advanced controls; specifically, competitors are no more likely to recruit employees with noncompetes (column (2)) in the last year relative to other employees. Employees with noncompetes are also relatively more likely to receive attention from noncompetitors (column (5)). Our within-employee difference estimates (column (8)) confirm that employees with noncompetes are relatively more likely to receive attention from noncompetitors than from competitors (with no evidence that they receive less attention from competitors).

The results we present in Panel B of Table 4 for on-the-job search effort largely mirror the findings of our recruitment analysis.[21] We estimate that search effort directed toward competitors in the last year, as measured from 0 to 10, is slightly lower for those who sign noncompetes, but not statistically significantly so (column (2)), while the coefficient we estimate on search effort toward noncompetitors is large, positive, and statistically significant (column (5)). Specifically, noncompetes are associated with an additional 0.4 units of search effort toward noncompetitors (12% of the sample mean). The relationship between a noncompete and the within-individual difference in search toward competitors and noncompetitors is large and negative (-0.5 units, column (8)), implying that employees with noncompetes are relatively more likely to redirect their on-the-job search effort away from competitors and toward noncompetitors.

Taken together, we find no evidence that noncompetes themselves dissuade competitors from recruiting employees with noncompetes nor do they appear to dissuade employees with noncompetes from searching for employment opportunities with competitors. Even so, our results help to account for our baseline finding that noncompetes are associated with employees being redirected toward noncompetitors, aligning nicely with the "career detours" hypothesis (e.g., Marx 2011). Indeed, our evidence illuminates at least two potentially important mechanisms underlying redirection: noncompetitors are relatively more likely than competitors to recruit employees with noncompetes, and employees with noncompetes are relatively more likely to search on the job for noncompetitor positions than for competitor positions. One theory that might explain this dynamic is that

[21] The results in Panel A and Panel B of Table 4 may capture just one result instead of two. We do not know in our data whether alternative employers might first have reached out to an employee who then engaged in "search" in response; nor do we know whether an employee might first have searched for and then contacted an alternative employer that subsequently "recruited" the employee as a result. If our respondents conflate recruitment and search to some degree, our two sets of results may capture the same (or partially overlapping) actual interactions.

noncompetes (even unenforceable ones) partially protect and therefore encourage employer investments that increase employee quality generally, making employees with noncompetes more attractive to all employers, including noncompetitors.

Mobility involving a new employer cannot occur without alternative employment offers, however. Table 4 also considers the relationship between noncompetes and employment offers received over the course of the last year (Panel C). The role of our advanced controls is also important here: our basic controls alone would lead to the conclusion that employees with noncompetes are more likely to receive job offers all else equal, but once we include the advanced controls in our regressions, we find no difference in the offer probabilities for both competitors and noncompetitors for the last year for individuals with noncompetes. Importantly, under both specifications, we find no evidence that those with a noncompete are *less* likely to receive job offers from alternative employers. We do observe that employees with noncompetes are more likely to receive an offer from a noncompetitor during their tenure than from a competitor, but the estimate is only marginally statistically significant (see Table A8).

In sum, the results we present in Table 4 demonstrate that employees with noncompetes are not shut out and do not opt out of the *early stages* of the mobility process as some might predict (and as some employers might intend). We find no evidence to indicate that those who are bound by noncompetes receive less recruiting attention from competitors, nor is there evidence to support the idea that employees with noncompetes forgo searching for new positions with competitors. Moreover, the data demonstrate that, in fact, individuals with noncompetes engage in search activity directed at noncompetitors and are recruited by noncompetitors at significantly higher rates than employees without noncompetes. We also find that employees subject to noncompetes appear somewhat more likely to receive offers from noncompetitors across their (longer) tenure.

We now return to our primary focus—the possibility that noncompetes matter to the mobility process regardless of state-level enforceability—by adding an interaction between noncompete enforceability and employee noncompete status to our regressions.

The recruitment results in Panel A of Table 4 indicate that while a noncompete is not associated with any statistically significant reduction in the probability of being recruited by a competitor (as before), the probability of a noncompete-bound employee being recruited by a competitor is somewhat lower in higher enforceability states. But this reduction may be spurious. Employees with noncompetes also appear less likely to be recruited by *noncompetitors* in states that vigorously enforce noncompetes (and to an equal extent), although on average noncompetitors still recruit employees with noncompetes at higher rates than other employees. Using our more robust within-employee differencing approach, we continue to observe a large relative difference in recruiting activity between competitors and noncompetitiors for those with noncompetes. By comparison, the estimated coefficient on the enforceability interaction is near zero and not statistically significant. Moreover, the associated standard errors imply that any enforceability effect would be minor (compared to the effect of having a noncompete). Thus, in our data, enforceability does not appear to deter competitors from recruiting those bound by noncompetes.

With respect to employee search behavior (Panel B), we discover a very similar pattern in our data: no statistically significant enforceability differentials for those bound by noncompetes. On the whole, employees with noncompetes appear to redirect search effort toward noncompetitors and away from competitors without regard to their state's level of noncompete enforcement, although, unlike with recruitment, the point estimate on the interaction points in the "right" direction. Enforceability appears weakly correlated with bound employees directing more search toward noncompetitors, but the point estimate is small—less than one-fifth of the main effect of simply having a noncompete, sizable even absent enforcement—and statistically insignificant.

We turn finally to examining the role of enforceability in explaining the relative likelihood that an employee with a noncompete has received a job offer in the last year (Panel C). Our results indicate that, in higher enforceability states, employees bound by noncompetes are no less likely to receive offers from competitors relative to employees without noncompetes (column (3)), but they are more likely to receive offers from noncompetitors (column (6)), such that individuals bound by noncompetes are more likely to receive offers from noncompetitors than competitors relative to the unbound (column (9)).[22] Put another way, individuals with noncompetes in our data appear to receive at least as many job offers from both competitors and noncompetitors, regardless of enforceability, as employees without noncompetes, but they receive relatively more offers from noncompetitors in states that enforce noncompetes more intensively.

Overall, as before, our evidence suggests that noncompetes matter regardless of noncompete enforceability. The story is more nuanced than in our baseline results, however. Drawing on our within-respondent estimates, differences in employer recruitment activity and employee search behavior associated with noncompetes do not appear to vary with the enforceability of noncompetes.[23] By contrast, the difference in whether an employee bound by a noncompete has received a job offer in the last year does vary somewhat with the enforceability of these provisions in the state. In higher enforceability states, those bound by noncompetes are relatively more likely to receive job offers from noncompetitors than from competitors in the last year (possibly following less recruitment but more search, according to our imprecise interaction estimates).

[22] In Table A8, we report the results of a similar analysis, using as an outcome instead whether an employee has received a job offer at any point while employed by their current employer. Our findings suggest that, as in the case of recruitment activity, the enforceability of noncompetes is associated with fewer job offers from competitors for those bound by noncompetes relative to other employees but job offers from noncompetitors also fall to the same relative extent (though the point estimate on the noncompetitor interaction is statistically insignificant), such that the estimated coefficient on the interaction term in the within-individual difference regression is small in magnitude and statistically insignificant. We de-emphasize these results because the likelihood of having had a job offer since the start of employment is correlated with an employee's tenure.

[23] Table A9 replicates Table 6 with the Garmaise (2009) measure of enforceability and finds similar results. We report less saturated models in Appendix Table A10, which show substantively similar results.

The potential asymmetries in these results (i.e., job offer behavior vs. on-the-job search and job offer behavior vs. recruitment activity) may reflect the fact that employers are relatively more knowledgeable about noncompete law than employees but also relatively uninformed about whether any particular employee is likely to have a noncompete (until after initiating recruitment). Whatever the mechanism, in the aggregate, these patterns suggest that employees bound by noncompetes in states where noncompetes are easy to enforce may be more likely to leave their industry due to higher offer rates from noncompetitors. But it is striking that there is no evidence that those with noncompetes are less likely to receive job offers (from either competitors or noncompetitors) in the prior year, regardless of the degree of noncompete enforceability.

4.3.2 Employment Offer Response

The puzzle that emerges from our work is that individuals with noncompetes appear to engage in no less on-the-job search, entertain no less recruitment activity, and receive no fewer offers from alternative employers on average, and yet they are less mobile, remaining at their employers longer than employees without noncompetes (regardless of noncompete enforceability). We hypothesize that one potential solution to this puzzle could be that, despite having similar job offer opportunities in a given year (relative to those without noncompetes), individuals bound by noncompetes simply decline their job offers at higher than average rates in both enforcing and nonenforcing states. To investigate this possibility, we return to our data to determine whether a noncompete influences an employee's decision to decline an offer. We also explore how that decision relates to an employee's beliefs about their employer's likely reaction to any departure to a competitor and the law's likely treatment of any legal action the employer pursues in response to such a departure (regardless of actual noncompete enforceability).

Panel A of Table 5 presents a simple tabulation of answers in response to the question: "Was your noncompete a factor in your choice to turn down the offer from a competitor?" Of applicable respondents—those who had noncompetes and turned down an offer from a competitor—41.5% indicated that their noncompete was indeed a factor in their decision to decline their offer. This proportion is similar in states that do (42.3%) and do not enforce (37.5%) noncompetes. Not all employees receive offers from competitors, however; to gain statistical power, we also evaluate responses to the following question: “If you received an offer from a competitor, would your noncompete be a factor in your choice to reject it?” A comparable 47.6% report that a noncompete would be a factor, with similar proportions in enforcing and nonenforcing states. Panel C tabulates the importance of a noncompete as a factor, with 54.2% of those who report it would be a factor stating that a noncompete would be somewhat, very, or extremely important in their decision. According to this evidence, employees with noncompetes are declining offers from competitors at significant rates because of their noncompete.

<<COMP: Place Table 5 About Here>>

To probe why employees may view their noncompete as a barrier to accepting a job offer (and becoming mobile), we begin by asking why respondents who indicate that their employer remained unaware of the competitor’s offer nevertheless attribute their choice to decline an employment offer to their noncompete status. The estimates in column (1) of Table 6 show that individuals who know that their employer has sued a former employee to enforce a noncompete agreement in the past are 16 percentage points more likely to report their noncompete as a reason for turning down a competitor’s offer. Furthermore, individuals who are “certain” that their employer will sue them over their noncompete and that a court will enforce that agreement are 61 percentage points more likely to answer that a noncompete was a factor in their choice to decline an offer relative to individuals who are certain that their employers will not sue and that a court will not enforce their noncompete. Importantly, employee beliefs about noncompete enforceability and beliefs about the likelihood of an employer filing a lawsuit are much more predictive of a noncompete being a factor in refusing an offer than actual enforceability under state law, which we estimate to play a very small and statistically insignificant role (column (2)).

<<COMP: Place Table 6 About Here>>

Once an employer is made aware that an employee has received an offer from a competitor, the employer has the option of intervening and reminding the employee of their contractual obligation not to compete by joining a competitor. In column (3) of Table 6, ignoring at first this possibility of strategic “reminders,” we report the coefficients we estimate for this population and find that they are very similar to those in column (2). However, when we account for the fact that employers often remind employees of their noncompete upon learning of the offer (which occurs 40% of the time), the coefficients on our subjective belief variables fall substantially while the coefficient on “reminding” is large at 40.7 percentage points (column (4)). These estimates imply, first, that an employer’s choice to simply remind its bound employees of their noncompete obligations has a strong positive association with whether an employee will turn down an offer from a competitor and, second, that employees who receive reminders are far more likely to believe that their employer will sue them and/or that a state will enforce their noncompete.[24]

5. Discussion and Conclusion

The core idea we explore in this paper is whether contracts (noncompetes) influence economic behavior (the rate and direction of mobility) independent of the governing law—i.e., do unenforceable contracts matter on the their own and in comparison with enforceable ones? In the

[24] Only 21% of our sample reports receiving an offer from a competitor, so we test the robustness of this analysis using the same specification to study whether an employee’s noncompete *would* be a factor in a hypothetical case in which the employee receives an offer from a competitor (i.e., the question and responses listed in Panel B of Table 5). The sample of respondents answering this question includes all individuals with noncompetes (both reported and imputed). We find results similar to the estimates we report in column (2) of Table 6. Specifically, an employee’s beliefs about the likelihood of a lawsuit and the likelihood that a court would enforce the noncompete in question are strongly positively associated with the choice to report that a noncompete is a factor in the choice to decline an offer.

context of covenants not to compete, research to date focuses almost entirely on differences in state enforcement policies, likely due to limited access to data on existing contracts. But contracts on paper and in hand may produce *in terrorem* behavioral effects without regard to "law," and legal scholars have speculated for years that unenforceable, invalid, and even unlawful contracts may dramatically alter party behavior, particularly when the party is unaware of the law. Accordingly, this paper's most important contribution—which derives from a novel survey effort to gather nationally representative data on noncompete use—is to empirically substantiate the hypothesis that contracts matter independent of the laws governing their enforceability.

To this end, we find that a noncompete is associated with both a longer tenure and a reduced propensity to leave for a competitor even when the noncompete in question is unenforceable under state law. Moreover, we decompose the mobility process into its subcomponents (offer generation and offer response) to identify where these mobility differences might accrue. The offer generation analysis reveals no evidence that employees bound by noncompetes engage in less search, experience less recruitment, or receive fewer offers relative to those not bound by noncompetes, though individuals with noncompetes do exhibit increased search effort toward and appear to receive additional recruitment activity from noncompetitors. These results also appear to be largely independent of enforceability, with the exception that job offers from noncompetitors are more common for the noncompete bound in higher enforceability states (potentially due to asymmetric knowledge of the law). In turn, the offer response analysis demonstrates that—in both enforcing and nonenforcing states—approximately 40% of employees with noncompetes identify their noncompete as a factor in turning down job offers from competitors. Finally, we document that beliefs about noncompete enforceability and the likelihood of being sued, as well as simple reminders by the employer, are strong predictors of whether an employee will decline an offer from a competitor, while the actual content of the law appears to be irrelevant. Taken together, these results provide the first rigorous evidence of an *in terrorem* effect in the noncompete context, including with respect to unenforceable contracts, for which any threat to sue is unlikely to be credible.

Before we turn to future research and policy implications, we revisit and highlight several limitations of our study, especially as they relate to the prior literature. First, because our data come from a cross-sectional survey, we can only track the absence of prior mobility (job tenure) and cannot track actual job-to-job transitions by respondents. We overcome this challenge by using prospective questions about respondent intentions to measure future mobility as well as retrospective evidence on prior job moves, search effort, recruitment, and the receipt of job offers. To our knowledge, no longitudinal data on the use of noncompetes and actual mobility events exist for anything like a nationally representative sample of employees. Second, along with the few other studies of noncompete use (Marx 2011, Johnson and Lipsitz 2020), we lack a credible source of exogenous variation in noncompete use, so our point estimates should be interpreted as causal with caution. Our multipronged approach to corroborate our baseline results is to use a rich and comprehensive set of controls, to estimate within-individual competitor versus noncompetitor models whenever possible, to test for susceptibility to selection bias, and to simply ask employees directly about their experiences. The story that emerges is consistent and commonsensical.

Nevertheless, selection into noncompete status may be conditionally nonrandom. For example, our regressions with only basic controls indicate that employees bound by noncompetes are relatively more attractive to employers. This could be causal—e.g., if such employees are afforded access to training, trade secrets, or customer lists that make them more valuable—or it could instead reflect the types of employers that use noncompetes or the types of employees who enter into such agreements. When we add our advanced controls, employees with noncompetes no longer appear clearly more attractive. What do we make of this change? Are the advanced controls shutting off channels through which noncompetes influence mobility, or are they solving an omitted variables problem? Further inspection reveals that our estimates are most sensitive to the inclusion of other contractual provisions (e.g., nondisclosure, nonsolicitation, etc.). These contracting terms seem likely to reflect employer sophistication, and their effects likely load on the noncompete variable when we omit them from the model. Regardless, including these controls does little to change the result that noncompete enforceability seems unnecessary for noncompetes to affect behavior.

Similarly, because we do not possess longitudinal data to exploit some of the recent changes in noncompete laws, we are unable to carefully estimate the causal effect of noncompete enforceability. Instead, we document that those who are bound by noncompetes exhibit different mobility patterns than other employees, and that these differences persist in states that do and do not enforce noncompetes. With regard to this limitation, our results raise the question of how our findings cohere with the existing empirical evidence that employee mobility appears to be lower in states that enforce noncompetes (Marx et al. 2009; Garmaise 2009; Balasubramanian 2020). We do not believe there is a clear inconsistency with respect to our paper's main contribution. First and foremost, our analysis documents the existence of noncompete mobility effects in *both* nonenforcing and enforcing states, not that robust enforcement has no impact. Indeed, our work implies that existing work may *underestimate* the combined effect of (1) a noncompete in (2) an enforcing state by assuming that variation in enforcement identifies the full effect of a noncompete.

Still, we also find little evidence that the mobility effects of noncompetes vary across enforcement regimes, which at least seems inconsistent with prior scholarship. Beyond the fact that our interaction estimates simply fail to reject a null hypothesis of no difference, there are several explanations that may account for this discrepancy. We broadly group these possibilities as related to measurement, identification, and sampling.

In the measurement category, our mobility outcomes (tenure and subjective probabilities of leaving in the next year) are not the ones employed by the rest of the literature, which generally capture job-to-job mobility. Our findings may thus emphasize the importance of mobility measurement. Another potential measurement issue is how best to capture the enforceability of noncompetes. Some studies exploit changes over time (e.g., bans) (Balasubramanian et al. 2020, Lipsitz and Starr 2020), while others measure cross-state enforceability differences in some way. Given our data, the cross-state measures are the best we can do, and our results are largely robust to all reasonable and available measures. Measurement issues are also central to the interpretation and substance of other noncompete studies, which do not analyze information as detailed as our data on the actual use of noncompetes, job offers, beliefs, etc. As a result, these studies are unable to show that any effects of law they identify are attributable to those bound by noncompetes. Our work begins to fill this gap by stressing that a noncompete may matter independent of the law.

Causal identification—especially within a cross-sectional survey—is always challenging, and despite our numerous attempts to carefully understand the biases that might arise and to suitably address them, significant caveats remain in our paper. Identifying assumptions, of course, exist throughout the noncompete literature and may account for apparent inconsistencies.

Finally, it is worth underscoring the differences in our sample composition: prior studies examine inventors (Marx et al. 2009, Marx et al. 2015), high tech workers (Balasubramanian et al. 2020), CEOs (Garmaise 2009), and physicians (Lavetti et al. 2020). These occupations comprise a small fraction of the U.S. labor force (and of our sample), and the average labor force participant likely has a different level of sophistication with respect to these provisions and their enforceability relative to highly compensated employees. Understanding the effects of employment noncompetes across a diverse workforce is an important direction for future work.[25]

All of these limitations notwithstanding, our results—if replicated with other samples, data, and designs—have important implications for understanding employment mobility constraints, the

[25] Our work also raises several new questions for future research. What do employees and employers know about the law, and how is that knowledge gained and diffused across organizations, industries, and regions? Do employers know which types of potential employees are bound by noncompetes and how do they decide whether and when to recruit them? To what extent do these same issues extend to other, similar restrictive provisions, like nonsolicitation or nondisclosure agreements?

effects of unenforceable contracts and the noncompete public policy debate. Furthermore, assuming noncompete use has been rising over time, our results also have implications for the supposed decline in labor market fluidity (Davis and Haltiwanger 2014; Decker et al. 2014). Assuming our results accurately reflect at least the direction of noncompete effects, policymakers wishing to spur mobility via noncompete reform may want to consider policies limiting the use of noncompetes themselves rather than weakening enforcement. Information campaigns may also make a difference. The fact that beliefs about enforceability and reminders matter for mobility decisions, even where noncompetes are unenforceable, emphasizes the scope for *in terrorem* effects via misinformation and manipulation. Today, many employees may turn down a job offer they would have otherwise taken simply because they incorrectly believe their noncompete is enforceable.

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Tables & Figures

Figure 1. Marginal Noncompete Effect in Nonenforcing and Average Enforcing States

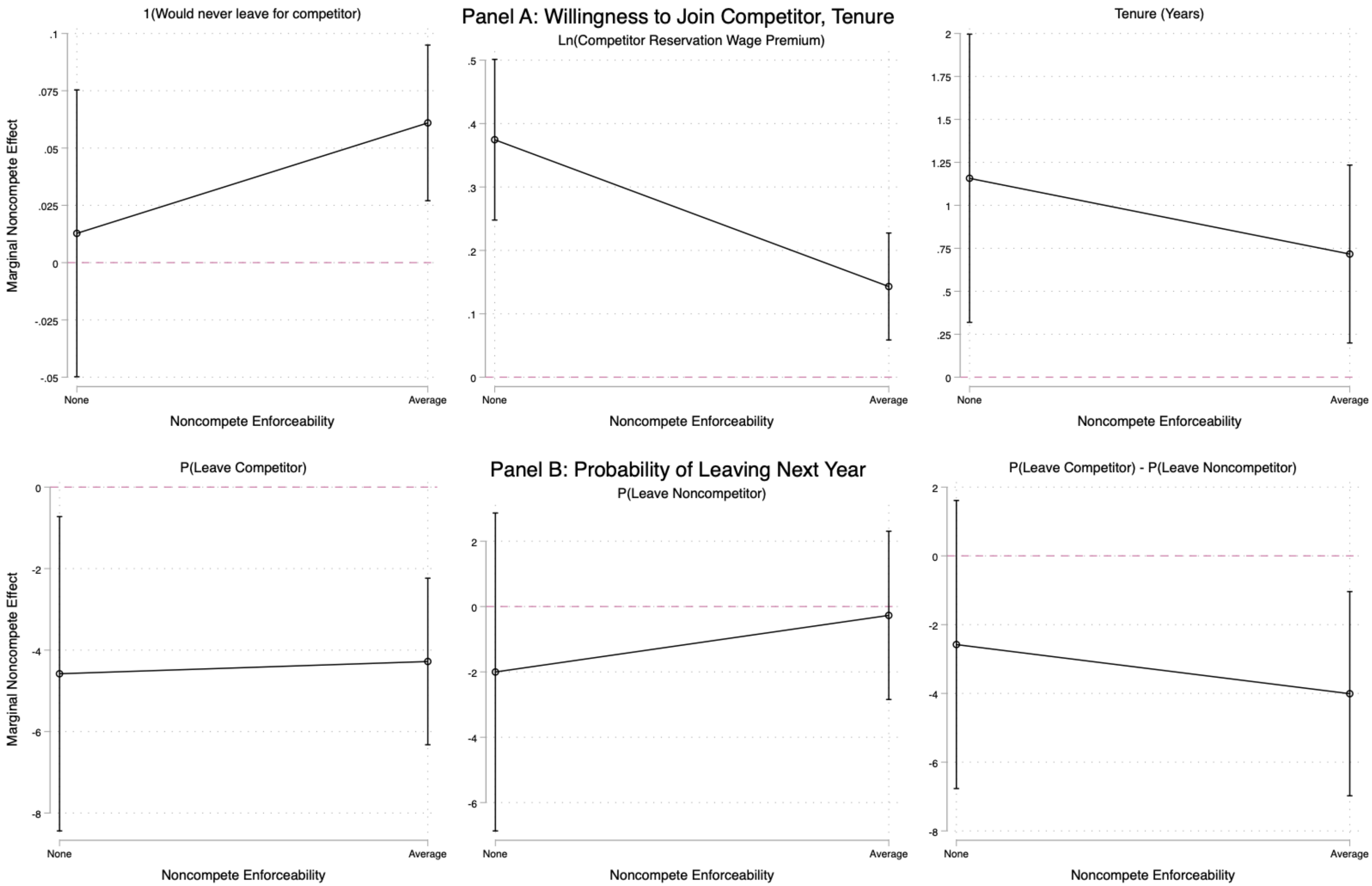

Table 1: Summary Statistics

Sample Statistic	All Mean	All SD	Noncompete Mean	No Noncompete Mean	Δ Mean
Enforceability of Noncompetes	-0.23	1.46	-0.24	-0.22	-0.02
Tenure (Years)	6.58	7.26	7.00	6.48	0.52
1(Would never leave employer)	0.08	0.28	0.11	0.08	0.03**
Ln (Required Competitor Wage Premium)	2.92	0.94	3.07	2.89	0.18***
P(Leave for a competitor)	12.70	21.94	12.05	12.84	-0.79
P(Leave for a noncompetitor)	19.96	29.11	20.96	19.74	1.22
Search effort towards competitor (0-10)	2.69	3.05	2.76	2.67	0.09
Search effort towards noncompetitor (0-10)	3.34	3.33	3.73	3.26	0.47***
1(Recruited by competitor in last year)	0.21	0.40	0.29	0.19	0.10***
1(Recruited by noncompetitor in last year)	0.25	0.43	0.38	0.22	0.15***
1(Competitor offer in last year)	0.10	0.30	0.15	0.09	0.06***
1(Noncompetitor offer in last year)	0.12	0.32	0.17	0.10	0.06***
1(Competitor offer while employed)	0.21	0.41	0.28	0.19	0.09***
1(Noncompetitor offer while employed)	0.25	0.44	0.36	0.23	0.13***
Subjective P(Firm will sue if leave for competitor)	38.44	35.83	37.88	38.57	-0.68
Subjective P(Court would enforce noncompete)	42.94	37.48	41.01	43.37	-2.36
1(Knows employer sued others re: noncompete)	0.06	0.24	0.20	0.03	0.16***

Note: Standard errors for the differences between the means for employees with noncompetes and those without noncompetes are clustered at the state level. In our sample, 18.1% of the respondents report that they are currently bound by a noncompete, and 38.1% have signed one at some point in their lives. See Starr, Prescott, and Bishara (forthcoming) for more details on who signs noncompetes. These estimates refer to the full sample of 11,505 respondents. The enforceability of noncompetes is from Starr (2019), and the measure is constructed to have a mean of 0 and a standard deviation of 1 in a sample in which each state has equal weight.

Table 2: Redirection and Retention

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Willingness to Join a Competitor and Tenure									
Dependent Variable	***1** (Will Never Move to a Competitor)*			*Ln (Required Competitor Wage Premium)*			*Tenure*		
Noncompete	0.048***	0.057***	0.060***	0.194***	0.160***	0.142***	0.498**	0.739***	0.713***
	(0.017)	(0.017)	(0.017)	(0.049)	(0.052)	(0.042)	(0.216)	(0.243)	(0.235)
Noncompete×Enforceability			0.012			-0.062***			-0.099
			(0.008)			(0.017)			(0.109)
Selection Test: δ		7.133			1.178			1.901	
Observations	11,462	11,010	11,010	10,650	10,241	10,241	11,462	11,010	11,010
R-Squared	0.103	0.121	0.121	0.093	0.111	0.113	0.431	0.492	0.492
Panel B: Subjective Probability of Leaving in the Next Year									
Dependent Variable	*P(Leave Competitor)*			*P(Leave Noncompetitor)*			*P(Leave Competitor)-P(Leave Noncompetitor)*		
Noncompete	-2.583***	-4.423***	-4.405***	0.952	-0.531	-0.411	-3.535**	-3.893***	-3.994***
	(0.845)	(1.022)	(0.994)	(1.371)	(1.180)	(1.289)	(1.339)	(1.352)	(1.485)
Noncompete×Enforceability			0.078			0.448			-0.370
			(0.427)			(0.699)			(0.668)
Selection Test: δ		3.311			0.315			8.074	
Observations	11,462	11,010	11,010	11,462	11,010	11,010	11,462	11,010	11,010
R-Squared	0.109	0.172	0.172	0.161	0.206	0.206	0.134	0.152	0.152
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the state level in the regressions with the measure of noncompete enforceability coming from Starr (2019). Note the main effect of enforceability is subsumed by state fixed effects. Basic controls include occupation-by-industry (2-digit SOC-by-2-digit NAICS) fixed effects, state fixed effects, employee gender, a third-degree polymomial in employee age, employee education, hours worked by the employee per week, weeks worked by the employee per year, the interaction of hours worked and weeks worked, class of the employer (e.g., for profit, nonprofit), indicators for how the employee is paid (e.g., salary, hourly), employer-size indicators, an indicator for multi-unit employers, the log of the number of establishments in the employee's county-industry, and the log of the state unemployment rate when the employee was hired, as well as the log of the size of the labor force within the state when the employee was hired. Advanced controls include indicators for a nondisclosure agreement, a nonpoaching agreement, a nonsolicitation agreement, an IP assignment agreement, an arbitration agreement, a retirement plan, employer-sponsored health insurance, paid vacation, sick leave, and life insurance, as well as indicators for how many employers the employee has had in the last five years, the types of confidential information currently possessed by the employee (e.g., trade secrets, client information), as well as indicators for the frequency of moves from the employer to competitors, to the employer from competitors, and between competitors in the industry generally.

Table 3: How Has a Noncompete Affected Your Choice to Stay at or Leave an Employer?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Noncompetes and the Choice to Stay							
	Delayed leaving	Stayed out of fear of lawsuit	Companies would not hire due to noncompete	Stayed b/c felt obligated not to compete	Paid more money to stay	Negotiated scope or waiver	
<Bachelor’s	11.4%	5.0%	5.7%	5.6%	4.0%	1.2%	
Bachelor’s	12.0%	5.9%	6.5%	9.0%	7.4%	3.9%	
>Bachelor’s	11.9%	7.7%	6.8%	8.6%	8.7%	4.3%	
Overall	11.6%	5.7%	6.1%	7.0%	5.7%	2.5%	

Panel B: Noncompetes and the Choice to Leave							
	Left the industry	Left, waited for expiration, then joined competitor	Joined competitor who could protect from lawsuit	Tried to prevent prior employer from learning	Moved locations	Went to school	Never been a factor in choice to leave
<Bachelor’s	12.0%	5.2%	2.1%	2.2%	1.9%	1.9%	70.3%
Bachelor’s	12.6%	7.3%	3.2%	2.3%	2.4%	2.3%	66.9%
>Bachelor’s	11.3%	10.2%	5.1%	3.0%	4.4%	4.0%	59.3%
Overall	12.0%	6.6%	2.9%	2.4%	2.4%	2.3%	67.5%

Note: Numbers are percentages conditional on education category. Responses are only for those who report having signed a noncompete at some point in their lives (not including those who were imputed to have signed).

Table 4: Process of Receiving an Offer

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	*Panel A Dependent Variable: **1** (Recruited in the last year by)*								
	Competitor			*Noncompetitor*			Δ		
Noncompete	0.054***	0.002	-0.002	0.113***	0.055***	0.051**	-0.059***	-0.053***	-0.053**
	(0.016)	(0.018)	(0.019)	(0.018)	(0.019)	(0.020)	(0.016)	(0.020)	(0.020)
Noncompete×Enforceability			-0.013*			-0.014			0.001
			(0.007)			(0.008)			(0.008)
R-Squared	0.145	0.209	0.209	0.129	0.184	0.184	0.094	0.109	0.109
	Panel B Dependent Variable: Search Effort (0-10) last year towards								
	Competitor			*Noncompetitor*			Δ		
Noncompete	-0.100	-0.104	-0.117	0.422***	0.394***	0.405***	-0.522***	-0.498***	-0.522***
	(0.121)	(0.125)	(0.129)	(0.123)	(0.128)	(0.132)	(0.119)	(0.123)	(0.125)
Noncompete×Enforceability			-0.048			0.041			-0.089
			(0.063)			(0.054)			(0.058)
R-Squared	0.105	0.183	0.183	0.141	0.188	0.188	0.126	0.149	0.149
	*Panel C Dependent Variable: **1** (Offer in the last year from)*								
	Competitor			*Noncompetitor*			Δ		
Noncompete	0.043***	0.019	0.018	0.046**	0.014	0.017	-0.003	0.005	0.001
	(0.013)	(0.015)	(0.016)	(0.019)	(0.019)	(0.019)	(0.018)	(0.018)	(0.017)
Noncompete×Enforceability			-0.002			0.014**			-0.015**
			(0.005)			(0.006)			(0.007)
R-Squared	0.113	0.156	0.156	0.112	0.143	0.143	0.078	0.094	0.095
Observations	11,462	11,010	11,010	11,462	11,010	11,010	11,462	11,010	11,010
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by state. Δ refers to the difference between competitors and noncompetitors for each dependent variable. Basic and advanced controls are described in the text and in Table 2.

Table 5: Turning Down Job Offers

	(1)	(2)	(3)
Sample	*All*	*States That Do Not Enforce Noncompetes*	*States That Enforce Noncompetes*
Panel A: Was your noncompete a factor in your choice to turn down your offer from a competitor?			
Yes	41.5%	37.5%	42.3%
Panel B: If you received an offer from a competitor, would your noncompete be a factor in your choice to accept it?			
Yes	47.6%	46.6%	47.8%
Panel C: How important is your noncompete in determining if you leave for a competitor?			
Not at all Important	9.3%	5.8%	9.9%
Very Unimportant	5.0%	5.0%	5.0%
Somewhat Unimportant	6.1%	5.1%	6.3%
Neither Important nor Unimportant	25.3%	26.1%	25.2%
Somewhat Important	21.7%	23.4%	21.4%
Very Important	16.5%	16.2%	16.5%
Extremely Important	16.0%	18.5%	15.6%
Somewhat or Very or Extremely Important	54.2%	58.1%	53.5%

Note: Panel A includes individuals who report signing a noncompete and reported turning down an offer from a competitor while employed (N=604). Panel B analyzes a hypothetical question and includes all individuals who have either reported signing a noncompete or have been imputed to have signed a noncompete (N=2,261). Panel C, like Panel B, includes all individuals who either reported signing or were imputed to sign (N=2,261) . Nonenforcing states include California, North Dakota, and Oklahoma, per Beck (2014). Enforcing states are all others. The difference in Panel A between enforcing states and nonenforcing states is not statistically significant.

Table 6: Why Do Some Turn Down Offers Because of the Noncompete But Not Others?

Dependent Variable: ***1*** *(Noncompete a factor in turning down actual/hypothetical offer from competitor)*

	(1)	(2)	(3)	(4)	(5)	(6)
Condition of offer:	*Employer is unaware of offer from competitor*		*Employer is aware of offer from competitor*		*Hypothetical offer from competitor*	
Reminded of Noncompete				0.407***		
				(0.070)		
1(Aware Employer Sued in Past)	0.158*	0.160**	0.185**	0.132*	0.081*	0.081*
	(0.080)	(0.079)	(0.080)	(0.070)	(0.045)	(0.045)
Subjective P(Lawsuit)	0.293*	0.288**	0.248**	0.170**	0.236***	0.233***
	(0.146)	(0.142)	(0.120)	(0.075)	(0.071)	(0.071)
Subjective P(Enforced)	0.321**	0.324**	0.283*	0.090	0.353***	0.357***
	(0.131)	(0.129)	(0.145)	(0.135)	(0.092)	(0.093)
Actual Enforceability		0.006	-0.067***	-0.060***		0.008
		(0.015)	(0.018)	(0.018)		(0.010)
Observations	382	382	219	219	2261	2261
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes

Results are from a linear probability model. Standard errors are clustered at the state level. Basic and advanced controls are described in the text and in Table 2, except here we include separate (as opposed to interacted) occupation and industry (2-digit SOC, 2-digit NAICS) fixed effects, given the smaller sample size. The enforceability variable is a continuous measure of noncompete enforceability from 2009, as described in Starr (2019). Columns (5) and (6) include those imputed to have signed a noncompete, whereas columns (1) and (2) include only affirmative noncompete signers.

Appendix A. Additional Tables

Table A1: Comparison to Other Studies

Study	Population	% US Labor Force	Data Source	Sample Size	Response Rate	Noncompete Incidence
Schwab and Thomas (2006)	Executives	0.18%	The Corporate Library, SEC EDGAR	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. (2015)	Executives	0.18%	SEC EDGAR	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. (2020)	Physicians	0.47	Survey of Professional Beauty Association	1,967 Individuals	69.8%	45.1%
Johnson and Lipsitz (2020)	Hair Stylists	0.25%	Survey of Professional Beauty Association	218 Hair Salons	4%-31%	30.0%
This study	US Labor Force	100%	Qualtrics (with 7 online survey panel providers)	11,505 Individuals	2%-23%	18.1%

Note: % of US Labor Force is based on the 2014 BLS Occupational Employment Survey: https://www.bls.gov/news.release/archives/ocwage_03252015.pdf

Table A2. Self-Described Occupation Title, Duties, and Industry for 15 Randomly Selected Respondents

	Occupation Title	Self-Described Occupation 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

Table A3: Distribution Comparison Between Weighted and Unweighted Noncompete Survey Project Data and 2014 American Community Survey

Variable	NSP Data		ACS	NSP-ACS Difference	
	Unweighted	Weighted		Unweighted	Weighted
1(< Bachelor's Degree)	0.48	0.69	0.70	-0.22***	-0.01
1(Bachelor's Degree)	0.37	0.21	0.20	0.16***	0.01
1(> Bachelor's Degree)	0.16	0.10	0.10	0.06***	0.00
1(Work ≥ 40 Hours per Week)	0.70	0.71	0.72	-0.02***	-0.01
1(Male)	0.47	0.53	0.53	-0.07***	0.00
Age	41.98	40.33	40.55	1.43***	-0.22

Note: This table shows the distribution of demographic characteristics between the Noncompete Survey Project Data, both weighted and unweighted, and the 2014 American Community Survey. The weighted data use raking weights, as described in the text and Prescott et al. (2016). In the difference columns, the standard errors are clustered at the state level.

Table A4: Redirection and Retention, Separating Maybes Explicitly

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Willingness to Join a Competitor and Tenure						
Dependent Variable	***1*** *(Will Never Move to a Competitor)*		*Ln (Required Competitor Wage Premium)*		*Tenure*	
Noncompete: Yes	0.047***	0.053***	0.193***	0.147***	0.528**	0.622**
	(0.015)	(0.017)	(0.048)	(0.047)	(0.252)	(0.271)
Noncompete: Maybe	0.019**	0.016	-0.031	-0.013	0.472***	0.455**
	(0.009)	(0.010)	(0.041)	(0.039)	(0.154)	(0.183)
Observations	11,462	11,010	10,650	10,241	11,462	11,010
R-Squared	0.102	0.119	0.0925	0.110	0.432	0.492
Panel B: Subjective Probability of Leaving in the Next Year						
Dependent Variable	*P(Leave Competitor)*		*P(Leave Noncompetitor)*		*P(Leave Competitor)-P(Leave Noncompetitor)*	
Noncompete: Yes	-3.025***	-4.827***	0.334	-1.316	-3.359**	-3.512**
	(0.690)	(0.802)	(1.196)	(1.024)	(1.410)	(1.382)
Noncompete: Maybe	-0.125	0.497	-0.997	-0.830	0.873	1.327
	(0.779)	(0.734)	(1.098)	(1.078)	(1.081)	(1.056)
Observations	11,462	11,010	11,462	11,010	11,462	11,010
R-Squared	0.109	0.172	0.161	0.206	0.134	0.152
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the state level. Basic and advanced controls are described in the text and in Table 2.

Table A5: Redirection and Retention by Noncompete Enforceability, Less Saturated

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Willingness to Join a Competitor and Tenure									
Dependent Variable	*1 (Will Never Move to a Competitor)*			*Ln (Required Competitor Wage Premium)*			*Tenure*		
Enforceability	0.003	-0.005		-0.010	-0.023**		0.140**	-0.058	
	(0.004)	(0.004)		(0.010)	(0.010)		(0.055)	(0.158)	
Noncompete	0.037**	0.051***	0.051***	0.165***	0.177***	0.180***	0.475	0.478**	0.474**
	(0.016)	(0.017)	(0.017)	(0.038)	(0.041)	(0.041)	(0.306)	(0.221)	(0.206)
Noncompete×Enforceability	0.010	0.012	0.011	-0.049***	-0.053***	-0.054***	-0.212	-0.132	-0.100
	(0.007)	(0.009)	(0.009)	(0.015)	(0.017)	(0.017)	(0.176)	(0.128)	(0.123)
Observations	11,505	11,462	11,462	10,689	10,650	10,650	11,505	11,462	11,462
R-Squared	0.003	0.091	0.104	0.007	0.081	0.094	0.002	0.359	0.431
Panel B: Subjective Probability of Leaving in the Next Year									
Dependent Variable	*P(Leave Competitor)*			*P(Leave Noncompetitor)*			*P(Leave Competitor)-P(Leave Noncompetitor)*		
Enforceability	-0.942***	-0.647***		-0.590***	-0.827***		-0.352*	0.180	
	(0.163)	(0.215)		(0.181)	(0.231)		(0.204)	(0.278)	
Noncompete	-0.768	-2.396***	-2.540***	1.342	1.170	1.091	-2.110	-3.566**	-3.631**
	(0.864)	(0.848)	(0.834)	(1.506)	(1.362)	(1.378)	(1.431)	(1.394)	(1.390)
Noncompete×Enforceability	0.163	0.203	0.184	0.543	0.603	0.582	-0.380	-0.400	-0.398
	(0.468)	(0.458)	(0.449)	(0.834)	(0.879)	(0.850)	(0.653)	(0.697)	(0.681)
Observations	11,505	11,462	11,462	11,505	11,462	11,462	11,505	11,462	11,462
R-Squared	0.004	0.100	0.109	0.001	0.152	0.161	0.001	0.124	0.134
Basic Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Advanced Controls	No	No	No	No	No	No	No	No	No

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the state level in the regressions with the measure of noncompete enforceability coming from Starr (2019). Basic controls are described in the text and in Table 2. The third column includes state fixed effects, which subsumes the main effect of enforceability.

Table A6: Redirection and Retention Split by Enforcing and Non-Enforcing States

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Willingness to Join a Competitor and Tenure by Enforcing and Non-Enforcing States						
Dependent Variable	***1*** *(Will Never Move to a Competitor)*		*Ln (Required Competitor Wage Premium)*		*Tenure*	
Noncompete	0.056	0.060***	0.356**	0.118**	1.792**	0.641**
	(0.030)	(0.019)	(0.056)	(0.049)	(0.409)	(0.262)
Sample: Enforcement Status	Not Enforce	Enforce	Not Enforce	Enforce	Not Enforce	Enforce
Observations	1,085	9,925	1,023	9,218	1,085	9,925
R-Squared	0.280	0.135	0.469	0.113	0.626	0.496
Panel B: Subjective Probability of Leaving in the Next Year by Enforcing and Non-Enforcing States						
Dependent Variable	*P(Leave Competitor)*		*P(Leave Noncompetitor)*		*P(Leave Competitor)-P(Leave Noncompetitor)*	
Noncompete	-6.972*	-4.080***	-1.802	-0.549	-5.170	-3.531**
	(2.159)	(1.058)	(1.876)	(1.320)	(2.717)	(1.499)
Sample: Enforcement Status	Not Enforce	Enforce	Not Enforce	Enforce	Not Enforce	Enforce
Observations	1,085	9,925	1,085	9,925	1,085	9,925
R-Squared	0.368	0.178	0.405	0.218	0.342	0.166
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Nonenforcing states include California, North Dakota, and Oklahoma, per Beck (2014). Enforcing states are all others. Standard errors are clustered at the state level in enforcing states. Due to low number of states, robust standard errors are used in specifications limited to nonenforcing states. Basic and advanced controls are described in the text and in Table 2.

Table A7: Redirection and Retention Heterogeneous Effects, Garmaise Enforceability Measure

	(1)		(2)		(3)	
Panel A: Willingness to Join a Competitor and Tenure						
Dependent Variable	***1*** *(Will Never Move to a Competitor)*		*Ln (Required Competitor Wage Premium)*		*Tenure*	
Noncompete	0.050***	0.059***	0.189***	0.153***	0.495**	0.731***
	(0.016)	(0.016)	(0.041)	(0.042)	(0.211)	(0.230)
Noncompete×Enforceability	0.021*	0.021*	-0.062**	-0.063**	-0.035	-0.098
	(0.012)	(0.011)	(0.023)	(0.025)	(0.185)	(0.155)
Observations	11,462	11,010	10,650	10,241	11,462	11,010
R-Squared	0.105	0.122	0.0943	0.112	0.431	0.492
Panel B: Subjective Probability of Leaving in the Next Year						
Dependent Variable	*P(Leave Competitor)*		*P(Leave Noncompetitor)*		*P(Leave Competitor)-P(Leave Noncompetitor)*	
Noncompete	-2.576***	-4.414***	0.960	-0.513	-3.537**	-3.901***
	(0.836)	(1.002)	(1.368)	(1.212)	(1.348)	(1.386)
Noncompete×Enforceability	0.119	0.133	0.124	0.202	-0.005	-0.068
	(0.559)	(0.505)	(1.319)	(1.054)	(1.073)	(0.967)
Observations	11,462	11,010	11,462	11,010	11,462	11,010
R-Squared	0.109	0.172	0.161	0.206	0.134	0.152
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the state level in the regressions with the measure of noncompete enforceability coming from Garmaise (2009), normalized to have a mean of zero and a standard deviation of one for comparability. Note the main effect of enforceability is subsumed by state fixed effects. Basic and advanced controls are described in the text and in Table 2.

Table A8: Receipt of Offers While Employed

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	*Dependent Variable: **1**(Offer while employed from)*								
	Competitor			*Noncompetitor*			Δ		
Noncompete	0.055***	-0.005	-0.008	0.096***	0.039*	0.036*	-0.040*	-0.043*	-0.044*
	(0.016)	(0.018)	(0.018)	(0.020)	(0.021)	(0.021)	(0.021)	(0.022)	(0.023)
Noncompete×Enforceability			-0.014*			-0.010			-0.003
			(0.007)			(0.011)			(0.009)
R-Squared	0.129	0.191	0.191	0.104	0.142	0.142	0.093	0.111	0.111
Observations	11,462	11,010	11,010	11,462	11,010	11,010	11,462	11,010	11,010
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the state level. Δ refers to the difference between competitors and noncompetitors for the relevant dependent variable. Basic and advanced controls are described in the text and in Table 2.

Table A9: Process of Receiving an Offer Heterogeneous Effects By Garmaise Measure of Noncompete Enforceability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Panel A Dependent Variable: 1 (Recruited in the last year by)						*Panel B Dependent Variable: Search Effort (0-10) last year towards*					
	Competitor		*Noncompetitor*		Δ		*Competitor*		*Noncompetitor*		Δ	
Noncompete	0.053***	0.000	0.112***	0.053***	-0.059***	-0.053***	-0.104	-0.109	0.424***	0.397***	-0.528***	-0.506***
	(0.016)	(0.019)	(0.018)	(0.020)	(0.016)	(0.019)	(0.120)	(0.124)	(0.121)	(0.128)	(0.113)	(0.122)
Noncompete×Enforceability	-0.015	-0.017**	-0.020**	-0.023**	0.005	0.005	-0.059	-0.059	0.041	0.028	-0.100	-0.087
	(0.009)	(0.009)	(0.010)	(0.010)	(0.009)	(0.009)	(0.080)	(0.076)	(0.081)	(0.070)	(0.073)	(0.071)
R-Squared	0.146	0.209	0.129	0.184	0.094	0.109	0.105	0.183	0.141	0.188	0.126	0.149
	Panel C Dependent Variable: 1 (Offer in the last year from)						*Panel D Dependent Variable: 1 (Offer while employed from)*					
	Competitor		*Noncompetitor*		Δ		*Competitor*		*Noncompetitor*		Δ	
Noncompete	0.042***	0.018	0.047**	0.014	-0.004	0.004	0.054***	-0.006	0.094***	0.037*	-0.040*	-0.043*
	(0.013)	(0.015)	(0.018)	(0.019)	(0.016)	(0.017)	(0.016)	(0.018)	(0.019)	(0.020)	(0.020)	(0.022)
Noncompete×Enforceability	-0.004	-0.004	0.008	0.005	-0.012	-0.009	-0.013	-0.015*	-0.021	-0.023	0.007	0.008
	(0.007)	(0.007)	(0.010)	(0.009)	(0.011)	(0.010)	(0.009)	(0.009)	(0.016)	(0.014)	(0.015)	(0.012)
R-Squared	0.113	0.156	0.112	0.143	0.078	0.094	0.129	0.191	0.105	0.142	0.093	0.112
Observations	11,462	11,010	11,462	11,010	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	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Columns (1)-(6) refer to Panels A and C, while columns (7)-(12) refer to Panels B and D. Enforceability measure from Garmaise (2009), normalized to have a mean of zero and a standard deviation of one for comparability. Standard errors are clustered at the state level. Δ refers to the difference between competitors and noncompetitors for the relevant dependent variable. Basic and advanced controls are described in the text and in Table 2.

Table A10: Process of Receiving an Offer by Noncompete Enforceability, Less Saturated

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	*Panel A Dependent Variable: **1** (Recruited in the last year by)*						*Panel B Dependent Variable: Search Effort (0-10) last year towards*					
	Competitor		*Noncompetitor*		Δ		*Competitor*		*Noncompetitor*		Δ	
Enforceability	-0.015***		-0.017***		0.002		-0.075**		-0.065**		-0.009	
	(0.003)		(0.004)		(0.005)		(0.028)		(0.025)		(0.029)	
Noncompete	0.100***	0.051***	0.150***	0.111***	-0.049***	-0.059***	0.073	-0.112	0.480***	0.433***	-0.407***	-0.545***
	(0.016)	(0.016)	(0.020)	(0.019)	(0.016)	(0.016)	(0.122)	(0.124)	(0.132)	(0.125)	(0.127)	(0.116)
Noncompete*Enforceability	-0.014*	-0.011*	-0.009	-0.010	-0.005	-0.002	-0.053	-0.047	0.049	0.049	-0.102*	-0.096*
	(0.007)	(0.007)	(0.008)	(0.009)	(0.007)	(0.008)	(0.063)	(0.066)	(0.057)	(0.062)	(0.052)	(0.055)
Constant	0.183***	-0.177	0.220***	-0.034	-0.036***	-0.143	2.654***	1.485	3.243***	4.432	-0.590***	-2.947
	(0.007)	(0.442)	(0.009)	(0.403)	(0.009)	(0.533)	(0.051)	(3.017)	(0.053)	(4.253)	(0.058)	(3.827)
R-Squared	0.0146	0.146	0.0225	0.129	0.00193	0.0937	0.00199	0.105	0.00371	0.141	0.00302	0.126
	*Panel C Dependent Variable: **1** (Offer in the last year from)*						*Panel D Dependent Variable: **1** (Offer while employed from)*					
	Competitor		*Noncompetitor*		Δ		*Competitor*		*Noncompetitor*		Δ	
Enforceability	-0.003		-0.008***		0.005**		-0.004		-0.002		-0.002	
	(0.003)		(0.002)		(0.002)		(0.003)		(0.004)		(0.004)	
Noncompete	0.056***	0.042***	0.065***	0.050***	-0.008	-0.008	0.087***	0.052***	0.127***	0.094***	-0.039*	-0.042*
	(0.013)	(0.013)	(0.017)	(0.017)	(0.015)	(0.016)	(0.015)	(0.016)	(0.021)	(0.021)	(0.020)	(0.021)
Noncompete*Enforceability	-0.005	-0.002	0.012**	0.016**	-0.017**	-0.018**	-0.010	-0.012*	-0.004	-0.006	-0.006	-0.007
	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.010)	(0.011)	(0.009)	(0.009)
Constant	0.091***	0.605	0.102***	0.909***	-0.011**	-0.303	0.190***	-0.010	0.231***	0.525	-0.040***	-0.534
	(0.005)	(0.376)	(0.005)	(0.310)	(0.005)	(0.317)	(0.007)	(0.421)	(0.009)	(0.428)	(0.009)	(0.569)
R-Squared	0.00585	0.113	0.00681	0.113	0.00118	0.0785	0.00810	0.130	0.0129	0.104	0.00127	0.0930
Observations	11,505	11,462	11,505	11,462	11,505	11,462	11,505	11,462	11,505	11,462	11,505	11,462
Basic Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Advanced Controls	No	No	No	No	No	No	No	No	No	No	No	No

Notes: *** p<0.01, ** p<0.05, * p<0.1. Columns (1)-(6) refer to Panels A and C, while columns (7)-(12) refer to Panels B and C. Standard errors are clustered at the state level. Δ refers to the difference between competitors and noncompetitors for the relevant dependent variable. Basic controls are described in the text and in Table 2.

Appendix B. Data Appendix[26]

This paper'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 the respondent's current job and throughout the respondent's career. In this appendix, we describe the details of the survey's origin, design, sampling frame, data cleaning, and data processing that are critical to understanding the empirical work in this paper. We draw heavily on the technical paper that describes these issues in meticulous detail (Prescott et al. 2016).

A.1 Sampling Frame and Data Collection Methodology

The sampling frame for this study are U.S. labor force participants aged 18–75 who are working in the private sector (for-profit or nonprofit), working for a public health system,[27] or unemployed and looking for work. We excluded individuals who reported being self-employed, government employees, and non-US citizens, and those who are out of the labor force. To collect the data, we considered numerous possible survey platforms and collection methods, including using RAND's American Life Panel (ALP), 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 much more sense to design and draft the survey ourselves so that we would be able to ask all of the potentially

[26] This description of the 2014 Noncompete Survey Project is identical in substance to the data appendix in Starr et al. (forthcoming). We include it here for convenience.

[27] We initially considered focusing only on the private sector, but we eventually recognized that public healthcare systems (e.g., those associated with a public university) also use noncompetes extensively.

relevant questions. We ultimately settled on using Qualtrics, a reputable online survey company with access to more than 10 million *verified* panel respondents.[28]

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 earnings of at least $50,000 annually from their current, highest paying job; and 30% over the age of 55. We chose these numbers either to align the sample with the corresponding sample moments for labor force participants in the 2012 ACS or to oversample certain populations of interest.

Respondents who completed the survey were compensated differently depending on the panel provider: some were paid $1.50 and entered into prize sweepstakes, others were given tokens or points in online games that they were playing. The median survey took the survey taker approximately 28 minutes to complete. Due to the length of the survey, we used three "attention filters" spaced evenly throughout the survey to ensure that respondents were paying attention to the questions. Before we describe the cleaning process of the survey, we briefly describe the costs and benefits of online surveys. The information contained in the following sections can be found in Tables 1–18 in Prescott et al. (2016).

A.2 Costs and Benefits of Online Surveys

[28] 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 survey companies as potential respondents to see how they vetted individuals who signed up to respond to surveys. A typical experience involves filling out an intake form, providing wide-ranging information, and 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.

Online surveys come with a variety of benefits. Relative to random-digit-dialing or in-person surveys, the cost per respondent is orders of magnitude lower and the data-collection time is orders of magnitude faster. The interactive survey interface also allows the survey designer to write complicated, nested questions that are easy for respondents to answer on the online platform. Online surveys also allow individuals to respond at their leisure and via their preferred method (e.g., computer, phone, tablet, etc.) and at their preferred place (e.g., at work, home, 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.[29]

These benefits come at a potentially high cost: a sample of online survey takers may not necessarily be representative of the population of interest to researchers. 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.[30] 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 the sample resulting from a random-digit-dial survey is still a random sample of the population. We address each of these selection concerns in detail in Prescott et al. (2016) and discuss the second concern in particular in the section on sample selection.

A.3 Survey Cleaning

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

[30] For example, random-digit dial surveys miss those without a phone, all those with a phone may not receive a phone call, and those who do get the call but decline the survey.

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 issue, we retained only the first attempt at the survey from a given IP address and only if that attempt ended 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 on 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 whom took the survey with a different panel partner. 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.[31]

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 questions. In analyzing these answers, we discovered that some individuals were intentionally noncompliant (e.g., they wrote curse words instead of their job title), and some who simply made idiosyncratic errors (e.g., noting that their entire employer was smaller than their establishment—i.e., their particular office or factory). We dropped respondents entirely if they were deemed intentionally noncompliant because their singular responses indicated that they did not take the survey seriously. This step left us with 11,529 responses.[32]

[31] See Tables 3–5 in Prescott et al. (2016) for more details.

[32] See p.412–14 in Prescott et al. (2016) for more details.

In the last round of cleaning, we began with those who either had clean surveys or 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 (Table 6 in Prescott et al. (2016)). The most common flag was for 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 paper (although our findings are not very sensitive to this choice), was to impute or correct these offending values. Specifically, we "repaired" entries that are marred by idiosyncratic inconsistency by replacing the less reliable offending value with the value closest to the originally submitted value that would not be inconsistent with the respondent's other answers. When an answer was clearly unreasonable or missing, and there was no workable single imputation procedure, we applied multiple imputation methods to calculate a substitute value for the original missing or unreasonable survey entry.

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

2-digit NAICS and SOC codes within the survey and also report their job title, job duties, and what their employer produced or sold.[33] To verify the 2-digit NAICS and SOC codes—which are crucial for both weighting and fixed effects in any subsequent analysis—we had four sets of RAs independently code the 11,529 responses by taking job titles, job duties, and employer descriptions and matching them with the appropriate 2-digit NAICS and SOC codes.[34] As part of this process, we found that 24 individuals were self-employed, worked for the government, or were retired, thus reducing our total number of respondents to 11,505.

The above process produced our final sample. Next we examine sample selection concerns, weighting, and how we correct for missing data using multiple imputation.

A.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 the survey, and (4) not everybody who receives the survey takes it. We describe these issues in greater detail in Section II.E in Prescott et al. (2016). All survey methods must confront issues (1), (3) and (4)—the only unique selection concern is (2). The key question is why individuals sign up for online surveys in particular and whether that reason is associated with the use of noncompetes.[35] 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 of Prescott et al. (2016). The most common reasons individuals report for signing up to take online surveys is that they like the rewards (59%) and sharing their opinion (58%). Only 40% indicated they wanted money, and only 23% claimed they needed money. Taking these responses

[33] Table A4 reports 15 randomly selected job titles, occupation duties, and industry.

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

[35] 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 fulltime employment.

seriously, the key selection question is, conditional on observables, whether individuals who like to share their opinion or like the rewards are less likely to be in jobs that require noncompetes. We believe it is certainly plausible that there is no such relationship.

A.5 Weighting and Imputation

In this section, we describe our approach to weighting and imputing data that is either actually missing or was marked as missing during the cleaning process. The fact that weights need to be included in the imputation process in order to impute unbiased population values complicates the process. We proceeded by first weighting the nonmissing data, then imputing the missing variables (including the weights), and finally reweighting given the imputed values so that the resulting dataset is nationally representative.

We considered several weighting schemes,[36] including post-stratification, iterative proportional fitting (also called raking), and propensity score weighting. Details on these methods can be found in Kalton and Flores-Cervantes (2003). For each method, we considered a variety of potential weighting variables, and we examined the ability of each scheme to match variable distributions in the 2014 ACS (see Table 17 in Prescott et al. (2016).). Iterative proportional fitting, or raking, performs best in matching the distribution of key variables in the ACS.

Using raking weights, we sought to impute multiple variables (see Table 18 in Prescott et al. (2016) for details). Some of these variables have missing values because of the cleaning process we describe above; others have missing values because the question generating the variable was added to the survey while the survey was in the field. In addition, as we describe in the paper, we also impute whether the "maybes" have currently or ever signed a noncompete. Because we impute multiple variables, we use Stata's chained multiple imputation command, which imputes all variables in one

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

step. As suggested in Sterne et al. (2009), we incorporate all the variables we use in our empirical analyses into the imputation model, which would otherwise result in attenuated estimates. A single imputation will generate unbiased coefficients, but the standard errors will be too small because the predicted value will not capture estimate uncertainty (King et al. 2001). To obtain correct standard errors, Graham et al. (2007) suggest carrying out at least 20 imputations when the proportion missing is 30%. We add another 5 for good measure.

The exact mechanics of a given imputation step are as follows: First, we fit a regression model using our nonmissing data. Second, we simulate new coefficients based on the posterior distribution of the coefficients and standard errors (this step is what gives us variation across the 25 datasets). Third, we apply these coefficients to the observed covariates of the missing observations and generate predicted values. For continuous variables, we use predictive mean matching in the third step in which we take the average of the 15 nearest neighbors from the predicted value. For binary variables, we use a logit model to create the predicted value. We repeat this process 25 times for all missing values to be imputed. Once we have the 25 imputed datasets, we re-weight within each dataset using the raking procedure we describe above, so that each individual dataset is nationally representative.

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