

# Subjective Beliefs about Contract Enforceability

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## Abstract

This paper assesses the content, role, and adaptability of subjective beliefs about contract enforceability in the context of postemployment covenants not to compete. We show that—while noncompete enforceability varies widely across states—employees of all stripes tend to believe that their noncompetes are enforceable, even when they are not. We provide evidence in support of both supply- and demand-side stories that explain employees’ persistently inaccurate beliefs. Moreover, we show that mistaken beliefs are not innocuous. Rather, believing that unenforceable noncompetes are enforceable causes employees to forgo better job options and to perceive that their employer is more likely to take legal action against them if they choose to compete. However, despite mobility-reducing effects ex post, mistakenly believing a noncompete is enforceable does not appear to cause someone to be more likely to negotiate over such provisions ex ante. Finally, we use an information experiment to simulate an educational campaign that informs employees about the enforceability (or lack thereof) of their noncompete. We find that this information matters a good deal; however, information does not appear to entirely eliminate an unenforceable noncompete as a factor in the decision whether to take a new job. We discuss the implications of our experimental results for the policy debate regarding the enforceability of noncompetes.

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## 1. Introduction

Behavior is driven by beliefs rather than facts. While this is a commonplace in fields like criminal law (e.g., “deterrence is a perceptual phenomenon” (Nagin 2013, p.215), it is no less true in contracting behavior and performance, where scholars and practitioners alike assume that choices are made in the shadow of *anticipated* court enforcement. However, we often ignore or assume away a distinct role for beliefs—in particular, mistaken beliefs—in economic settings (such as contracting). The stakes are usually high and agents have many opportunities to learn: incentives and low-cost access to information (usually through interaction with others) suggest a natural tendency for beliefs to quickly approximate facts. Conditions may exist, however, where mistaken beliefs remain stable, either because baseline access to facts is limited or because parties who benefit from systematic mistakes invest in maintaining them (Gabaix and Laibson 2006). Where persistent mistakes relate to the content of policies or law, and where those mistaken beliefs are socially costly, interventions designed to disrupt this equilibrium may be able to change behavior and potentially improve welfare.

In this paper, we consider the role of beliefs in the use of and behavior associated with covenants not to compete (noncompetes). Noncompetes are employment contract provisions that prohibit employees from leaving their employer to join or start a competitor, typically within particular geographic and time boundaries (Balasubramanian et al. 2020, Starr 2019). Our work is motivated by two recent findings that point to the possible influence of mistaken beliefs in this area of economic life. First, employers heavily use noncompetes in states that explicitly will not enforce them (Starr et al. 2021a; Colvin and Shierholz 2019). Second, noncompetes influence behavior, including mobility, even in states where such provisions are unenforceable (Starr et al. 2021b). While there are several possibilities for why employers might use and employees might comply with noncompetes even when employees *know* they cannot be enforced (e.g., reputational harm, disutility from breaking a promise), one explanation for these results is that employees have mistaken beliefs about noncompete enforceability and that these beliefs matter to their choices.<sup>1</sup> The possibility that employees are systematically uninformed or perversely misinformed has important implications for the regulation of noncompetes and the interpretation of existing noncompete research.

Existing reform efforts and research almost invariably (if implicitly) assume that employees operate rationally or at least with awareness of existing law when navigating noncompete-related choices. Indeed, one common starting point has been that noncompetes and their enforceability must be beneficial to employees and employers (Rubin and Shedd 1981, Posner et al. 2004) because parties

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<sup>1</sup> Catherine Fiske (2002) highlights this possibility when she writes: “In California, covenants not to compete have been unenforceable against employees since 1872. Employers have nevertheless sought to restrict their employees from working for competitors ... presumably counting on the *in terrorem* value of the contract when the employee does not know that the contract is unenforceable.”

would not otherwise agree to such provisions. And yet the potential consequences of assuming that employees understand the legal effect of noncompetes are significant. Research in the last decade has witnessed a near explosion in the empirical examination of frictions that limit the *within-industry* mobility of human capital, including covenants not to compete (Marx et al. 2009, Conti 2014, Samila and Sorenson 2011, Starr 2018), the inevitable disclosure doctrine (Flammer and Kacpercyk 2017, Hsu et al. 2017), and trade secret protections (Png 2016). The reason is clear: within-industry frictions prevent the free flow of knowledge and employees to competitors and thus have important implications for both individual and societal welfare (Treasury 2016).

The traditional reform proposal of those who believe that the welfare implications of noncompete contracting are largely negative is to simply prohibit court enforcement of such provisions.<sup>2</sup> But if individuals are systematically mistaken about noncompete enforceability in states that do not enforce such provisions, the *in terrorem* effects of noncompetes are likely to remain significant even following a noncompete ban (Starr et al. 2021b). Most research on the consequences of noncompetes ignores this possibility. The vast majority of scholarship examines the effects of state-level policy changes, effectively assuming that applicants, employees, and employers are informed about such policies (Garmaise 2009). However, if employees decline job offers based on their *beliefs* about enforceability as opposed to actual enforceability (Starr et al. 2021b), we must question the accuracy of the perfect information assumption and the implications of models that incorporate it.

In this study, we use detailed, nationally representative survey data and an information experiment involving 11,505 labor force participants to examine what employees believe about the enforceability of noncompetes—which varies markedly across states (Bishara 2011)—and the causal effects of believing that a noncompete will or will not be enforced on prospective mobility and entrepreneurial decisions.<sup>3</sup> In particular, we examine a set of questions related to mistaken beliefs about law, the effects of these beliefs, and how such beliefs respond to new information. We begin by studying what employees *believe* about noncompete enforceability, whether those beliefs are accurate, how patterns in accurate beliefs differ across employee characteristics, and why employees ap-

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<sup>2</sup> Somewhat ironically, proponents of banning noncompete *enforcement* often rely on the lack of sophistication, lack of understanding, or lack of bargaining power on the part of the employee. At least with respect to uninformed applicants and employees, it seems optimistic to believe that these individuals will become aware of and be able to take advantage of subtle changes in state statutory or case law when they are uninformed about the content or implications of the noncompete in their employment contract.

<sup>3</sup> We use data from the 2014 Noncompete Survey Project, the first nationally representative survey of noncompetes (Prescott et al. 2016). In previous work, we have used this data to describe the incidence of noncompetes across the U.S. labor force (Starr et al. 2021a), how noncompetes affect the process of mobility (Starr et al. 2021b), and how noncompetes create externalities even among those not bound by such agreements (Starr, Frake, and Agarwal 2018). For our analysis here, we exploit two unique features of the survey. First, the survey contains a series of questions asking what respondents know or believe about the *enforceability* of noncompetes. Second, within the survey itself, we deployed an information experiment in which we provided individuals with accurate information on the actual enforceability of noncompetes in their state. We describe the information experiment in more detail below.

pear to remain persistently misinformed. Second, we explore whether informing individuals about *actual* noncompete enforceability under state law influences their beliefs about enforceability, and we identify the causal effect of their beliefs about enforceability on prospective mobility plans, entrepreneurship intentions, and interest in negotiating their noncompete. The answers to these questions have broad implications about the accuracy, formation, and manipulation of beliefs about law beyond the national debate over noncompetes (Treasury 2016).

We document that employees are largely uninformed regarding the enforceability of noncompetes. Approximately 70% of those who are currently bound by noncompetes are unaware that non-compete policy is (to date) determined at the state level. Moreover, we find that subjective beliefs about the probability that a court would enforce a noncompete, conditional on an employer bringing a lawsuit, are uncorrelated with true enforceability in a state. Surprisingly, and in contrast to the prevailing assumption in the literature, we find no evidence that higher-skilled, better-educated employees are more likely to be correct in their beliefs about enforceability (Friedman 1991). Our data offer support for both supply- and demand-side hypotheses that might explain persistently mistaken beliefs. First, individuals who mistakenly believe their noncompete to be enforceable are less likely to search at a competitor, reducing their access to outside information. Second, we find that employees who do interact with competitors are actually *more* likely to believe that noncompetes are enforceable, in part because individuals in states that do not enforce noncompetes are more likely to receive “reminders” of their noncompete from their present employers.

We next establish that it is possible to counter mistaken beliefs simply by providing employees with accurate information about the law, and further that an increase in belief accuracy causes a change in prospective decisions. We find that those in low/non-enforceability states dramatically reduce their expectations that their noncompete will be enforced post-treatment. But, more surprising perhaps, our findings are asymmetric: we find less evidence that employees in high enforceability states with initially mistaken beliefs actually revise their beliefs. Regardless, we find that when changes in beliefs do occur, they seem to matter to employee choices: employees amend their view on whether their noncompete would influence their choice to accept an offer from a competitor or to start a business.<sup>4</sup> Using our treatment as an instrument for an individual’s beliefs about noncompete enforceability, we find that believing a noncompete is enforceable increases the likelihood that a noncompete would be a factor in choosing to start or join a competitor by 60 percentage points relative to an employee who believes their noncompete to be unenforceable.

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<sup>4</sup> Interestingly, again, these effects appear to be concentrated among individuals in states that do not enforce non-competes, suggesting that views about lack of enforceability may be driven less by views about the law than about other beliefs that might make enforcement unlikely.

Finally, given that believing that a noncompete is enforceable causes noncompetes to be more of a factor, we assess the extent to which these beliefs also cause changes in (prospective) negotiation over noncompetes *ex ante*, and the extent to which our results are driven by changes in the likelihood of a lawsuit. We find that, among those presently bound by noncompetes, believing that a noncompete is enforceable does not cause workers to be more likely to negotiate over it. We also show that 20-30% of the effect of beliefs about the on the extent to which noncompetes matter for taking a job is explained by changes in the likelihood of a lawsuit. Nevertheless, we also find that among workers with unenforceable noncompetes who see their noncompetes as unenforceable and who see the likelihood of a lawsuit as very low, 12-25% still view their noncompetes as a factor in the choice to take a job with a competitor—perhaps because of moral or reputational costs from breaking promises.

Our results give rise to at least three implications. First, mistaken beliefs about the law may be persistent, and despite avenues for correction, interested parties, like employers, may work to reinforce ignorance about the law when it benefits them. Beliefs may also be self-reinforcing if employees who mistakenly believe their noncompetes to be enforceable simply opt out of searching for jobs at competitors. Second, mistaken beliefs about enforceability explain at least some of the behavioral response of employees to *unenforceable* noncompetes, while alternative theories, such as concern about reputation or the moral costs of breaking a promise also appear to have merit. Third, given that changes in beliefs and prospective decisions result from supplying people with information about the law, educational campaigns as a form of regulation may offer promise—more effective, perhaps, than statutes or judicial opinions that render noncompetes unenforceable in court.

We organize the remainder of our paper as follows: In Section 2, we review relevant literature—particularly research on ignorance about the law, its consequences, the surprisingly common use of unenforceable contractual provisions, and their behavioral effects—and motivate our particular research questions and hypotheses. In Section 3, we introduce our survey data and our empirical design. Section 4 presents the results of our empirical work. In Section 5, we conclude by discussing the implications of our findings for reform and future research.

## **2. Related Literature and Research Motivation**

Despite the casual and common assumption that people either correctly gauge the content of the law from the get-go or that they will otherwise quickly self-correct whenever it matters (i.e., when they have an incentive to get things right), mistaken beliefs about law appear to be common and have significant ramifications. For example, Kim (1997) finds that job-seekers overwhelmingly overestimate the legal protections afforded by default (at-will) employment contracts. This type of mistaken belief is especially relevant to our work; in contrast to some consumer settings, the employ-

ment relationship is central to many people’s lives and the stakes are high, supplying many reasons to “read the fine print.” Kim’s study confirms that employees enter into employment relationships systematically misinformed as to the extent of their protections from discharge. The research also implicitly undermines an alternative theory that justifies the rule as a reflection of the parties’ preference for internal, non-contractual norms to ensure against welfare-reducing discharges.

Kim (1997) identifies a particular legal doctrine about which a large majority of employees are mistaken, but it is no anomaly: other empirical research confirms that systematic mistakes about the content of law are a more general phenomenon while also making progress at sketching the mechanisms that might explain the direction and character of these mistakes. Darley et al. (2001) survey respondents across four states on four topics of the law, explicitly testing to see whether people are aware of “minority” rules that apply to them in their jurisdictions. They find that respondents in “minority” and “majority” rule states do not differ in their subjective beliefs about the content of law, indicating that mistakes may be the result of reasonable estimates across jurisdictions with different laws. (This theory is consistent with the direction of mistaken beliefs in our data.) Darley et al. also find support for the idea, aligning with Kim (1997), that mistaken views of what the law *is* can be driven by beliefs about what the law *should* be. Rowell (2017) likewise finds that normative beliefs about what the law should be are better predictors of beliefs about the content of law in some areas than the “true” content of law. Rowell also detects varying degrees of informedness across ten states about relevant state laws, from relatively high (the requirement to file an income tax return) to relatively low (a constitutional right to a clean environment). Rowell detects no relationship between the perceived importance of the law and the accuracy of respondents’ beliefs, again consistent with systematic mistakes about weighty employment law issues (Kim 1997).<sup>5</sup>

These studies suggest two conclusions. First, people are broadly misinformed about important areas of the law, including laws that affect them directly. Second, the direction of mistaken beliefs may not be arbitrary but a function of views about what the law should be or by what seems most familiar. One implication of these conclusions is that people’s beliefs, and potentially their behavior, can be shaped, either randomly or with a particular purpose in mind. Stolle and Slaine (1997), Furth-Matzkin and Sommers (2020), and Matzkin (2019), among others, uncover support for the idea that one can strategically influence the beliefs and behavior of others, showing in experimental settings

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<sup>5</sup> Other studies have examined the problem of inadequate information in actors asserting their legal rights and entitlements. For example, in another context, Grisso (1980) empirically measures the capacity of juveniles to understand their Miranda rights and finds they overwhelmingly could not. Grisso contends that the law should adapt to this widespread misapprehension by developing a per se rule excluding juvenile waivers. Other studies, uncovering similarly widespread misapprehension of rights, have argued knowledge of rights can be expanded through improved notice. For instance, Tymchuk et al. (1986) finds that user-friendly methods like large print and videos increase comprehension of patient rights in the elderly. DeChiara (1995) argues that a rule requiring employers to provide more detailed information on worker rights could reduce employee ignorance of the right to bargain.

that the inclusion of erroneous law (specifically, unenforceable provisions) in contracts and leases can have the effect of deterring individuals from exercising their rights under the law—rendering them “demoralized by contractual fine print” (Furth-Matzkin and Sommers 2020).<sup>6</sup>

Research indicates that the inclusion of terms in contracts in particular (as opposed to, say, an online policy containing the same information) influences people’s beliefs about the enforceability of the terms in question and deters action at odds with mistaken beliefs (Wilkinson-Ryan 2017). In a lab experiment close in flavor to our own research in a real-world employment setting, Wilkinson-Ryan (2017) studies whether exposing individuals to information at odds with contract language can counter mistaken beliefs about the presumptive enforceability of contract terms. She shows that giving individuals information indicating that a court previously held a term in a contract to be unenforceable reduces an individual’s belief that the same term in their contract will be enforced. But without such guidance there is considerable scope for sophisticated parties to generate and take advantage of mistaken beliefs about the law and, particularly, the enforceability of unenforceable terms. Darley et al. (2001) hints that such manipulation will be likely be easier to accomplish when unenforceable terms are actually enforceable in many or most other places.

Together, this body of research implies that employers in jurisdictions where noncompetes are unenforceable may nonetheless include them in their employment contracts to limit employee mobility. The potential profitability of such a strategy calls to mind Salop and Stiglitz’s (1977) model where imperfect information and high costs to attaining information among consumers benefits knowledgeable sellers. Such conditions will produce monopoly prices, with sellers confident that the burdens and costs of obtaining market information will prevent consumers from switching to another seller. In our context, it would be monopsony power wielded by employers. The high cost of ascertaining adequate information deters employees from exercising their legal rights against unenforceable restrictions. Employers, then, reduce their employees’ outside options by eliminating many alternative employers and consequently increase their own leverage.

Starr et al. (2021b) presents evidence that unenforceable noncompetes do in fact affect employee mobility. In other words, the mere existence of a noncompete term even in a nonenforcing jurisdic-

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<sup>6</sup> It is now well established that the use of unenforceable contractual provisions is anything but rare. In the non-compete setting, Prescott et al. (2016) and Starr et al. (2020a) show that noncompetes are virtually as common in jurisdictions that do not enforce noncompetes as they are in jurisdictions that do enforce them. Furth-Matzkin’s (2017) seminal work in the residential lease context shows that this finding is no fluke. In Boston, she found widespread inclusion of either misleading or flat-out invalid terms within these agreements. Her work confirmed empirically, at least in the rental context, what the literature had long contemplated: that offerors have much to gain and little to lose by including beneficial yet unenforceable terms (Kuklin 1988). Furth-Matzkin’s more recent work (including with Sommers) establishes that “gain” is the more likely outcome, with unenforceable terms apparently influencing beliefs and behavior in experimental settings involving consumer scenarios. Our work here extends this literature to real-world long-term employment contracts/relationships and actual future mobility intentions.

tion alters behavior. In this work, we test whether at least some of this behavioral influence is due to mistaken beliefs about enforceability. Of course, there are alternative explanations. First, employees may not be mistaken about noncompete unenforceability and yet comply because of the reputational costs of not following through on their “promise” (Sullivan 2009). Second, even if there are no reputational consequences, employees may not violate a noncompete they *know* to be unenforceable because of a subjective cost of breaking one’s word (Sullivan 2009, Fried 2015). However, if the provision of information alone about enforceability changes behavior for those who were mistaken but not those who were not mistaken about enforceability, we can infer that these alternative theories at least do not entirely explain the effects of unenforceable noncompetes.

Additionally, our paper seeks to understand why, in the noncompete context, mistaken beliefs are persistent and whether providing accurate information can change persistent mistaken beliefs about enforceability. Given previous findings suggesting people view contractual terms as presumptively enforceable (and fair), their inclusion within an employment contract is likely to be determinative of employees’ beliefs about their enforceability. We test the consequences of directly providing information on whether a noncompete is enforceable on beliefs, predictions, and intentions. In doing so, we extend Wilkinson-Ryan’s (2017) research by evaluating the informational impact of the provision of a more reform-friendly summary of settled state law about an entire categories of provisions rather than a past case finding a particular hypothetical term unenforceable. The strategic use of unenforceable provisions is particularly likely and costly in the context of noncompete agreements (Sullivan 2009),<sup>7</sup> so evidence that speaks to the potential value of an information campaign designed to reduce or eliminate mistaken beliefs is of particular policymaking significance.

### 3. Survey Data and Enforceability Measures

Our data come from a proprietary survey that we developed and implemented in 2014 to examine the use and consequences of noncompetes in the U.S. (Prescott et al. 2016).<sup>8</sup> The sample population are individuals aged 18 to 75 who are either unemployed or employed in the private sector or in a public healthcare system. The full sample comprises 11,505 respondents drawn from all states, industries, occupations, and other demographic categories.<sup>9</sup> Using this data, Starr, Prescott, and Bisha-

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<sup>7</sup> Sullivan (2009) reviews how the approach courts take toward unenforceable noncompete clauses encourages their use by employers. Courts, Sullivan argues, seek to do justice among the parties before them and often construe these clauses in ways to strike the unenforceable portions but salvage the contract broadly. He argues this does little to address the actual problem of these unenforceable provisions: the deterrence of the many who view these terms in these contracts as enforceable. Instead, he argues courts should rethink their role as salvagers of contracts and take on a role with a broader view of these terms’ impacts.

<sup>8</sup> We provide a brief discussion of the data here and refer the interested reader to our Online Data Appendix for further information, with an even more detailed description appearing in Prescott et al. (2016).

<sup>9</sup> To ensure that the data are nationally representative, we created weights for our analysis using iterative proportional fitting (“raking”) to match the marginal distributions of key variables in the 2014 American Community Survey. Many weighting schemes were considered. See Tables 16 and 17 in Prescott et al. (2016) for more details.

ra (2021a) provide the first systematic evidence of the use of noncompetes across the labor force, finding that roughly one in five U.S. labor force participants are currently bound by noncompetes. Starr, Prescott, and Bishara (2021b) follow up by showing how noncompetes influence the process of employee mobility, independent of their actual enforceability.

To examine what employees believe about noncompete enforceability and the consequences of violating their noncompete as well as how those beliefs matter to their forward-looking intentions and expectations, we leverage several novel aspects of our survey data. First, we analyze employees' *beliefs* about whether their employer would take noncompete-related legal action if they took a job with a competitor and their *beliefs* about whether their noncompete would ultimately be enforced in court.<sup>10</sup> Second, we conduct an information experiment that we built into our survey instrument in which a random selection of respondents are informed of the actual noncompete enforcement policies of their state. In our view, our information experiment can be taken as a rough simulation of an educational campaign or improved access to legal information, but it also functions as a source of exogenous variation in beliefs about noncompete enforceability, which allows us to identify the effects of beliefs on future behavior.

To study how beliefs vary by noncompete enforceability—and to implement our information experiment—we build a measure of actual enforceability using contemporaneous state noncompete policies (Beck 2014),<sup>11</sup> which captures the conditions under which states will (and will not) enforce noncompetes and includes any exemptions under state law. We summarize these dimensions in Table A1,<sup>12</sup> which shows which states have adopted each policy and the score that each policy receives in our overall measure. In the table, we report policy variation with respect to 1) how states treat overbroad noncompete clauses, 2) whether states enforce noncompetes when employees are terminated without cause, and 3) whether noncompetes require additional consideration beyond continued employment. For each policy, a score of “1” is associated with the highest likelihood that a court will enforce a noncompete coming before it (e.g., even scenarios in which an employer terminates the employee without cause), and “0” is associated with the lowest likelihood of court enforcing a noncompete. We then add a fourth dimension: whether the state will enforce noncompetes at all (the three states that do not enforce at all are California, North Dakota, and Oklahoma). Next, we

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<sup>10</sup> We gauge this in two ways. First, we ask “*Are noncompetes enforceable in your state?*” Second, we ask respondents to assign a probability to the extent to which a court would enforce their noncompete were they violate it and their employer were to sue: “*If you were to quit your current job to work for or start a competing company, how likely is it that a court would actually enforce your noncompete (assuming your employer took legal action to try to enforce your noncompete).*” Third, we ask respondents to assess how likely their employer is to sue to try to enforce their noncompete: “*If you were to quit your current job to work for or start a competing company, how likely is it that your employer would take legal action to try to enforce your noncompete.*”

<sup>11</sup> See our Online Appendix OB for the exact documentation in Beck (2014).

<sup>12</sup> The language we use to describe enforceability in Table A1 with respect to each particular aspect of noncompete policy is also identical to the language we used in our information experiment.

aggregate across all four measures for each state, such that the maximum score a state can receive is “4” for robust enforceability. Finally, we take into account any exemptions associated with specific professions (e.g., physicians) in the state (meaning that employees with different occupations in the same state may have different enforceability measures) and divide by the maximum score possible for each state. The final score for each respondent is between “0” and “1.”

For purposes of the paper and in our analysis, we classify state-occupation combinations with a score of “0” as “no enforceability,” scores between “0” and “1” as “medium enforceability,” and scores of “1” as “high enforceability.” Table 1 shows which states (or state-occupations) fall into each category and provides summary statistics across the full sample and the sample of individuals with noncompetes, which will be our focus in most of our analysis. Figure A1 shows a map of the US, shaded according to the level of enforceability.

#### **4. Empirical Analysis and Results**

In this section, we study what individuals believe about the enforceability of their noncompetes, the accuracy of those beliefs, and why, if at all, employees may be persistently misinformed. We also describe and report the results from our information experiment, which effectively “shocks” employees’ beliefs with accurate information about noncompete enforceability. We use the experiment not only to determine whether and how accurate information alters preexisting mistaken beliefs about noncompete enforceability—as well as to see whether mistaken beliefs can fully account for the behavioral effects of unenforceable noncompetes (Starr et al. 2021b)—but also to identify the causal relationship between an employee’s beliefs about enforceability and their future expectation and intentions regarding their noncompete-related behavior. Our various questions require a range of empirical tools, so we describe our empirical methods as needed along the way.

##### **4.1 Employees Beliefs About Noncompete Enforceability**

To begin, Table 2 tabulates responses to the survey question “Non-competition enforcement policy is determined at what level?” Notwithstanding recent federal noncompete policy proposals (beginning circa 2015) and conversations about regulation by the Federal Trade Commission, non-compete policies are and historically have been under the purview of states (Bishara 2011). Only 23% of respondents—just three percentage points higher than guessing at random—are aware of state legal primacy in this domain. The proportion of respondents who answer correctly in our survey scales somewhat with education, with those with above a bachelor’s degree more likely to recognize that noncompetes are enforced at the state level (32%) in comparison to those with less than a bachelor’s degree (21%). Those who agreed to a noncompete with their current employer are also slightly more likely to recognize that their noncompete is governed by state law (30%) relative to those who are not bound by a noncompete (23%). Taken together, Table 2 suggests that the majori-

ty of employees, regardless of their education level and even if they are presently subject to a non-compete, are unaware that noncompete enforceability is state-level policy.

Panel A of Table 3 presents tabulated answers to the following question “Are noncompetes enforceable in your state?” In the full sample, 59% believe that noncompetes are enforceable, compared to just 5% who believe that they are unenforceable (which is clearly too low, considering that 13% of the population resides in states that do not enforce noncompetes) and 37% who report that they do not know the answer to the question. While there is relatively little heterogeneity across education levels, 76% of those bound by a noncompete believe that noncompetes are generally enforceable, compared to 61% of those who do not have a noncompete (and just 37% of those who are not sure if they are bound). For each cut of the data, however, less than 10% of the sample believe that noncompetes are not enforceable, suggesting that the modal set of beliefs are that noncompetes are enforceable—especially for those presently subject to one.

Panel B of Table 3 documents the extent to which these beliefs are accurate, using our broad classification in which California, North Dakota, and Oklahoma are treated as the only states in which noncompetes are unenforceable.<sup>13</sup> We refer to those who report not knowing their state’s law in Panel A as the “uninformed,” and their proportions are unchanged in Panel B. The “misinformed” are those who incorrectly estimate noncompete enforceability in their state. They make up 11% of the full sample and 13% of those who are bound by noncompetes.<sup>14</sup> In contrast, the “informed”—those who correctly estimate noncompete enforceability in their state—amount to 52% of the population and 67% of those bound by noncompetes. The apparently high proportion of “informed” may be illusory and just a function of chance and the relevant shares; most states happen to enforce noncompetes, and the majority of employees appear to believe that their states will enforce noncompetes. The proportion could simply be the result of individuals going with what they sense is the “majority” rule and just happening to be correct most of the time (Darley et al. 2001).

Figure 1 depicts the level of employee “informedness” about the law among those with noncompetes according to actual state policies, where the “no enforceability” states are those that entirely preclude enforcement for all employees (i.e., California, North Dakota, and Oklahoma) and where the medium/high enforceability states are the complement. The figure shows that while 74.4% of those with noncompetes are informed in states that enforce noncompetes, 69.2% of them are misinformed in states that do not enforce noncompetes (9.9% are uninformed). Figure 2 depicts these patterns by education level (among those affirmatively bound by a noncompete). While more

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<sup>13</sup> We do not incorporate the occupation-specific carveouts in this measure because the question refers to the state broadly and is not specific to the respondent’s occupation.

<sup>14</sup> We classify as misinformed those in California, Oklahoma, or North Dakota who answered that noncompetes *are* enforceable and those in the rest of the states who stated that noncompetes are *not* enforceable.

educated employees appear to be slightly more informed in states that do not enforce noncompetes, more than 70% of those with above a bachelor’s degree are misinformed (64.8%) or uninformed (7.1%). Taken together, Table 2 and Figures 1 and 2 establish that employees bound by noncompetes tend to believe that noncompetes are enforceable in their state—even when they are not—and that this pattern is relatively stable across education levels.

We can assess the robustness of these findings by turning to a more nuanced measure of non-compete enforceability that is specific to the employee’s current employment situation. The survey asks respondents to answer the following question using a scale of 0–100: “If you were to quit your current job to work for or start a competing company, how likely is it that a court would actually enforce your noncompete (assuming your employer took legal action to try to enforce your non-compete).” Answers to this question thus provide a continuous and subjective assessment of the employee’s beliefs that a court, if asked, would enforce their specific noncompete. Figure 3 documents a strong, positive relationship between this continuous measure of beliefs and the blunt, categorical beliefs of Table 3. The graph plots the predicted values from a regression of subjective beliefs on an indicator for categorical beliefs fully interacted with indicators for whether the employee is bound by a noncompete, controlling for demographic, job-level, and firm-level characteristics, which we refer to as “basic controls.”<sup>15</sup> Figure 3 shows that employees who report that noncompetes are not enforceable estimate the likelihood of enforcement in their case to be much lower than those who believe noncompetes are enforceable, with those who are uncertain falling in the middle (see Table A2, columns (1) and (2) for regression results with and without controls).

Using this individual-specific measure of enforceability (i.e., respondent’s beliefs about likely enforcement in their own situation), Figure 4 depicts whether beliefs about enforceability correspond with actual enforceability by noncompete status, while accounting for our basic controls.<sup>16</sup> If employees are accurately informed about noncompete enforceability generally speaking, Figure 4 should be weakly upward sloping. But the lines are relatively flat and between 40 and 47% for the population with a noncompete—and similarly flat for those without a noncompete, though the levels differ (see columns 3 and 4 of Table A2). These figures suggest that, as before, employees whose noncompetes would not be sanctioned by their state courts are generally unaware of that fact. Figure

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<sup>15</sup> Specifically, basic controls include employee gender, employee education, employee race, a third degree polynomial in employee age, the class of the employer (e.g., for-profit), the type of occupation (2-digit SOC), industry (2-digit NAICS), employee class (e.g., hourly vs. salary), hours worked per week, weeks worked per year, the interaction of hours and weeks worked, employer size, whether the employer has multiple establishments, and the log of number of establishments in the employee’s county-industry. The 95% confidence intervals reflect standard errors clustered at the state level, the level at which courts and legislatures determine noncompete enforcement policy (Abadie et al. 2017).

<sup>16</sup> In contrast to the broad state-level measure of actual enforceability (i.e., do vs. do not enforce) that we use in the previous section, in this analysis and in all work below that relies on these individual-specific, continuous beliefs, we incorporate the occupation-specific exemptions under the law from Table 1 into the “no enforceability” group.

5 studies only the noncompete population to determine whether more educated employees are more likely to be informed (accounting for our basic controls). As in Figure 2, we find that employees of all education levels seem to be mistaken about the law, at least in states where noncompetes are unenforceable (see columns (5) and (6) of Table A2).

## 4.2 Persistent Inaccuracy of Employee Beliefs

The prior section establishes that employees with unenforceable noncompetes are largely unaware of the fact that courts will refuse to enforce their agreement not to compete. Importantly, employee beliefs are not random. Descriptively, employee mistakes about enforceability favor mistaken beliefs that unenforceable noncompetes are enforceable rather than beliefs that enforceable noncompetes are unenforceable. Hypotheses that would explain this pattern include that 1) a default presumption that contracts generally and noncompetes specifically are enforceable and 2) that any particular noncompete is likely enforceable given noncompetes are enforceable in a “majority” of jurisdictions (Darley et al. 2001). But both of these hypotheses fly in the face of traditional views about the advantages of learning the truth (which seem significant) and the information-diffusing benefits of labor markets. Employment contracts are high stakes, and employees looking for a new position will presumably meet competitors who *do* know when a provision is unenforceable. In this section, we consider two hypotheses—one supply side and one demand side—to explain why employee beliefs about enforceability may be persistently and asymmetrically inaccurate.

The supply-side hypothesis is that many employees who mistakenly believe their noncompetes are enforceable may opt out of searching for a position with a competitor, thereby short-circuiting the labor market’s ability to correct their mistaken beliefs. To assess this possibility, we study the extent to which an employee reports searching for jobs at competing firms within the last year (measured on a scale from 0–10). In the sample of employees with noncompetes, we regress this measure of search effort on indicators for whether the employee is informed about the law, interacted with actual noncompete enforceability, and our basic set of employer and employee controls. The results, shown in Figure 6, indicate some support for this hypothesis. Employees who are informed that their noncompetes are unenforceable exert 50% more search effort towards competitors relative to those who are misinformed (mistaken) or uninformed (3.74 vs. 2.48). In contrast, among employees with enforceable noncompetes, we observe little difference between the informed and un/misinformed (see columns (1) and (2) of Table A3).

An important limitation of this analysis is that we have no exogenous variation in an employee’s beliefs or in the accuracy of their beliefs about enforceability. Accordingly, our results should be interpreted as descriptive; some unobservable factor may exist that drives both how informed an employee is about the enforceability of their noncompete and their search effort. A related concern is

that the relationship may capture reverse causation—those who exhibit more search effort toward competitors may be more likely to learn about the law. Acknowledging these concerns, we nevertheless show that those who do not know that their noncompete is unenforceable—approximately 80% of those living in states where noncompetes are unenforceable per Figure 1—are less likely to search for new positions at competing firms, thus limiting their ability to learn about the laws governing their contract. This finding reminds us that certain mistakes—even mistakes about the law—may deter activities that lead to learning and error correction and thus become persistent.

The demand-side hypothesis is that employers in states that do not enforce noncompetes may have relatively weak incentives to inform employees at competing firms about the lack of enforceability of their noncompetes—even when they wish to poach these employees. At first blush, this possibility seems counterintuitive. If a competing firm wants to poach employees with unenforceable noncompetes, one would guess it only needs to give these employees offers and inform them that their noncompetes are unenforceable. However, such “informative” recruiting may either be unattractive to the poaching employer or unlikely to succeed without substantial effort (Gabaix and Laibson 2006). First, the recruiting employer may not benefit on net from informing a prospective employee of the lack of noncompete enforceability. If the recruiting employer’s existing employees, for instance, *also* mistakenly believe their noncompetes are enforceable (as seems likely given Section 4.1), then successful recruiting may be a pyrrhic victory that produces higher employee turnover and wage costs if the hire eventually informs the employer’s workforce about their own unenforceable noncompetes. Second, employers using unenforceable noncompetes may still threaten potentially departing employees with litigation by reminding them of these provisions (or actually suing them), which may render employees *more* (not less) likely to believe their noncompetes are enforceable.

To assess the potential for competitor recruitment to inform employees about the law, we exploit two unique aspects of our survey data. The first is an indicator for whether the employee reported receiving a job offer from a competitor in the last year. The second is an indicator for whether—if an employee’s present employer becomes aware of the employee’s job offer from a competitor—the employer reminded the employee of their noncompete obligations. Figure 7 displays the results from a regression, including the basic controls, of employee beliefs regarding the level of noncompete enforceability interacted with the whether the employee in question received a job offer from a competitor within the last year. The results offer some support for the demand-side hypothesis: employees who received offers from competitors are actually somewhat more likely to believe unenforceable noncompetes are enforceable (55% vs 47%), though these differences are not statistically significant (see columns (3) and (4) of Table A3).

Figures 8 and 9 attend to the role of strategic reminders in keeping employees misinformed about the unenforceability of their noncompete. Figure 8 shows that, comparing two observationally

equivalent employees who are bound by noncompetes and have received job offers from competitors, an employee with an *unenforceable* noncompete is approximately 40 percentage points more likely to be reminded about their noncompete (71% vs 32%, 34%). Figure 9 documents that these reminders alone are associated with increased beliefs about the enforceability of noncompetes, regardless of the level of enforceability (see columns (1)-(4) of Table A4).<sup>17</sup> Taken together, Figures 7, 8, and 9 imply that rather than operating to inform employees when they have an unenforceable noncompete, recruitment activity by competitors—and the subsequent threats and reminders that may result—can prevent employees from learning that their noncompetes are unenforceable.

A key limitation of our analysis of noncompete reminders is that relatively few employees with noncompetes in our sample have received offers from competitors that become known to their employer—which is necessary for their employer to respond by issuing a reminder (237 total observations). To bolster our analysis, we turn to a second question in the survey that asks all individuals with a noncompete: “Are you aware of any instances in which your employer sued an employee for violating a non-competition agreement?” The logic for considering this question is that reminders are a likely precursor to a lawsuit, and so knowledge of a prior lawsuit may operate similarly to increase employee beliefs about enforceability. It also reflects the idea that employee beliefs may respond not only to what they have experienced personally (as in the reminders analysis) but also to the experiences of their present and former coworkers. Figure A2 shows that approximately 20–24% of individuals with noncompetes are aware of (or believe they are aware of) their employer suing others over noncompetes, and this relationship is relatively flat according to the level of enforceability (see columns (5) and (6) of Table A4). Combined with the reminder results, this pattern suggests that while employers are more likely to remind employees about unenforceable noncompetes, they may be no more likely to pursue litigation. Interestingly, however, Figure A3 shows that employees who believe their employer has sued past employees are significantly more likely to believe that their noncompetes are enforceable (see columns (7) and (8) of Table A4), and this effect is especially pronounced for employees whose noncompetes are actually unenforceable. Thus, with reminders and (frivolous) lawsuits, employers seem endowed with at least some ability to convince individuals with unenforceable noncompetes that their noncompetes are in fact enforceable.

### 4.3 Information Experiment Design and Balance Tests

Regardless of the *reasons* for employees’ persistent mistaken beliefs about noncompete enforceability, plausibly effective policy responses include educational campaigns—such as the regular posting of employee contractual rights and information on signs at the workplace or elsewhere—and mandatory legal disclosures that are comprehensible, easy to verify, and conspicuous. To gauge the

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<sup>17</sup> Both Figures 8 and 9 report results from regressions reported in Table A4.

potential effects of providing accurate information to employees, we roughly simulate this strategy for correcting mistaken beliefs by conducting an information experiment within our survey. Researchers have employed information experiments in many settings to examine, for example, the impact of information on business economic expectations over time (Coiboin et al. 2018), college major choices (Wiswall and Zafar 2015), and settlement decisions (Sullivan 2016).

Our information experiment proceeds in three steps. First, we assess our respondents' baseline expectations about noncompete enforceability (which we describe and analyze at length above) and how respondents regard the effect of their noncompete on their behavior. Next, we randomly assign approximately 50% of respondents (50.1% and 52.43% of the unweighted full and noncompete samples, respectively) to receive legal information on the actual enforceability of noncompetes, individualized for a given respondent based on their state of employment. Finally, we reevaluate their beliefs about the enforceability of noncompetes and the potential impact of these provisions on the respondent's behavior by re-administering questions from the first stage of the information experiment—including even those who did not receive the information treatment.

We gathered the specific information we provided in the experiment from the characterization in of law in Beck (2014), provided in Online Appendix OB, which we summarize in Table A1. The actual information that we provided to those who received information is provided in Figure A4 and Figure A5, which appeared in that order to respondents. Figure A4 explains that noncompete policy is conducted at the state level, and that only a few states do not enforce them.<sup>18</sup> It also highlights the typical reasonableness test that state courts conduct when they decide whether to enforce a non-compete. Figure A5 displays all of the state-specific information that was presented to respondents, where the blue arrows allow for “display logic” such that only certain sets of information will show up depending on the state in which the respondent works (see Table A1 to link specific policies to individual states).<sup>19</sup>

In Table 4, we display the results of a balance test to verify that individuals with noncompetes are balanced between treatment and control groups, both overall and within each of the state enforceability levels. With the exception of the gender variable—men are five percentage points more

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<sup>18</sup> In Figure A4, we only list California and North Dakota as the nonenforcing states. This is discordant with Beck (2014), who includes Oklahoma as a nonenforcing state. We exclude Oklahoma from Figure A4 because in the literature we found competing views on whether Oklahoma was truly a non-enforcing state (see Bishara 2011). Nevertheless, we include in the state-specific information regarding Oklahoma the characterization indicated in Beck (2014). As a result, employees in Oklahoma (of which we only have 118 in the full sample—of whom only 13 indicated having a noncompete) may be *undertreated* by our experimental choices.

<sup>19</sup> We made one error in carrying out our information experiment. According to Beck (2014), Alabama does not enforce noncompetes for professionals. Our information experiment unintentionally neglected to include that information. There are only 25 respondents with noncompetes from Alabama, although 12 of these are professionals. Fortunately, this error does not materially influence our results.

likely to be in the group that received information (and this is driven by the medium enforceability category)—there are no statistically significant differences between the treatment and control groups in the full sample or any subsample.

#### 4.4 Information Effects on Employee Beliefs

Figure 10 shows the distribution of beliefs among individuals with noncompetes across the treatment and control groups—i.e., according to whether the individual received information on actual noncompete enforceability in their state. The top row of Figure 10 shows, not surprisingly but reassuringly, that the distribution of beliefs before and after the experiment among those who did not receive any information are nearly identical. In contrast, for those who receive information in the no enforceability group, we see a large leftward shift in the distribution of beliefs, which indicates that the employees actually read and absorbed the information in the treatment. In the medium and high enforceability states, we see slight shifts rightward in the distribution. Figure 11 presents the simple mean effects corresponding to the post-experiment beliefs by treatment status (corresponding to regression results in Table A5 columns (1) and (2)). Consistent with Figure 10, those who receive information that their noncompetes are unenforceable are far less likely to believe that their noncompete is enforceable (24%) relative to those who did not receive information (46%). The effects are more muted for the medium and high enforceability levels. Taken together, Figures 10 and 11 demonstrate that the information experiment was most effective in changing beliefs for those whose noncompetes were entirely unenforceable, which is where the bulk of mistaken beliefs can be found in this domain. Notably, providing information that noncompetes are unenforceable—at least as we do here—does not completely disabuse the treated of their mistaken beliefs.

Importantly, the raw distributions and mean effects we present in Figure 10 and 11 may mask heterogeneity in whether and how much respondents update their beliefs after the experiment relative to their initial beliefs. Figure 12 addresses this issue by presenting the unconditional binned scatterplot of the relationship between pre-experiment beliefs and post-experiment beliefs (Starr and Goldfarb 2020). If respondents estimate the same level of enforceability before and after the experiment, their responses would line up along the 45-degree line (shown in red in Figure 12). Matching estimates along the 45-degree line is primarily what we observe for those who do not receive information, regardless of the level of actual enforceability (left panel of Figure 12). In contrast, Figure 12's right panel indicates those who receive information update *differently* given initial beliefs and actual enforceability. For example, those who initially estimate their noncompete to be enforceable with certainty reduce their post-experiment beliefs considerably: those whose noncompetes are unenforceable reduce their estimate to approximately 35%, while those in medium and high enforceability states reduced their beliefs to 75-80%. These latter shifts imply that accurate, precise information even for the medium and high enforceability states may give employees some doubt that

their noncompetes will be enforced. We see a similar pattern among those who previously viewed their noncompetes as largely unenforceable—those individuals update their beliefs upward, especially if they live in a state where noncompetes are moderately or highly enforceable.

Figure 13 characterizes the mean effects on beliefs among individuals with noncompetes that we document in Figure 12 by splitting the sample by pre-experiment beliefs above or below the median (50%) and then regressing post-experiment beliefs on a treatment indicator that we interact with actual enforceability and basic controls (see Table A5 columns (3) and (4)). The results show that the drop in mean beliefs in Figure 11 is driven almost entirely by the changing beliefs of those who initially viewed their noncompetes as enforceable. For example, for those with above-median pre-experiment beliefs about enforceability in their state, information receipt causes beliefs to fall from 81% to 26% when their noncompetes are actually unenforceable, and even causes drops of 8–10 percentage points in medium and high enforceability states. In contrast, those who initially believed their noncompetes were unenforceable (left panel of Figure 13) are largely unmoved by the information—even in the medium and high enforceability states.<sup>20</sup>

#### 4.5 Information Effects on Prospective Employee Behavior

In this section, we examine whether the delivery of accurate information about noncompetes enforceability produces changes in an employee’s prospective mobility behavior. Unfortunately, we are unable to track employee decisions or behavior over time. Instead, we estimate an employee’s very short run reaction to exposure to enforceability information using their answers to questions that appear after the experimental treatment in the survey flow. We cannot know whether the outcomes we study below will ever translate to actual changes in mobility at some point in an employee’s future. However, it is reasonable to assume that changes in prospective mobility outcomes are a necessary precursor to behavioral change.<sup>21</sup> In other words, if information has no apparent effect on an employee’s expectations or predictions, it seems unlikely to matter to actual behavior.

To collect a broad measure of whether noncompetes influence employee mobility, our survey presents respondents with the following question both before and after the experimental treatment portion: “If you received a much better offer from a comparable, competing employer, would your noncompetes be a factor in preventing you from moving?” In Figure 15, we calculate how responses

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<sup>20</sup> Figure A6 shows the same heterogeneity for the sample of employees not bound by a noncompetes. Those who receive information and mistakenly believed that any noncompetes would have been enforceable (had they agreed to one in their current job) also dramatically update their beliefs about enforceability (right panel). In contrast to the sample of individuals with noncompetes, however, those who mistakenly believed any noncompetes would not have been enforceable also update their beliefs moderately when those noncompetes are highly enforceable (left panel).

<sup>21</sup> Anecdotally, several of the survey participants who received information thanked us at the end of the survey for letting them know that their noncompetes were unenforceable. This suggests that there may have been some real knowledge learned that might underly future behavioral decisions.

to this question differ depending on treatment status and the level of enforceability.<sup>22</sup> For individuals with unenforceable noncompetes, 51% of those who did *not* receive information indicated that their noncompete would be a factor in whether they would accept the job offer, versus 26% among those who received accurate information about lack of enforceability. For individuals with moderately enforceable noncompetes, the difference is smaller (46% vs 38%), while there is no difference for highly enforceable noncompetes. Figure 16 breaks out this analysis based on individual responses to this same question before the experiment (see Table A6 columns (3) and (4)). Those who initially believe that their noncompetes are enforceable—when they are not—experience the largest drop to 51%. Notably, the control group also shifts downward a little as well, suggesting that control respondents answer the question differently the second time. There are fewer differences in the sample of individuals who initially report that their noncompete would not be a factor.<sup>23</sup>

One important and interesting result of this analysis is that, even after employees learn that their noncompete is unenforceable, many still indicate that they weigh their noncompete as a factor in deciding whether to take a better job. This result implies that while mistaken beliefs about enforceability explain a relatively large portion of how unenforceable noncompetes succeed at deterring employees from taking better jobs, noncompetes—even unenforceable ones—likely play other roles in employee mobility decisions as well. These could include, for example, the subjective cost of violating one’s word, the reputational cost of breaking a “promise,” or even the financial cost of defending oneself from a frivolous lawsuit (Sullivan 2009). We return to this issue in Section 4.7.

#### **4.6 Effects of Beliefs about Enforceability on Employee Behavior**

The experimental treatment exogenously causes employees to update their beliefs about noncompete enforceability. We can use this variation to understand how post-experiment beliefs affect various prospective behavioral outcomes. The basic idea is that employee beliefs randomly update when (1) initial beliefs are wrong and (2) the respondent receives the information treatment, as shown in Figure 13. To identify the effects of beliefs about enforceability on an employee’s predicted future mobility decisions, we develop an instrument which is the three way interaction between the actual enforceability of noncompetes, the individual’s pre-experiment enforceability beliefs (a dummy for above or below 50%), and an indicator for whether they receive information. Figure 13 (which is the first stage of the 2SLS regressions) shows that the compliant subpopulation driving any

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<sup>22</sup> The sample is limited to individuals with current noncompetes, and the underlying regression specification includes basic controls. We report the full results in Table A6.

<sup>23</sup> Figures A7 and A8 show the same patterns hold for whether noncompetes will be a factor in starting a new business. The precise question in the survey is: “If you developed an idea to start a new company that competes with your current employer, would your noncompete be a factor in preventing you from starting the competing firm?”

local average treatment effects are mostly those individuals whose noncompetes are unenforceable and who initially believed their noncompete was enforceable.

Table 5 documents the 2SLS results for a variety of relevant behavioral outcomes. Columns (1)–(3) examine whether beliefs that a noncompete is enforceable cause an employee to conclude that their future job options are limited and whether an employee’s noncompete would be a factor in their choice to take a better job or start a competing enterprise. In all cases, we find that believing that the noncompete is enforceable causes sizable increases in feel their noncompete limits their job options. These estimates are also quite large in magnitude. For example, a worker who believes their noncompete is enforceable with certainty is 43 percentage points more to feel like their noncompete limits their future job options (86% of the sample mean) and 66 percentage points more likely to report their noncompete is a factor in joining a competitor (59% of the sample mean) relative to a worker who does not believe their noncompete is enforceable<sup>24</sup>

If believing that a noncompete is enforceable causes employees to forgo job opportunities (at least prospectively), an important question is whether these ex post consequences lead employees to negotiate over the terms of their noncompetes or for other benefits in exchange for agreeing not to compete. That is, if employees who believe their noncompetes are enforceable are more likely to see their noncompete as limiting their job opportunities in the future, do they negotiate in the hope of obtaining some compensating differential up front? Figure 17 shows that, comparing observationally equivalent individuals with noncompetes, the likelihood they report negotiating over their noncompete does not vary dramatically between states that do and do not enforce noncompetes.<sup>25</sup> Column (5) of Table 5 reports the instrumented results for how believing that noncompetes are enforceable causes changes in negotiation expectations. Consistent with Figure 17, we find no evidence that believing noncompetes are enforceable causes employees to change their negotiating patterns—at least for those bound by noncompetes. This set of results calls into question freedom-of-contract argu-

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<sup>24</sup> Table A7 bolsters these relationships by exploiting answers to a series of questions about how important various factors are in determining whether an employee decides to move to a comparable, competing company. Column (1), (2), and (3) shows that believing that a noncompete is enforceable increases the importance of the employee’s simply agreeing to a noncompete, the importance of the possibility their employer will sue to enforce the noncompete, and the importance of the likelihood that the court will enforce it. Columns (4), (5), and (6) examine how beliefs about noncompete enforceability change the relative importance of entering into a noncompete as compared to a range of employment amenities. In each specification, believing that a court would approve a noncompete following litigation causes an employee to more heavily weight the importance of agreeing to a noncompete relative to job amenities such as compensation, lifestyle benefits, or opportunities for greater prestige or training.

<sup>25</sup> Figure A9 examines whether the information treatment led employees to update the likelihood that they would negotiate in the future over noncompetes. While there is enormous difference in levels between Figure 17 (which reflects actual reported negotiation behavior) and Figure A9 (which reflects prospective negotiation behavior), the information treatment does not appear to differentially cause individuals to change their negotiation likelihood relative to the control group. A likely reason that the mean levels of negotiation are different is that the second question asks about whether the employee *would* negotiate over a noncompete as opposed to whether those with noncompetes *actually* negotiated over their current noncompete.

ments often made in favor of enforcing noncompetes—that applicants and employees will negotiate for compensating differentials.<sup>26</sup>

#### 4.7 Beliefs about the Likelihood of a Lawsuit as a Mechanism

How much beliefs about the enforceability of noncompetes matter to behavior may depend in part on what employees believe about the likelihood that their employer will sue them for violating a noncompete in the first place—whether or not the noncompete is enforceable. Employers may sue an employee even when a noncompete is unenforceable simply to force the employee to defend at significant personal cost, and an employer who has an employee dead to rights for violating an enforceable noncompete may choose not to file a complaint. Furthermore, noncompetes may still matter even when employees believe them to be unenforceable and also believe that their employer will not sue to enforce because there may be moral or reputational costs to violating the spirit of the noncompete. We are able to use our rich data to investigate these ideas.

We begin by assessing whether noncompetes appear to influence job mobility choices even when employees believe that noncompetes are unenforceable *and* that, in any event, their employer will not litigate the point. Figure 18 examines this question by categorizing employees based on whether they view their noncompete as enforceable and whether employees perceive a lawsuit as likely (based on whether the reported likelihood is above or below 25%). We then cut the data by actual noncompete enforceability and further by whether a respondent received information on actual noncompete policies in their state.<sup>27</sup>

We uncover two strong patterns, both for those who have and do not receive information. First, individuals with noncompetes who believe that their noncompete is enforceable and that their employer is likely to sue them for breaching are much more likely to see their noncompete as a factor in whether to join a competitor (57%–78% depending on the level of actual enforceability) relative to those who see neither likelihood as very high (5%–25%). Second, even when employees know that their noncompete is unenforceable and do not believe their employer is likely to sue them, a non-negligible proportion still view their noncompete as a factor in accepting a competitor’s offer: 12% among those who are informed about the law, and 25% among those who did not receive information. This evidence suggests that while beliefs about enforceability and the likelihood of a lawsuit

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<sup>26</sup> In contrast, column (6) of Table 5 shows that those who are not bound by noncompetes would be more likely to negotiate over a new noncompete when they believe it would be enforceable. This shift appears to be driven by the fact that those not bound by noncompetes report being less likely to negotiate when they receive information about unenforceable noncompetes (Figure A10). It is not clear *ex ante* why these answers might differ from the noncompete sample in both direction and statistical significance. One possibility is that because these employees are not presently bound by noncompetes, they may be unfamiliar with the typical contracting process around noncompetes and therefore make different assumptions the costs and effectiveness of negotiation.

<sup>27</sup> We control for basic controls and cluster standard errors at the state level.

can explain a substantial proportion of the variation in whether employees view their noncompetes as a factor in accepting a position with a competitor, other reasons likely remain to account for why they see a noncompete as an impediment. Two natural explanations, which we cannot address further, are the subjective disutility and the reputational costs of breaking a promise.

This analysis is limited, however; it ignores the potential for beliefs about noncompete enforceability to change beliefs about the likelihood of a lawsuit. Figure A11 documents a strong positive correlation between an employee's beliefs about the law and their likelihood of being sued.<sup>28</sup> Moreover, in column (1) of Table 6, we use the same instrumental variables strategy we used in prior sections to examine how a change in beliefs about enforceability causally affects an employee's perception of the likelihood that their employer will sue them to enforce their noncompete. The results indicate that an employee who believes with certainty that his noncompete is enforceable will also believe that his employer is 41.1 percentage points (106% of the sample mean) more likely to take legal action relative to an employee who is certain noncompetes are unenforceable.

Given that changes in an employee's beliefs about enforceability cause changes in beliefs about litigation risk—and that both seem to relate to the extent to which noncompetes are a factor in changing jobs per Figure 18—we next explore whether the relationship between beliefs about enforceability and behavioral outcomes is driven entirely, in part, or not at all by changes in an employee's beliefs about the possibility of being sued over their noncompete. First, we explore the robustness of our earlier results to including controls for post-experiment beliefs about an employer lawsuit. We present our findings in columns (2) and (3) of Table 6. We estimate that those who receive information on the lack of enforceability of noncompetes are 25 percentage points less likely to report that their noncompete would be a factor in deciding to leave to work for a competitor. However, once we hold fixed an employee's post-experiment beliefs about the likelihood of a lawsuit, the estimate falls to 15 percentage points. Thus, changes in employee beliefs about litigation risk account for 40% of the overall effect of information about unenforceable noncompetes. Our analysis also indicates that beliefs about the threat of a lawsuit mediate the effect of information in medium and high enforceability states to a similar degree.

We take one final step to better understand how strongly the relationship between beliefs about enforceability and behavioral outcomes are explained by the likelihood of a lawsuit. Columns (4)–(7) examine OLS and 2SLS models, comparing whether beliefs about a noncompete's enforceability

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<sup>28</sup> Figure A9 further documents the correlation between beliefs about enforceability and beliefs about the likelihood of a lawsuit in three ways. In the left panel, we examine the unconditional correlation between pre-experiment beliefs about enforceability and the likelihood of a lawsuit; the middle panel shows the same correlation for the post-experiment variables, and the third panel looks at the correlation between the within-individual difference (post-pre experiment) in beliefs about enforceability and the beliefs about lawsuit likelihood. In all cases, increases in an employee's beliefs about enforceability are correlated with beliefs about the likelihood of a lawsuit.

relate to whether a noncompetitor would be a factor in accepting an officer with a competitor. The OLS specifications suggest that 27.9% of the overall relationship between beliefs and noncompetes being a factor in departing is explained by how much beliefs about enforceability drives changes in beliefs about litigation risk. Columns (6) and (7) report the same analysis, except using the instrumented measure for post-experiment beliefs.<sup>29</sup> A similar pattern arises, with the likelihood of a lawsuit accounting for approximately 18% of the relationship between beliefs about the law and the extent to which noncompetes matter for taking a competing job.

## 5. Discussion and Conclusion

In this study, we examine the beliefs employees possess about the enforceability of noncompetes, the accuracy of those beliefs, and how those beliefs influence behavior. We find that employees of all education levels tend to believe that noncompetes are enforceable, even when they are not. We present evidence for two mechanisms capable of supporting persistently mistaken beliefs by circumventing normal pathways for correction. First, employees who are unaware that their noncompetitor is unenforceable may opt out of an important “corrective” labor market activity by searching for jobs at competitors less often. Second, employers often remind employees of their noncompetitor—especially those who are only “bound” by unenforceable noncompetes—and these employees are subsequently more likely to (wrongly) believe that their noncompetitor is enforceable. Moreover, we show that beliefs about enforceability cause employees to be more concerned about their noncompetes when accepting jobs from competitors, and that this effect is driven in part by perceptions of the likelihood of a lawsuit. In turn, however, noncompetitor-bound employees are no more likely to negotiate over the terms of their noncompetitor or for other benefits in exchange for agreeing not to compete when the noncompetitor in question is more likely to be enforced.

We also show that an information experiment, which simulates an education campaign, can cause employees to update their beliefs—especially employees whose noncompetes are unenforceable. After receiving information, employees with unenforceable noncompetes subsequently report that their noncompetitor would be less of a factor in their choice to accept employment with a competitor. However, employees do not *fully* update their mobility choices, and even for employees who see their noncompetes as unenforceable and who see a lawsuit as unlikely, noncompetes still remain a factor in taking a job offer at a competitor for a nontrivial fraction of employees.

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<sup>29</sup> We note that the 2SLS estimates in columns (6) and (7) of Table 6 are constructed “by hand” (i.e., taking the predicted values from the first stage and including them in the second stage manually), so that we can include the beliefs about the likelihood in the second stage, but not the first. Note that typical empirical packages require all variables in the second stage to also be in the first stage. An exception is the “ivmediate” command in Stata for causal mediation analysis (Pinto (2019)), but this approach requires only one instrument—as opposed to our double and three-way interactions—and we also identified a weak instruments problem with respect to the mediating variable. As a result of our “by hand” approach, the standard errors in columns (6) and (7) are likely attenuated because they do not account for the uncertainty of the first-stage predictions.

Several limitations of this study are worth noting. First, because we cannot follow employees over time we can only estimate very short term elasticities. We hope future work will address this shortcoming by studying the long-term outcomes of similar information experiment. Second, our experiment was somewhat convoluted in its execution and specific to the context of a survey. To the extent that the medium and the specifics of the language itself were responsible for the effects (or lack of effects), our results may not extend to other types of educational campaigns (Armantier et al. 2016, Hertwig et al. 2014). Third, our negotiation results pointed in opposite directions for those bound and those not bound by noncompetes—the source of this apparent inconsistency is unclear, but perhaps individuals with noncompetes are more familiar with the typical contracting process. Nevertheless, noncompete negotiation important topic for future work. Finally, while we took great pains to clean and weight our data, the data nevertheless are derived from a selected sample. Accordingly, we hope future work examines these issues using alternative samples.

Our results contribute to the growing literature in law related to our understanding of unenforceable provisions. While others have found that employees tend to view contracts (especially leases) as enforceable in other settings, there are good reasons why we might observe different patterns for contracts that more directly determine an employee’s livelihood. Our results indicate that employees’ modal view is that contracts are enforceable regardless of whether they are.

These results also make several contributions to the growing literature on postemployment restrictive covenants. This literature focuses mostly on the enforceability of noncompetes, exploiting bans or other smaller changes in noncompete laws (Marx et al. 2009, Garmaise 2011, Balasubramanian et al. 2020, Lipsitz and Starr 2020, Johnson et al. 2020, Jeffers 2020). However, very little attention has been paid to the potential impact of unenforceable noncompetes and the role of individual beliefs about the law (Starr et al. 2021b). Our central contribution to this literature—and to all such literature studying state policy shocks without examining underlying beliefs—is that voiding contracts in court may have little practical effect if employees are unaware of the law. Moreover, employers may respond on other margins, such as reminding employees more vigorously about unenforceable noncompetes, in an effort to keep employees uninformed. Our work also documents that while education campaigns may be effective, they likely will not fully correct the problem since there may (at least for some) remain moral, reputational, and perhaps financial costs to violating even entirely unenforceable contracts. As a result, policymakers concerned about the potential ill effects of noncompetes may need to consider policies that induce employers to reduce the use of noncompetes in the first place, as opposed to policies that limit their enforceability in court.

Another set of contributions follows from our results on how beliefs affect negotiation. A central tenant of contracting, and indeed of legal reasoning (Blake 1960), is that freedom to contract will ensure that employees will not agree to contracts that hurt them—at worst they will negotiate for a

compensating differential. Our results challenge this argument by showing that when noncompete-bound employees believe their noncompete could be enforced they are no more willing to negotiate over the terms of their noncompete. Accordingly, theories that rely on assumptions that employees will be more likely to negotiate over enforceable noncompetes because they understand they law are unfounded and are unlikely to lead to accurate predictions. For empirical research that examines state-level policy shocks, our paper also raises broader questions related to how employees will understand these policy shocks. Our work emphasizes that whether shocks will influence subsequent behavior depends on, in part, employee beliefs about those policies. Empirical and theoretical work should be wary of assuming fully informed agents and ought to explicitly consider both how default beliefs are formed and what those beliefs imply when policies change.

Finally, persistent mistaken employee beliefs (and employees' unwillingness to negotiate for a compensating differentials) imply that noncompete agreements give employers monopsony power over employees in the labor market. That is, as Salop and Salop (1977) argue, if the marginal buyer is uninformed then firms can charge monopoly prices. In this context, if the marginal employee is mistaken about the law (and doesn't negotiate for a compensating differential up front), then the firm can exert monopsony power by reducing turnover and keeping wages low.

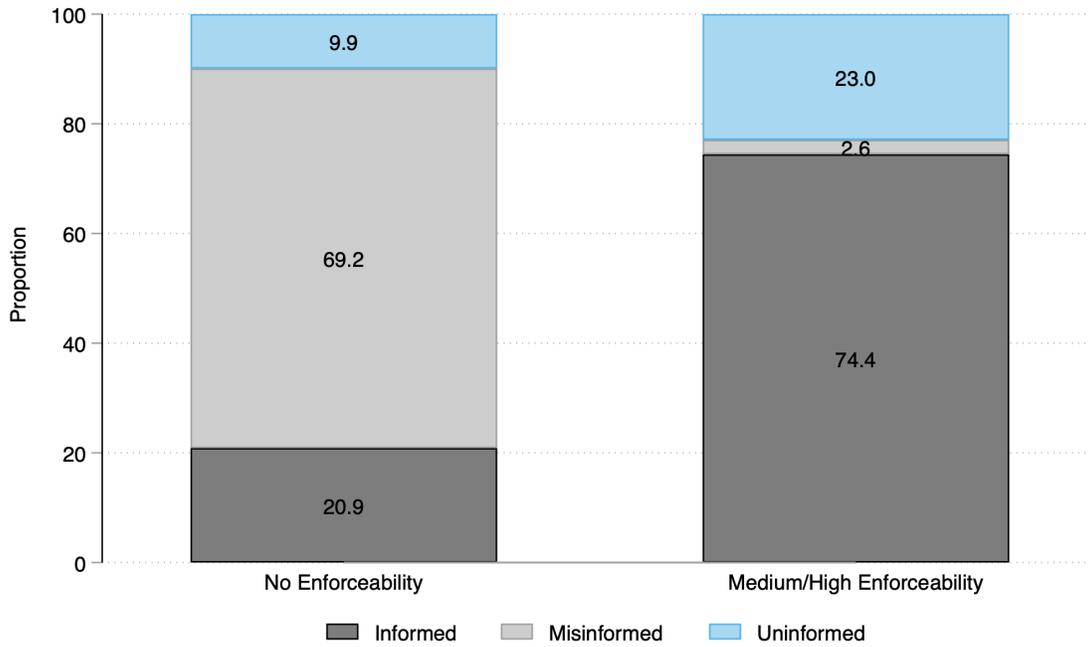
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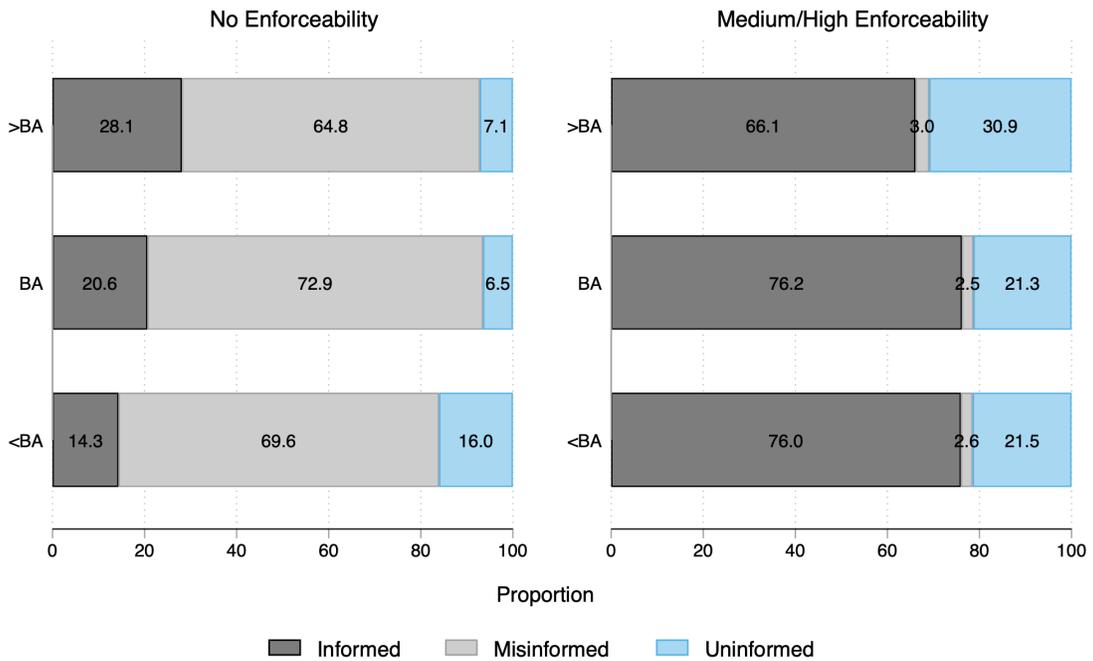
**Figures**

Figure 1. Correctly estimating NCA enforceability by enforceability level



Correct is simply an indicator for if the respondent correctly reported that their state will or will not enforce noncompetes. Results are from the weighted noncompete signer sample.

Figure 2. Correctly Estimating NCA Enforceability by Enforceability and Education



Correct is simply an indicator for if the respondent correctly reported that their state will or will not enforce noncompetes. Results are from the weighted noncompete signer sample.

Figure 3. Categorical and Continuous Beliefs about NCA Enforceability

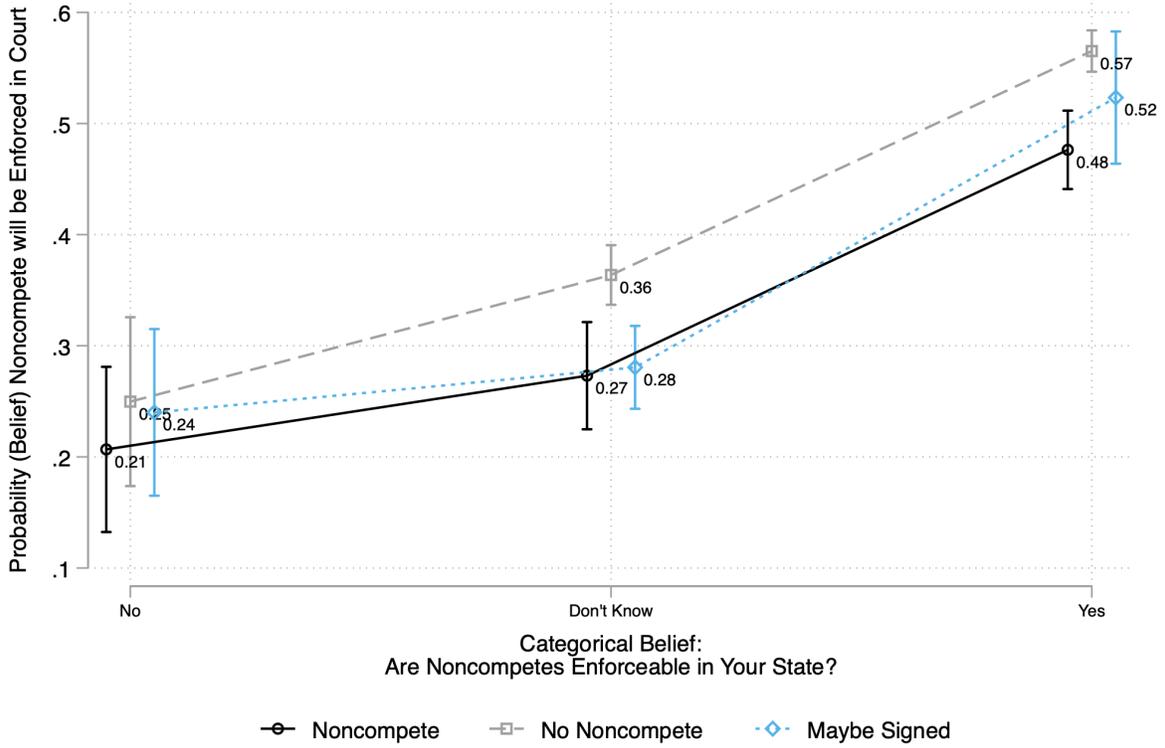


Figure 4. Beliefs about NCA Enforceability by Actual Enforceability and NCA Status

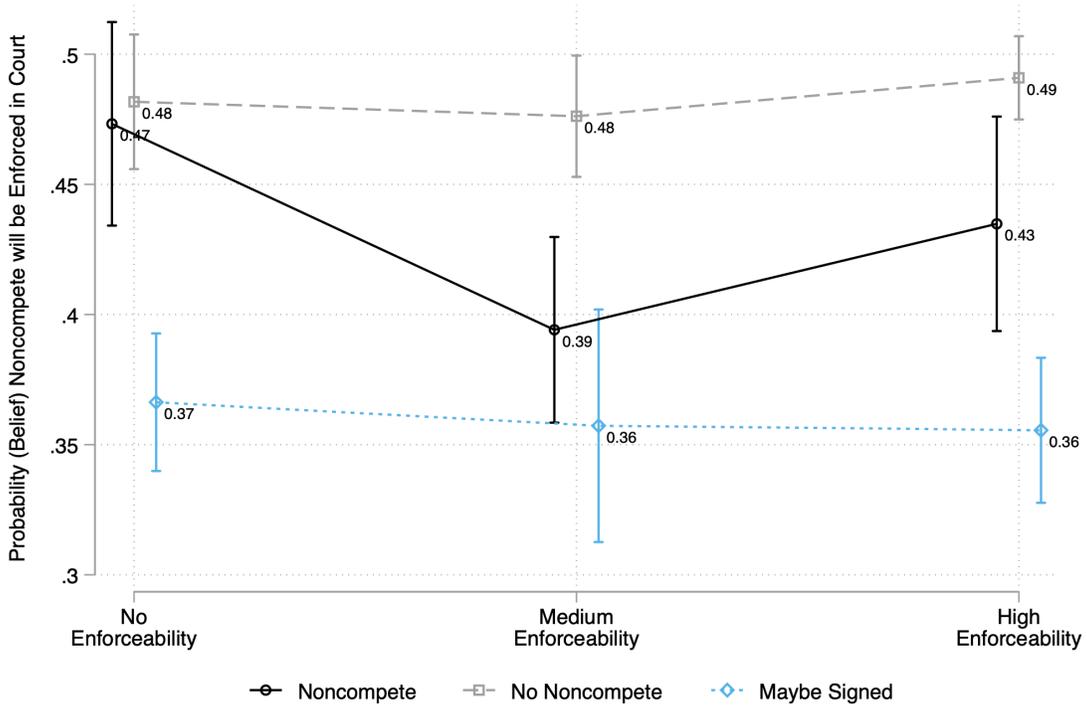
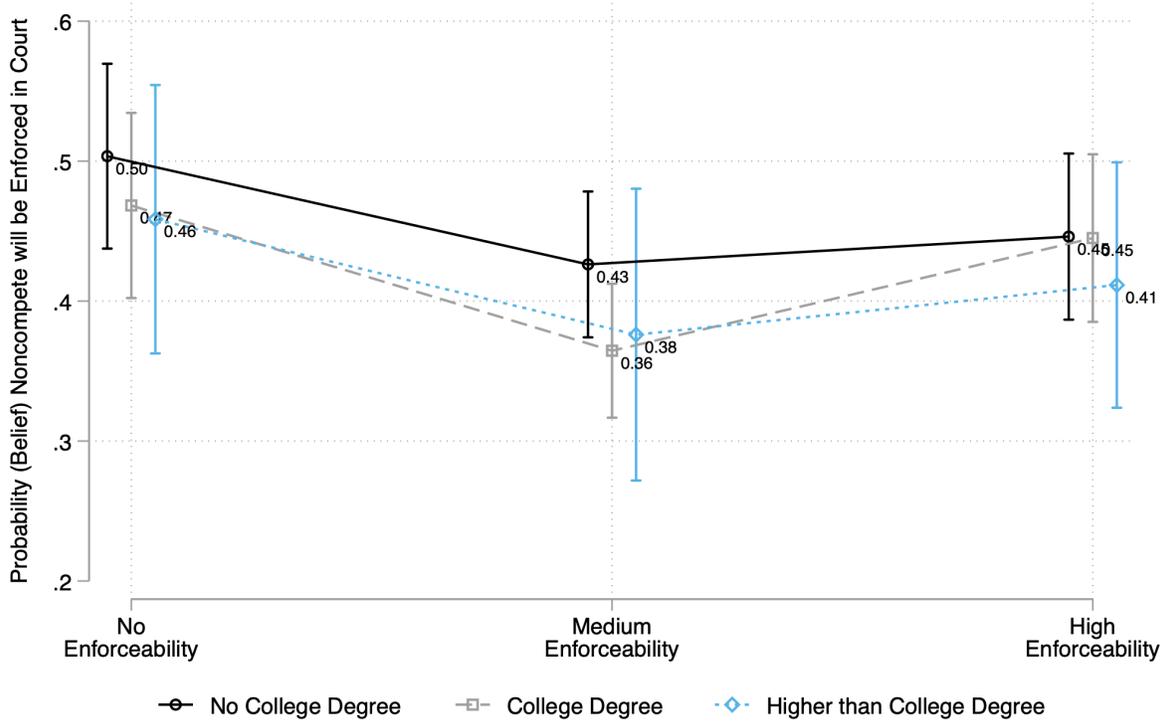
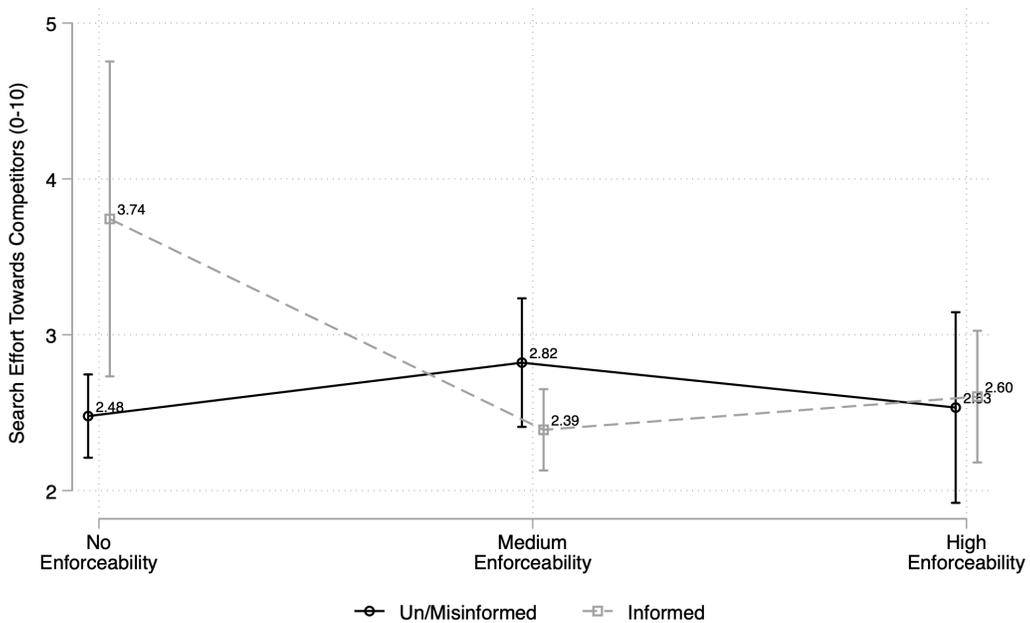


Figure 5. Beliefs about NCA Enforceability by Actual Enforceability and Education



Sample limited to those affirmatively bound by a noncompetite.

Figure 6. Search Effort Towards Competitors and Beliefs about NCA Enforceability



Sample includes only affirmed noncompetite signers. The plot shows the marginal effect of being informed relative to being un/misinformed by actual noncompetite enforceability.

Figure 7. NCA Enforceability Beliefs by Actual Enforceability and Competitor Offer

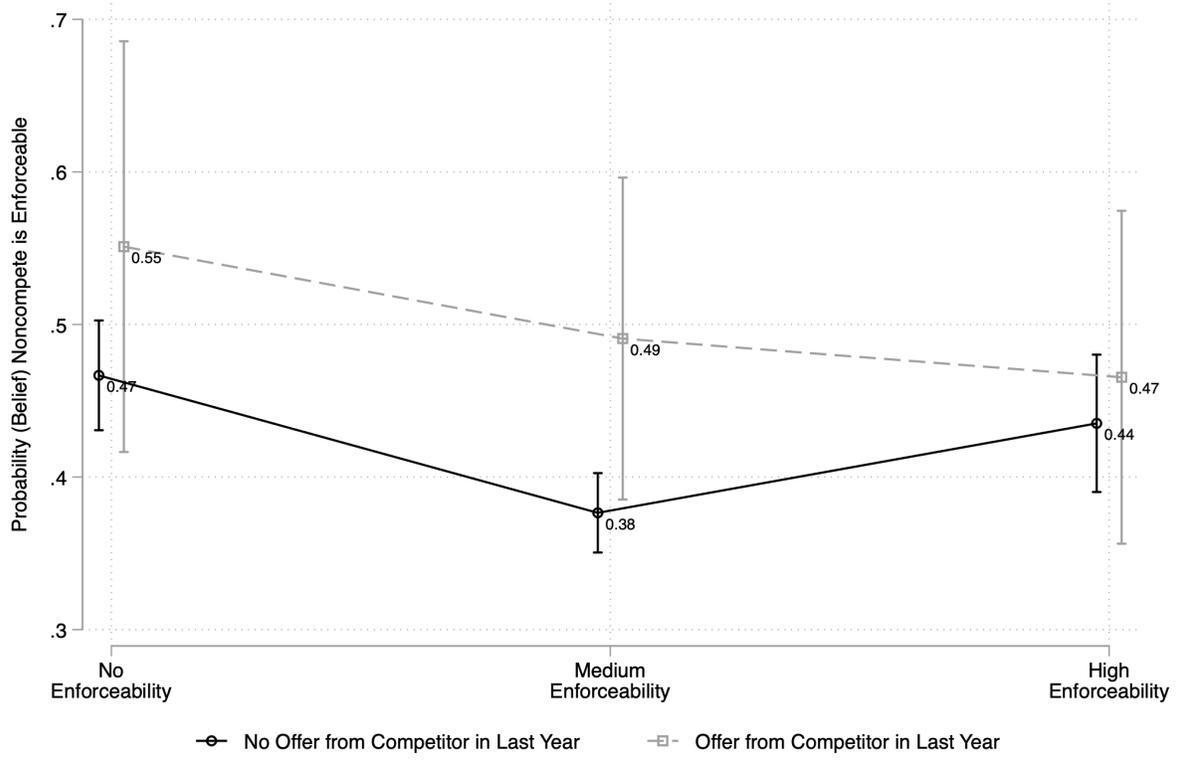


Figure 8. P(Reminded about NCA) by NCA Enforceability

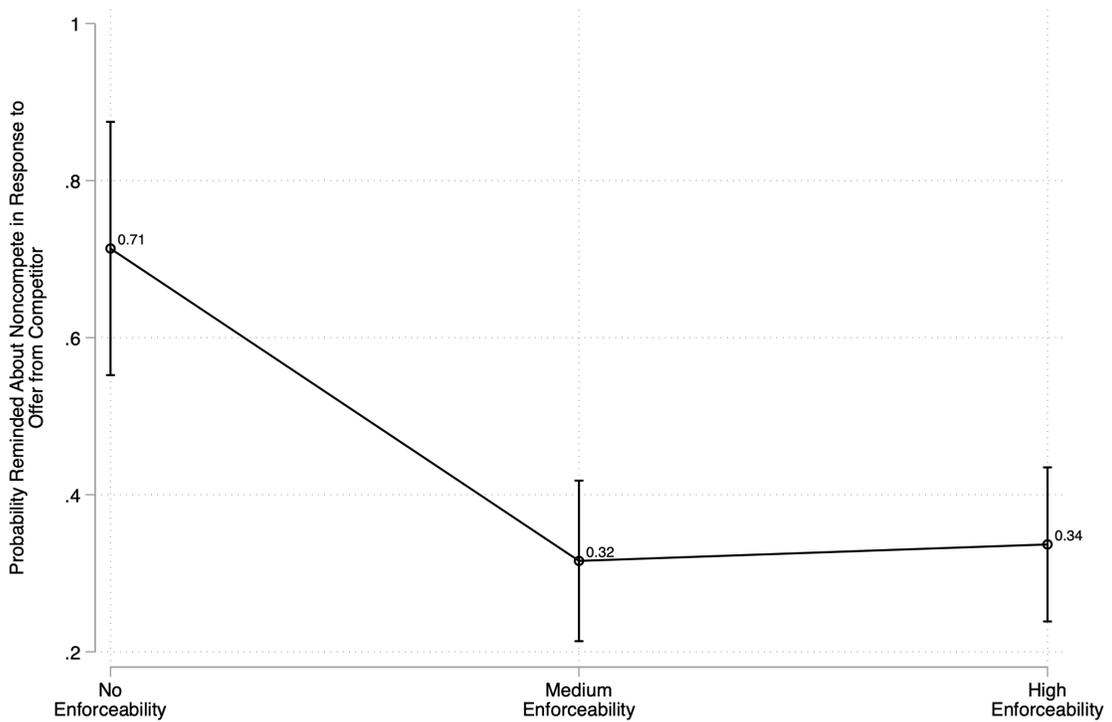


Figure 9. Reminders and Beliefs about NCA Enforceability

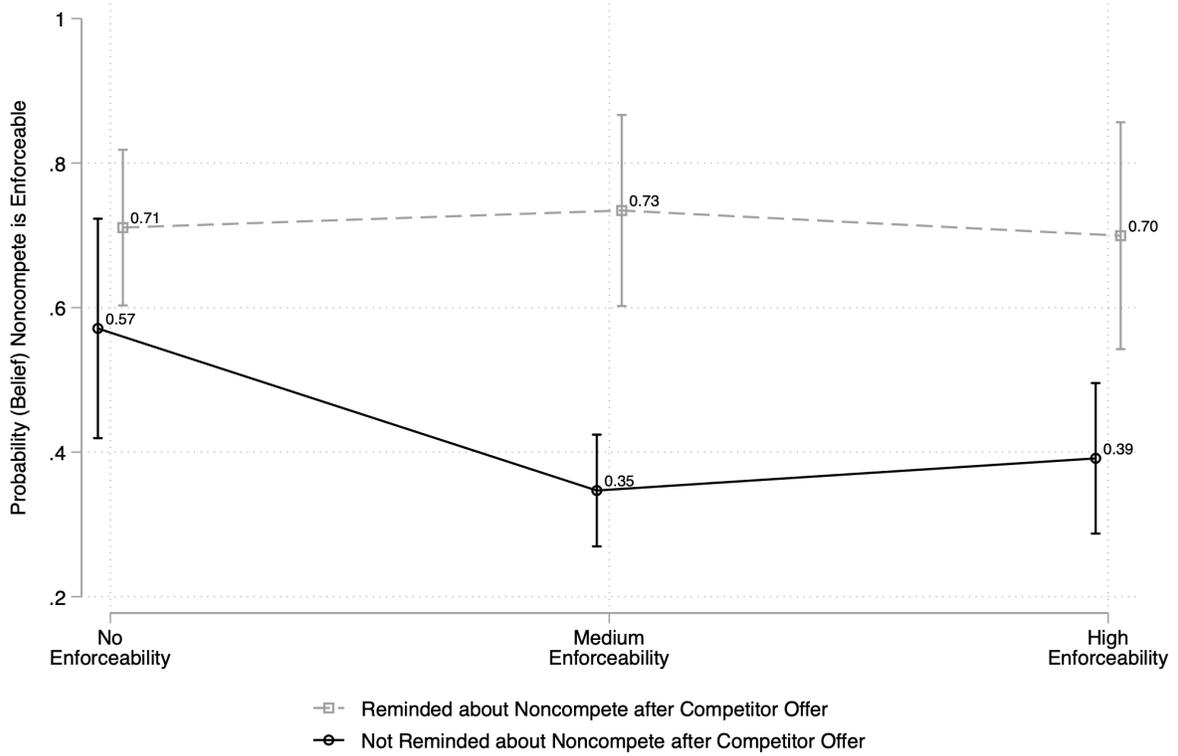


Figure 10. Distribution of Beliefs Pre and Post Experiment

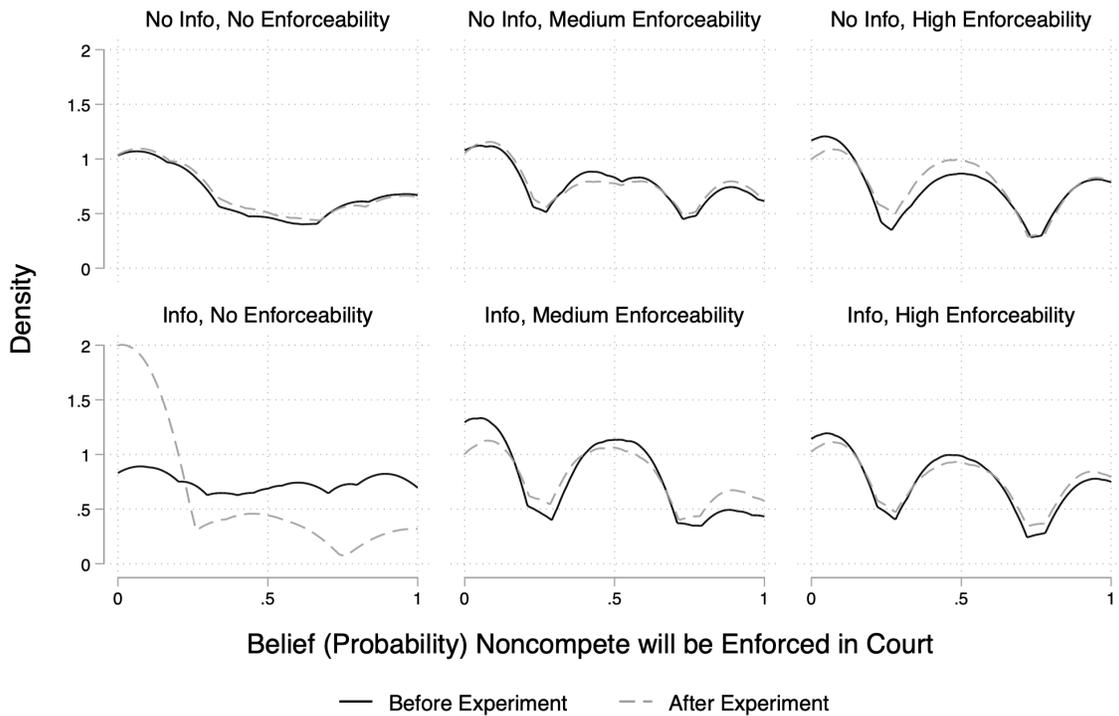


Figure 11. Average Post-Experiment Beliefs by NCA Enforceability and Treatment Status

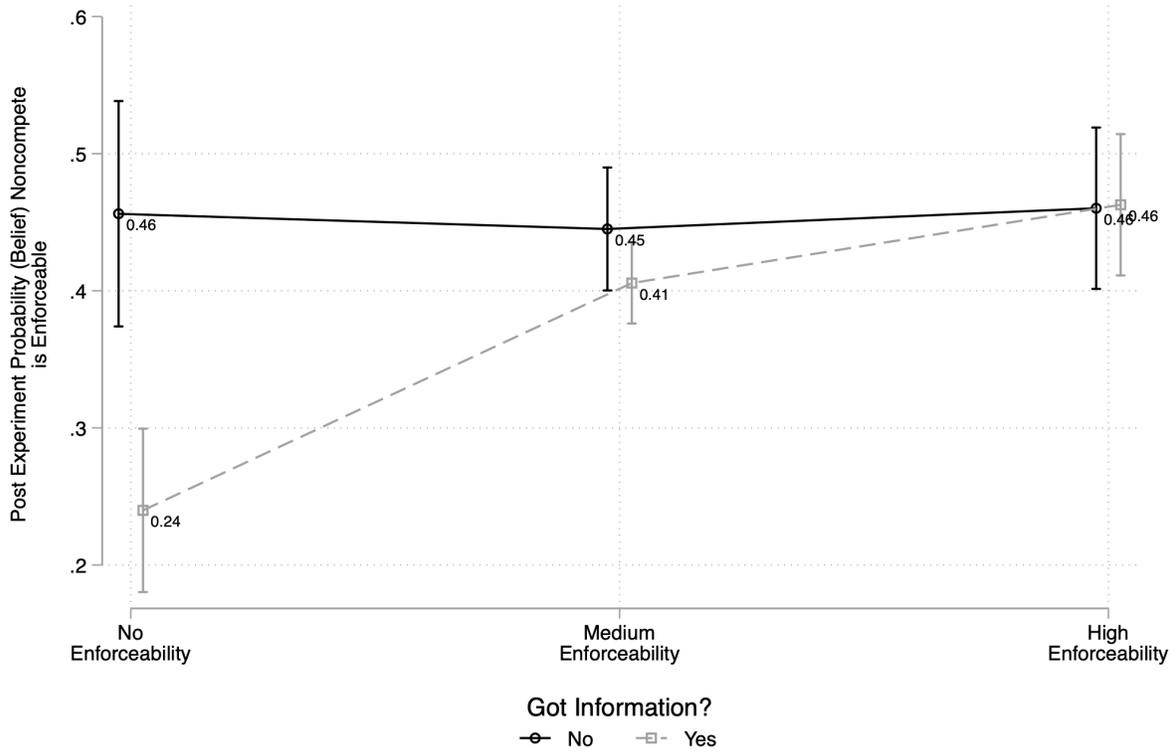


Figure 12. Relationship between Pre-Experiment and Post Experiment Beliefs

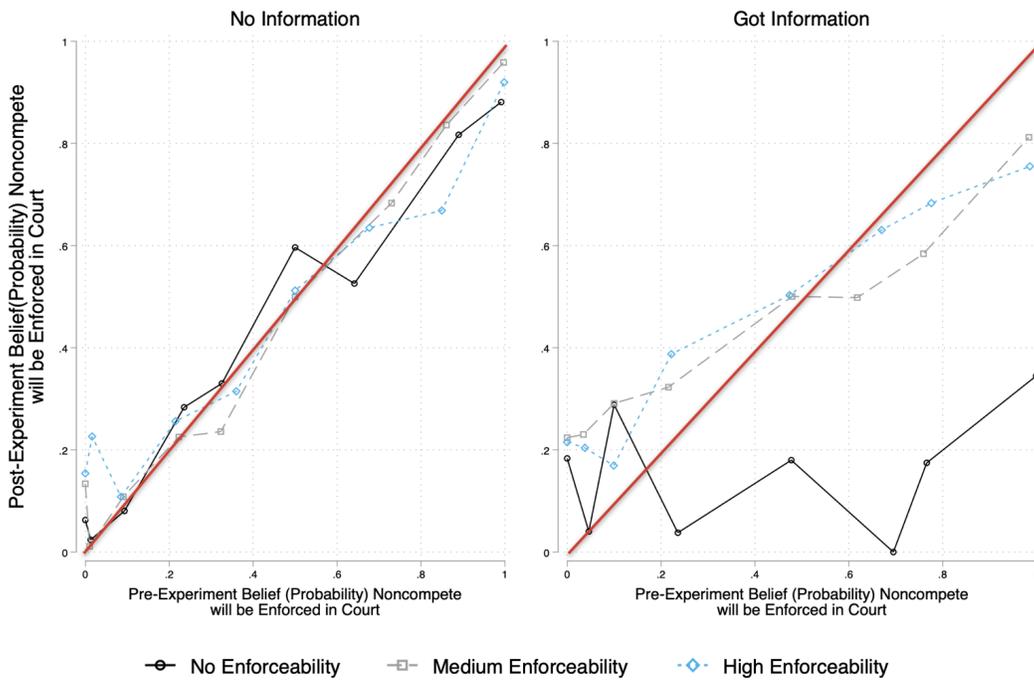


Figure 13. Heterogeneity in Post-Experiment Beliefs by Pre-Experiment Beliefs, among Noncomplete Signers

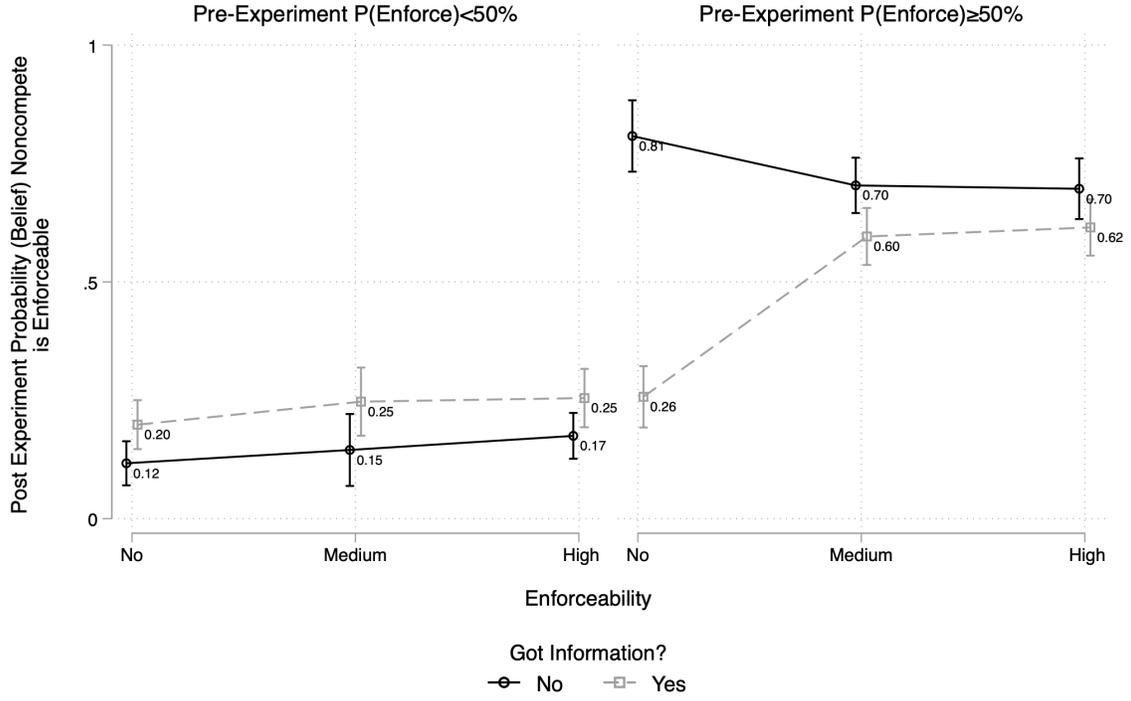


Figure 15. NCA Factor in Leaving by NCA Enforceability and Treatment Status

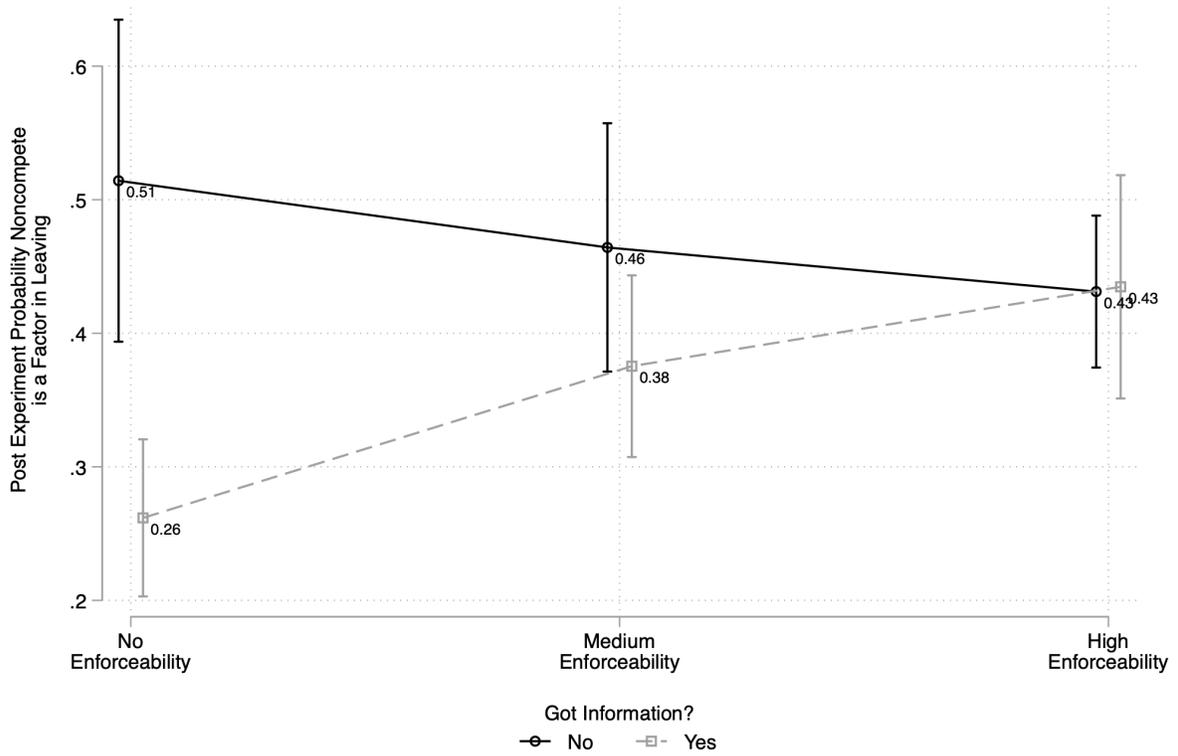


Figure 16. Heterogeneity in NCA Factor in Leaving by Pre-Experiment Answer

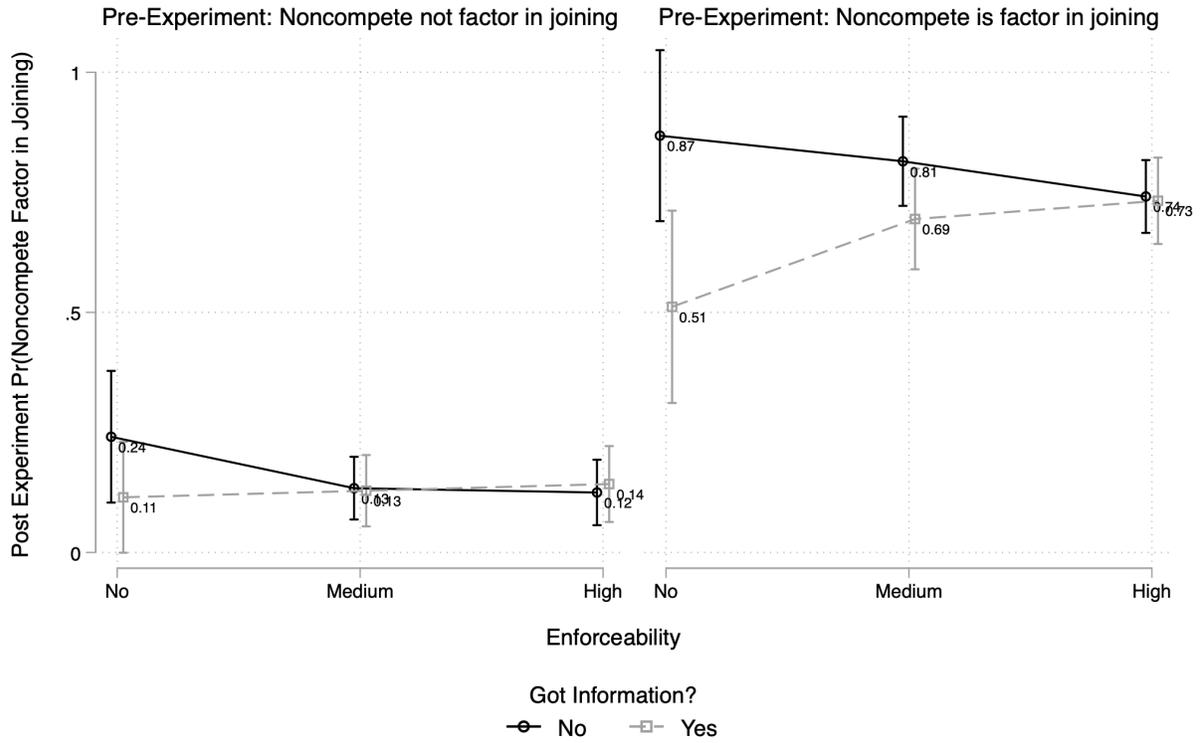


Figure 17. Negotiation over NCAs and NCA Enforceability, Among Noncompete Signers

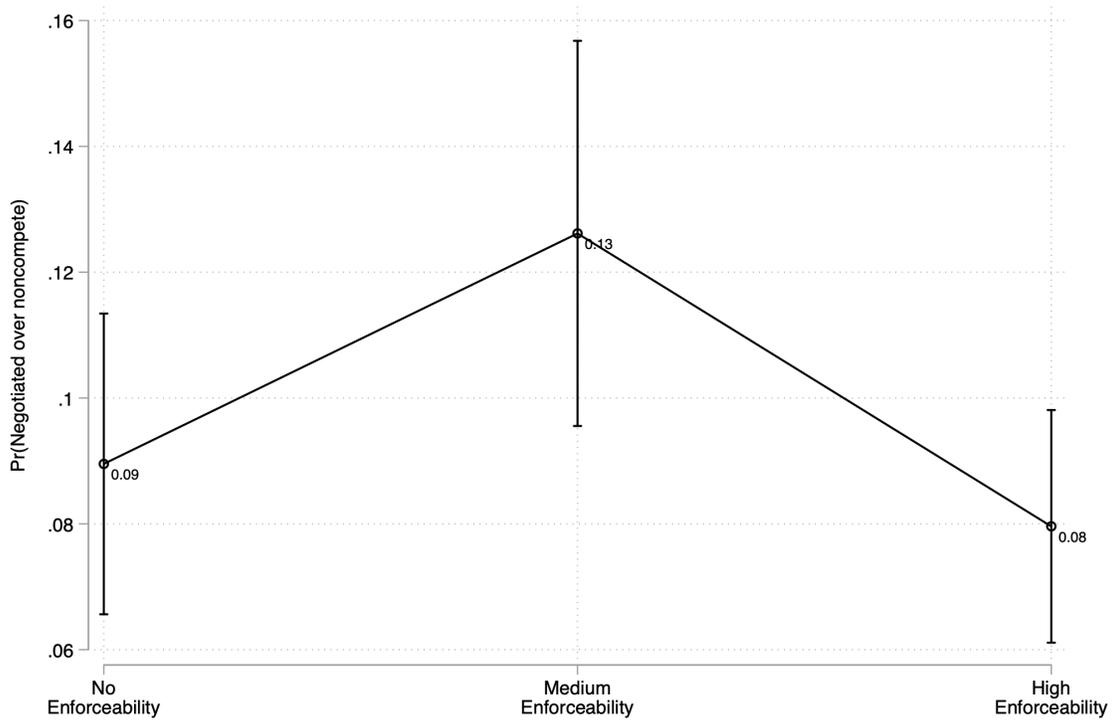
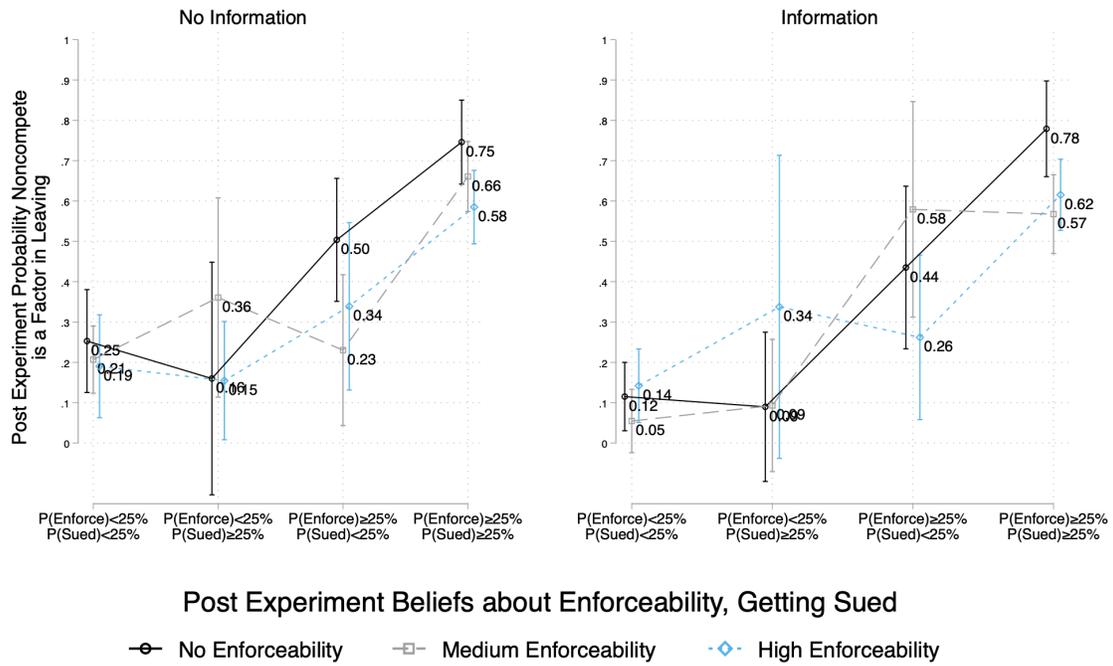


Figure 18. Noncompetes as a Factor in Leaving by Beliefs about Enforceability, Lawsuit



## Tables

Table 1. Summary Statistics By Enforceability Level

	(1)	(2)	(3)	(4)	(5)	(6)
	No Enforceability		Medium Enforceability		High Enforceability	
States	Arizona (Physicians), California, Colorado (Non-Professionals, Physicians), Delaware (Physicians), Illinois (Physicians), Massachusetts (Physicians), Tennessee (Physicians), North Dakota, Oklahoma, Texas (Physicians)		Arkansas, Indiana, Louisiana, Nebraska, Rhode Island, South Carolina, Virginia, Wisconsin		Alabama, Alaska, Arizona, Connecticut, Colorado (Professionals), Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Iowa, Kansas, Kentucky, Maine, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Nevada, New Hampshire, New Jersey, New York, Ohio, Pennsylvania, South Dakota, Tennessee, Texas, Washington, Wyoming	
<i>Sample</i>	Full Sample	Noncompete Sample	Full Sample	Noncompete Sample	Full Sample	Noncompete Sample
Number of Observations	1484	205	4376	685	5645	857
Age	40.51	42.43	40.11	39.32	40.48	40.45
Hours Worked Per Week	39.24	42.44	37.24	40.61	37.34	41.50
Weeks Worked Per Year	48.80	49.84	47.90	47.46	47.40	48.65
1(Male)	0.56	0.72	0.52	0.55	0.52	0.56
1(Multi Unit Firm)	0.64	0.78	0.64	0.77	0.62	0.67
1(Firm > 1k Employees)	0.39	0.49	0.38	0.45	0.37	0.40
1(Highest degree is $\geq$ BA)	0.44	0.68	0.27	0.47	0.30	0.51
Pre-Experiment P(Enforce)	0.43	0.46	0.42	0.40	0.43	0.44

Notes. Sample means presented for each sample, cut by the level of noncompete enforceability.

Table 2. Non-competition enforcement policy is determined at what level?

	Overall	Education Level			Agreed to Noncompete?		
		<BA	BA	>BA	Yes	No	Maybe
Don't know	0.44	0.48	0.39	0.32	0.33	0.45	0.52
Citywide	0.05	0.05	0.05	0.07	0.05	0.05	0.06
Countywide	0.05	0.04	0.05	0.07	0.06	0.04	0.04
Nationally	0.23	0.22	0.24	0.23	0.26	0.23	0.19
Statewide	0.23	0.21	0.27	0.32	0.30	0.23	0.19
Unweighted Observations	9,460	4,116	3,717	1,627	1,747	6,344	1,369

Note: The table displays percentages that sum to 100% within each column. Education level is the highest educational degree.

Table 3. Beliefs about noncompete enforceability in state

*Panel A. Are noncompetes enforceable in your state?*

	Overall	Education Levels			Agreed to Noncompete?		
		<BA	BA	>BA	Yes	No	Maybe
Don't know	0.37	0.38	0.33	0.34	0.21	0.36	0.54
No	0.05	0.05	0.04	0.07	0.04	0.04	0.09
Yes	0.59	0.57	0.63	0.60	0.76	0.61	0.37

*Panel B. Accuracy of Beliefs*

	Overall	Education Levels			Agreed to Noncompete?		
		<BA	BA	>BA	Yes	No	Maybe
Uninformed	0.37	0.38	0.33	0.34	0.21	0.36	0.54
Misinformed	0.11	0.10	0.13	0.15	0.13	0.10	0.12
Informed	0.52	0.52	0.54	0.52	0.67	0.54	0.34
Unweighted Observations	9,460	4,116	3,717	1,627	1,747	6,344	1,369

Note: The table displays percentages that sum to 100% within each column. Education level is the highest educational degree. Uninformed refers to those who do not know, while misinformed refers to those who selected the opposite of their state's policy. The states in which noncompetes are unenforceable are California, Oklahoma, and North Dakota. All others are enforcing (to some degree).

Table 4. Balance Test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Panel A: Full Sample of Noncompete Signers</i>									
	No Info	Info	p-value						
Age	41.87	41.47	0.51						
Hours Worked Per Week	42.16	42.60	0.38						
Weeks Worked Per Year	48.63	48.91	0.33						
1(Male)	0.53	0.58	0.05						
1(Multi Unit Firm)	0.74	0.72	0.21						
1(Firm > 1k Employees)	0.46	0.43	0.32						
1(Highest degree is BA)	0.68	0.67	0.90						
Pre-Experiment P(Enforce)	0.44	0.43	0.55						
<i>Panel B: Cut by Enforceability</i>									
	No Enforceability			Medium Enforceability			High Enforceability		
	No Info	Info	p-value	No Info	Info	p-value	No Info	Info	p-value
Age	41.85	41.10	0.67	41.50	41.14	0.70	42.17	41.83	0.69
Hours Worked Per Week	41.58	41.67	0.96	42.64	42.36	0.72	41.90	43.03	0.12
Weeks Worked Per Year	48.09	49.39	0.16	48.66	48.78	0.81	48.70	48.86	0.70
1(Male)	0.58	0.59	0.94	0.49	0.56	0.07	0.55	0.58	0.31
1(Multi Unit Firm)	0.76	0.77	0.82	0.77	0.74	0.34	0.72	0.69	0.31
1(Firm > 1k Employees)	0.43	0.45	0.79	0.49	0.46	0.36	0.43	0.41	0.48
1(Highest degree is BA)	0.73	0.74	0.85	0.65	0.64	0.66	0.68	0.69	0.95
Pre-Experiment P(Enforce)	0.40	0.44	0.47	0.42	0.40	0.64	0.48	0.46	0.41

Notes: Sample includes 1,747 noncompete signers. P-value reflects a test of the null hypothesis of no mean difference between the information and no information groups. Comparisons are unweighted using Stata's "orth\_out" command.

Table 5. Instrumenting for post-experiment enforceability beliefs.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Model: 2SLS	1(Current noncompete limits future job option)	1(CNC factor in joining competitor)	1(CNC factor in starting competitor)	1(Would negotiate over non- compete)	
Instrumented P(Enforce)	0.434*** (0.163)	0.659*** (0.127)	0.577*** (0.121)	-0.121 (0.136)	0.286*** (0.081)
Sample	Noncompete	Noncompete	Noncompete	Noncompete	No Noncom- pete
Controls	Yes	Yes	Yes	Yes	Yes
Pre-Experiment measure of DV	Yes	Yes	Yes	Yes	No
Observations	1,747	1,747	1,747	1,709	9,758
F-Stat	38.24	27.44	28.09	34.52	47.61
Mean of DV	0.233	0.415	0.523	0.603	0.744

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Robust standard errors in parentheses, clustered at the state level. Columns (1)-(5) include only noncompete signers, while column (6) focuses on those not bound by noncompetes. All models except for column (6) include the main effects of the pre-experiment measure of the particular dependent variable, which are all measured a second time after the experiment (both for those who did and did not receive the information). The instrument for post experiment beliefs is a three-way interaction of an indicator for pre-experiment beliefs about enforceability being greater than 50%, being in a no, medium, or high enforceability state, and whether they randomly received information. Controls include the pre-experiment beliefs about enforceability, indicators for enforceability (no/low, medium, high) interacted with a dummy for pre-experiment beliefs being greater than 50% (as in the instrument), and other demographics described in text. The F-Stat reported is the Kleibergen-Paap Wald rk F statistic which tests for weak instruments with clustered standard errors.

Table 6. The Mediating Effect of the Likelihood of a Lawsuit

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	P(Employer would sue over noncompete if violated)					1(CNC factor in joining competitor)	
Post Experiment P(Enforce)	0.411*** (0.087)			0.541*** -0.035	0.390*** (0.049)	0.657*** (0.110)	0.540*** (0.111)
Post Experiment P(Sued)			0.570*** -0.056		0.287*** (0.069)		0.207*** (0.051)
1(Information)		-0.252*** (0.064)	-0.150** (0.056)				
1(Medium Enforceability)		-0.050 (0.079)	-0.051 (0.071)				
1(High Enforceability)		-0.083 (0.065)	-0.105* (0.057)				
1(Medium Enforceability)*1(Information)		0.164** (0.077)	0.071 (0.072)				
1(High Enforceability)*1(Information)		0.256*** (0.081)	0.171** (0.073)				
Model	2SLS	OLS	OLS	OLS	OLS	2SLS	2SLS
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,747	1,747	1,747	1,747	1,747	1,747	1,747
F-Stat	30.88						
Mean of DV	0.389	0.415	0.415	0.415	0.415	0.415	0.415
% of Main Effect Driven by Likelihood of Lawsuit					27.91%		17.81%

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses, clustered at the state level. Only noncompete signers included in the sample. The instrument for post experiment beliefs is a three-way interaction of an indicator for pre-experiment beliefs about enforceability being greater than 50%, being in a no, medium, or high enforceability state, and whether they randomly received information. Controls include the pre-experiment beliefs about enforceability, indicators for enforceability (no/low, medium, high), and other demographics described in text. The F-Stat reported is the Kleibergen-Paap Wald rk F statistic which tests for weak instruments with clustered standard errors. For columns (6) and (7), the mediation was performed "by hand" using standard 2SLS procedures, such that the likelihood of a lawsuit only appears in the second stage in (7), and not in the first stage. One limitation is that the standard errors in (6) and (7) are too small because they do not account for the uncertainty of the predicted effects from the first stage.

## Appendix A. Additional Figures and Tables

Table A1. NCA Policies by State (as provided in the information experiment)

Score		
<i>Panel A. Handling of Overbroad Covenants</i>		
1	Rewrite unreasonably overbroad non-competes terms to make the terms reasonable and then enforce the revised agreement	Alabama, Alaska, Colorado, Connecticut, Delaware, DC, Florida, Georgia, Hawaii, Idaho, Illinois, Iowa, Kansas, Kentucky, Maine, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Nevada, New Hampshire, New Jersey, New York, Ohio, Oregon, Pennsylvania, South Dakota, Tennessee, Texas, Washington, West Virginia, Wyoming
0.5	Remove unreasonably overbroad terms from the noncompete contract, but enforce the rest	Arizona, Indiana, Louisiana, Maryland, Montana, North Carolina, Rhode Island
0	Not enforce a noncompete if <i>any</i> part of the contract is unreasonably overbroad	Arkansas, Nebraska, South Carolina, Virginia, Wisconsin
<i>Panel B. Enforced if Worker is Fired Without Cause?</i>		
1	Enforce the noncompetes of workers who are fired from their jobs without cause	Alabama, Connecticut, Delaware, Georgia, Idaho, Illinois, Indiana, Iowa, Kansas, Louisiana, Maine, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, New Jersey, New York, North Carolina, Ohio, Pennsylvania, South Dakota, Texas, Utah, Vermont, Virginia, Washington, Wyoming
0	Enforce <i>only</i> the noncompetes of workers who either voluntarily leave or are fired for cause (not enforced if fired without cause)	DC, Maryland, Montana
<i>Panel C. Enforcement Dependent on Consideration?</i>		
1	Enforce a worker's noncompete even if the worker <i>only</i> received continued employment in exchange for signing	Alabama, Arizona, Arkansas, Colorado, Connecticut, Delaware, DC, Florida, Georgia, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maine, Maryland, Massachusetts, Michigan, Mississippi, Missouri, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, New York, Ohio, South Dakota, Tennessee, Utah, Vermont
0	Only enforce the noncompete of a worker who is given additional benefits (such as additional compensation, training, or other benefits) <i>beyond</i> continued employment in exchange for signing a non-compete	Minnesota, Montana, North Carolina, Oregon, Pennsylvania, South Carolina, Texas, Washington, West Virginia, Wisconsin, Wyoming

0	Not enforce a signed noncompete if the employer did not notify the employee at least 14 days before the start of employment that a noncompete would be requested.	Oregon
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*Panel D. Exemptions*

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1	Enforce <i>only</i> the noncompetes of executive or management-level employees and related professional staff	Colorado
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0	Either will not enforce or are unlikely to enforce noncompetes for physicians	Arizona, Colorado, Delaware, Illinois, Massachusetts, Tennessee, Texas
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0	Not enforce noncompetes for employees who leave to join or start a competing business, regardless of the circumstances	California, North Dakota
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0	Not enforce noncompetes for employees leaving to join or start a competing business, but will restrict the ability of the employee to directly solicit clients from his/her former employer	Oklahoma
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Notes: The language for the policies is identical to those used in the information experiment. This classification is derived from the contents of the classification described in Beck (2014). See Online Appendix OB for more details. The overall measure of enforceability adds each score for each state and adds an additional 1 for states that enforce noncompetes at all. As a result, the maximum score a state can receive is four. We normalize this measure by dividing by the maximum score for each state, such that the non-enforcing states (or state-occupation combinations) receive a score of 0 and the highest enforcing states receive a score of 1.

Table A2. Enforceability Beliefs by Noncompete Status, Education, and Categorical Beliefs

DV: Probability Noncompete Enforced	(1)	(2)	(3)	(4)	(5)	(6)
	Categorical Beliefs		Noncompete Status		Education	
Constant	0.207*** (0.042)	0.682 (0.592)	0.462*** (0.011)	0.456** (0.201)	0.491*** (0.026)	0.792 (0.569)
1(Don't know if noncompete enforceable)	0.065 (0.043)	0.082** (0.033)				
1(Believe noncompete is enforceable)	0.274*** (0.048)	0.296*** (0.032)				
1(Medium Enforceability)			-0.059*** (0.020)	-0.079*** (0.023)	-0.073 (0.052)	-0.077 (0.047)
1(High Enforceability)			-0.021 (0.027)	-0.038 (0.029)	-0.060 (0.042)	-0.057 (0.046)
1(No Noncompete)			0.009 (0.018)	0.008 (0.021)		
1(Maybe Noncompete)			-0.099*** (0.014)	-0.107*** (0.019)		
1(Medium Enforceability)*1(No Noncompete)			0.062** (0.030)	0.074** (0.029)		
1(Medium Enforceability)*1(Maybe Noncompete)			0.058* (0.031)	0.070** (0.028)		
1(High Enforceability)*1(No Noncompete)			0.039 (0.031)	0.048 (0.030)		
1(High Enforceability)*1(Maybe Noncompete)			0.018 (0.031)	0.028 (0.030)		
1(Bachelor's Degree)		-0.019 (0.025)		-0.028*** (0.010)	-0.041 (0.049)	-0.035 (0.053)
1(Above Bachelor's Degree)		-0.010 (0.029)		-0.067*** (0.013)	-0.045 (0.049)	-0.045 (0.046)
1(Medium Enforceability)*1(Bachelor's)					0.003 (0.069)	-0.026 (0.062)
1(Medium Enforceability)*1(Above Bachelor's)					0.024 (0.100)	-0.005 (0.084)
1(High Enforceability)*1(Bachelor's)					0.068 (0.065)	0.034 (0.056)
1(High Enforceability)*1(Above Bachelor's)					0.045 (0.074)	0.010 (0.069)
Controls	No	Yes	No	Yes	No	Yes
Observations	1,747	1,747	11,505	11,505	1,747	1,747
Mean R-Squared	0.0662	0.155	0.0221	0.0483	0.00571	0.0967

Notes: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors in parentheses, clustered at the state level. Basic controls include gender, education, race, a third degree polynomial in age, the class of the worker (e.g., for-profit), occupation (2 digit SOC), industry (2 digit NAICS), how the worker is paid (e.g., hourly vs. salary), hours worked per week, weeks worked per year, the interaction of hours and weeks worked, firm size, whether the firm has multiple establishments, and the log of number of.

Table A3. Search Effort and the Receipt of Job Offers from Competitors

Dependent Variable: Model: OLS	(1) Search Effort To- wards Competitor	(2)	(3) P(Enforce)	(4)
Constant	2.759*** (0.131)	-0.442 (3.988)	0.459*** (0.011)	0.766 (0.548)
1(Medium Enforceability)	-0.324 (0.307)	0.343 (0.253)	-0.079*** (0.018)	-0.090*** (0.024)
1(High Enforceability)	-0.276 (0.343)	0.055 (0.352)	-0.022 (0.024)	-0.031 (0.030)
1(Informed)	1.535*** (0.350)	1.265** (0.476)		
1(Medium Enforceability)*1(Informed)	-1.651*** (0.486)	-1.696*** (0.486)		
1(High Enforceability)*1(Informed)	-1.359*** (0.466)	-1.195** (0.589)		
1(Received Competitor offer)			0.018 (0.067)	0.084 (0.062)
1(Medium Enforceability)*1(Competitor offer)			0.113 (0.082)	0.030 (0.084)
1(High Enforceability)*1(Competitor offer)			0.010 (0.091)	-0.054 (0.086)
Controls	No	Yes	No	Yes
Observations	1,747	1,747	1,747	1,747
Mean of Dependent Variable	2.573	2.573	0.428	0.428
R-Squared	0.0142	0.178	0.0118	0.102

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Robust standard errors in parentheses, clustered at the state level. Basic controls include gender, education, race, a third degree polynomial in age, the class of the worker (e.g., for-profit), occupation (2 digit SOC), industry (2 digit NAICS), how the worker is paid (e.g., hourly vs. salary), hours worked per week, weeks worked per year, the interaction of hours and weeks worked, firm size, whether the firm has multiple establishments, and the log of number of establishments in the worker's county-industry.

Table A4. Reminders and Lawsuits

Dependent Variable: Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1(Employer reminds about noncompete)		P(Enforce)		1(Employee aware of firm suing re: noncompete)		P(Enforce)	
Constant	0.591*** (0.067)	3.683*** (1.248)	0.383*** (0.078)	2.077 (1.400)	0.208*** (0.031)	-0.670 (0.471)	0.415*** (0.008)	0.874 (0.557)
1(Medium Enforceability)	-0.239** (0.093)	-0.398*** (0.113)	0.048 (0.094)	-0.224** (0.084)	0.022 (0.040)	0.042 (0.042)	-0.043 (0.029)	-0.063** (0.029)
1(High Enforceability)	-0.242** (0.092)	-0.377*** (0.088)	0.010 (0.123)	-0.180** (0.087)	-0.003 (0.034)	0.005 (0.042)	-0.010 (0.019)	-0.018 (0.027)
1(Employer reminds about noncompete)			0.331*** (0.088)	0.140 (0.098)				
1(Medium Enforceability)*1(Noncompete reminder)			-0.074 (0.123)	0.248** (0.115)				
1(High Enforceability)*1(Noncompete reminder)			-0.052 (0.196)	0.169 (0.130)				
1(Aware employer sued re: Noncompete)							0.224*** (0.045)	0.280*** (0.041)
1(Medium Enforceability)*1(Aware employer sued)							-0.092 (0.080)	-0.142** (0.060)
1(High Enforceability)*1(Aware employer sued)							-0.050 (0.086)	-0.119 (0.094)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	237	237	237	237	1,747	1,747	1,747	1,747
Mean of Dependent Variable	0.392	0.392	0.519	0.519	0.216	0.216	0.428	0.428
R-Squared	0.0343	0.522	0.151	0.601	0.001	0.141	0.038	0.129

Notes: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses, clustered at the state level. Sample is limited to noncompete signers. Basic controls include gender, education, race, a third degree polynomial in age, the class of the worker (e.g., for-profit), occupation (2 digit SOC), industry (2 digit NAICS), how the worker is paid (e.g., hourly vs. salary), hours worked per week, weeks worked per year, the interaction of hours and weeks worked, firm size, whether the firm has multiple establishments, and the log of number of establishments in the worker's county-industry

Table A5. Information Experiment and Post Experiment Beliefs About Enforceability

Model: OLS	(1)	(2)	(3)	(4)
Dependent Variable	Post Experiment Beliefs P(Enforce)			
Constant	0.418*** (0.040)	0.619 (0.384)	0.101*** (0.018)	-0.015 (0.307)
1(Medium Enforceability)	0.026 (0.047)	-0.011 (0.047)	0.051 (0.040)	0.028 (0.051)
1(High Enforceability)	0.045 (0.049)	0.004 (0.053)	0.076* (0.039)	0.058* (0.031)
1(Information)	-0.215*** (0.032)	-0.216*** (0.034)	0.068** (0.026)	0.082** (0.038)
1(Medium Enforceability)*1(Information)	0.202*** (0.037)	0.177*** (0.039)	0.041 (0.075)	0.020 (0.084)
1(High Enforceability)*1(Information)	0.214*** (0.045)	0.219*** (0.051)	0.022 (0.070)	-0.002 (0.058)
1(P(Enforce≥50))			0.665*** (0.032)	0.691*** (0.044)
1(P(Enforce≥50))*1(Medium Enforceability)			-0.109* (0.055)	-0.132* (0.076)
1(P(Enforce≥50))*1(High Enforceability)			-0.142** (0.059)	-0.169*** (0.063)
1(P(Enforce≥50))*1(Information)			-0.604*** (0.046)	-0.632*** (0.066)
1(P(Enforce≥50))*1(Medium Enforceability)*1(Information)			0.380*** (0.109)	0.422*** (0.126)
1(P(Enforce≥50))*1(High Enforceability)*1(Information)			0.437*** (0.096)	0.471*** (0.089)
Controls	No	Yes	No	Yes
Observations	1,747	1,747	1,747	1,747
Mean R-Squared	0.0394	0.122	0.400	0.460
Mean of Dependent Variable	0.425	0.425	0.425	0.425

Notes: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses, clustered at the state level. Sample includes only noncompete signers. 1(P(Enforce≥50)) is based on the pre-experiment measure. Basic controls include gender, education, race, a third degree polynomial in age, the class of the worker (e.g., for-profit), occupation (2 digit SOC), industry (2 digit NAICS), how the worker is paid (e.g., hourly vs. salary), hours worked per week, weeks worked per year, the interaction of hours and weeks worked, firm size, whether the firm has multiple establishments, and the log of number of establishments in the worker's county-industry.

Table A6. Information Experiment and Noncompetes as Factor in Moving to a Competitor

Model: OLS	(1)	(2)	(3)	(4)
Dependent Variable	Post Experiment 1(Noncompete be a factor in moving)			
Constant	0.467*** (0.054)	2.721*** (0.665)	0.194* (0.106)	1.871*** (0.560)
1(Medium Enforceability)	-0.015 (0.087)	-0.050 (0.079)	-0.089 (0.124)	-0.107 (0.093)
1(High Enforceability)	-0.015 (0.066)	-0.083 (0.065)	-0.057 (0.112)	-0.116 (0.092)
1(Information)	-0.251*** (0.046)	-0.252*** (0.064)	-0.105 (0.104)	-0.126 (0.103)
1(Medium Enforceability)*1(Information)	0.205** (0.098)	0.164** (0.077)	0.170 (0.121)	0.121 (0.109)
1(High Enforceability)*1(Information)	0.224*** (0.063)	0.256*** (0.081)	0.130 (0.130)	0.144 (0.124)
1(Noncompete Factor in Moving)			0.629*** (0.089)	0.627*** (0.071)
1(Noncompete Factor in Moving)*1(Medium Enforceability)			0.081 (0.104)	0.054 (0.086)
1(Noncompete Factor in Moving)*1(High Enforceability)			0.004 (0.117)	-0.011 (0.108)
1(Noncompete Factor in Moving)*1(Information)			-0.304*** (0.104)	-0.230*** (0.080)
1(Noncompete Factor in Moving)*1(Medium Enforceability)*1(Information)			0.116 (0.117)	0.115 (0.088)
1(Noncompete Factor in Moving)*1(High Enforceability)*1(Information)			0.229 (0.160)	0.204 (0.138)
Controls	No	Yes	No	Yes
Observations	1,747	1,747	1,747	1,747
Mean R-Squared	0.0185	0.150	0.372	0.464
Mean of Dependent Variable	0.415	0.415	0.415	0.415

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Robust standard errors in parentheses, clustered at the state level. Sample includes only noncompete signers. 1(P(Enforce $\geq$ 50)) is based on the pre-experiment measure. Basic controls include gender, education, race, a third degree polynomial in age, the class of the worker (e.g., for-profit), occupation (2 digit SOC), industry (2 digit NAICS), how the worker is paid (e.g., hourly vs. salary), hours worked per week, weeks worked per year, the interaction of hours and weeks worked, firm size, whether the firm has multiple establishments, and the log of number of establishments in the worker's county-industry.

Table A7. Beliefs about Enforceability and the Importance of Noncompetes

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Suppose that at your current job you receive an offer to perform your same duties in a comparable, competing company. How important are the following factors in determining whether or not you decide to move to the comparable, competing company? (7 Extremely important to 1 Not at all important)</i>						
	Column (4)-(6) Dependent Variable: Importance of _____ minus Importance of the "fact that I signed a CNC"					
Dependent Variable: Model: 2SLS	Importance of "The fact that I signed and agreed to the CNC"	Importance of "The chance my employer would take legal action to try to enforce my CNC"	Importance of "The chance the court will enforce my noncompete"	"The increase in prestige, training, or opportunity to do more excit- ing work"	"The increase in my compensa- tion or other benefits"	"The location of the new job and other life- style benefits"
Instrumented P(Enforce)	2.100*** (0.629)	1.751*** (0.419)	2.925*** (0.557)	-1.344*** (0.340)	-2.023*** (0.457)	-2.591*** (0.725)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Experiment measure of DV	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,747	1,747	1,747	1,747	1,747	1,747
F-Stat	36.55	33.60	34.02	41	40.14	37.72
Mean of DV	4.448	4.525	4.543	1.038	1.566	1.277

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Robust standard errors in parentheses, clustered at the state level. Sample includes only noncompete signers. All models include the main effects of the pre-experiment measure of the particular dependent variable, which are all measured a second time after the experiment (both or those who did and did not receive the information). The instrument for post experiment beliefs is a three-way interaction of pre-experiment beliefs about enforceability, being in a no/low, medium, or high enforceability state, and whether they randomly received information. Controls include the pre-experiment beliefs about enforceability, indicators for enforceability (no/low, medium, high) interacted with a dummy for pre-experiment beliefs being greater than 50% (as in the instrument), and other demographics described in text. The F-Stat reported is the Kleibergen-Paap Wald rk F statistic which tests for weak instruments with clustered standard errors.

Figure A1. Noncompete Enforceability in 2014 for Contiguous United States

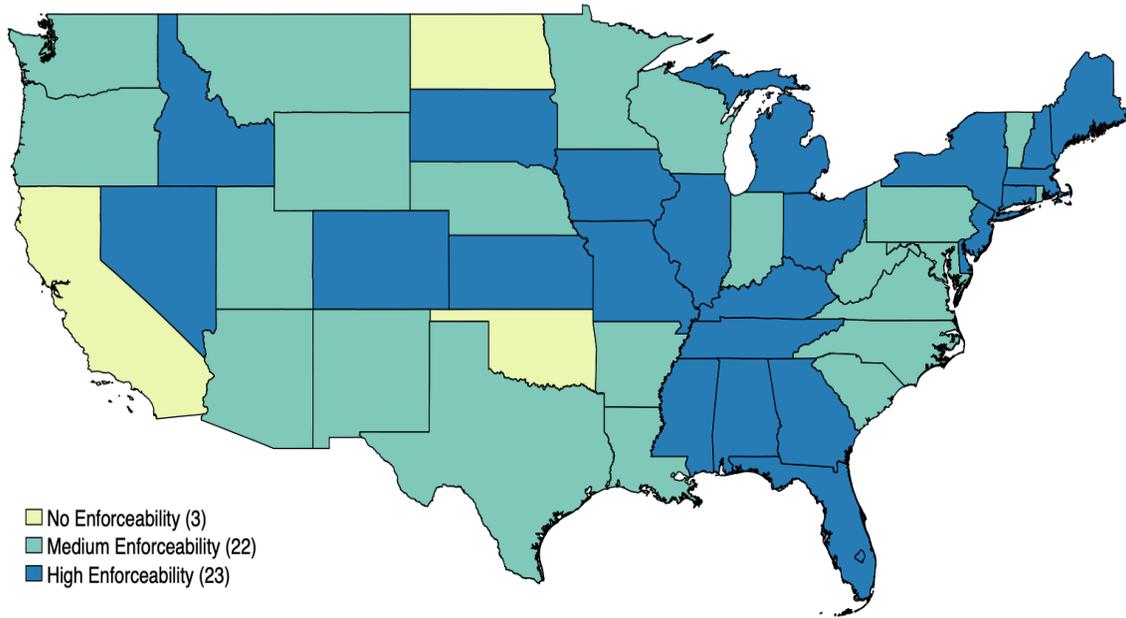


Figure A2. Likelihood of awareness that firm has sued others of noncompete

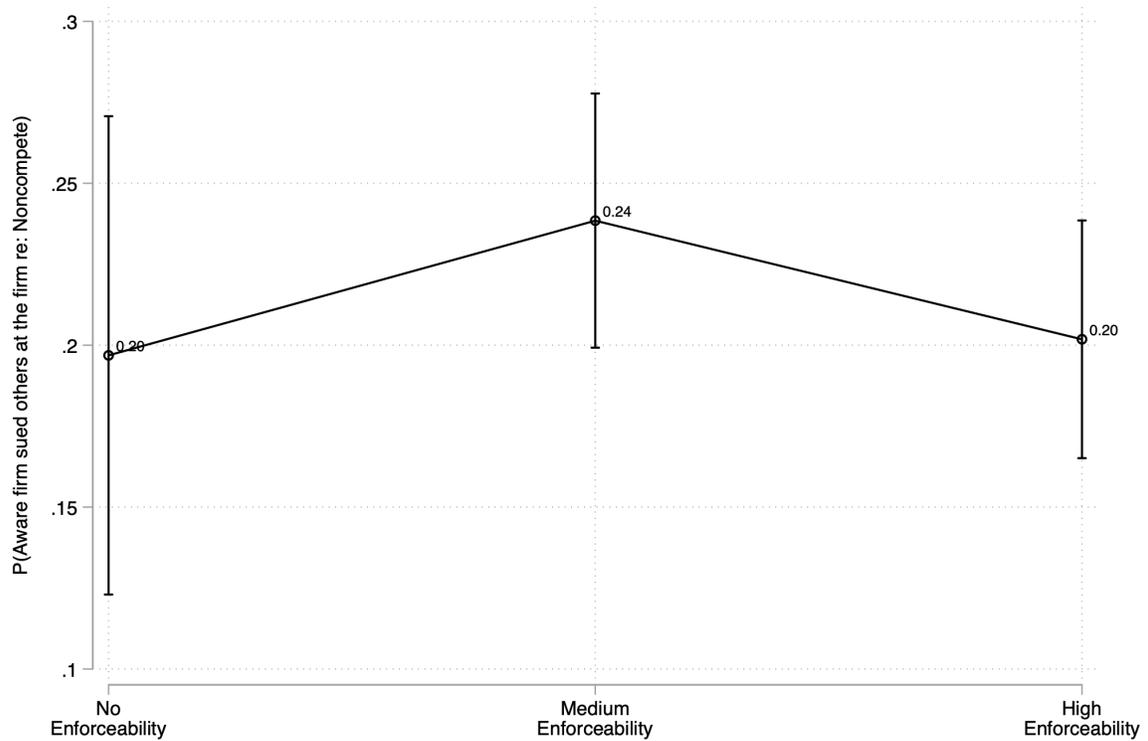


Figure A3. Awareness of Lawsuits and Beliefs about Enforceability

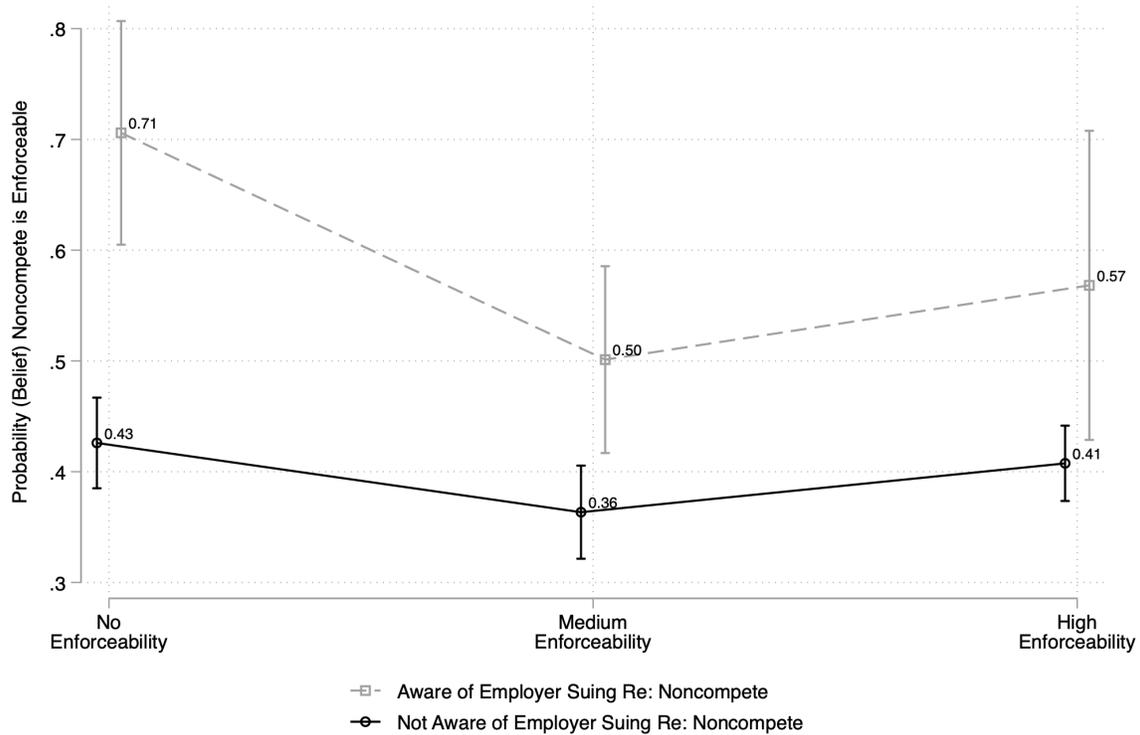


Figure A4. General Information on Noncompete Enforceability, as provided in the experiment

**General Noncompete Enforcement Information:**

- Noncompete policy is conducted at the state level. States have very different noncompete enforcement policies
  - California and North Dakota have bans on enforcing noncompetes, without exceptions.
  - Every other state enforces noncompetes, but under different circumstances.
- To be enforceable, noncompetes must generally satisfy each of the following conditions:
  - (1) The worker must leave the employer in which the noncompete was signed and join or start a **competing** business;
  - and
  - (2) The employee must possess valuable, non-public information, that would cause harm to the initial employer's legitimate business interests if competitors had access to it. This information can come in the form of client relationships, client lists, client specific information, trade secrets, or other types of sensitive information such as business strategy or future plans;
  - and
  - (3) The scope of the noncompete must be reasonable so as not to unduly harm the worker or the public interest.

Figure A5. Specific Information on Noncompete Enforceability for Each State, as Provided in the Experiment

**Noncompete Enforcement Information Specifically For  $\{q://QID406/ChoiceGroup/SelectedChoices\}$**

**Under  $\{q://QID406/ChoiceGroup/SelectedChoices\}$ 's noncompete policy, courts will typically:**

(The bullet points below are information about your state's noncompete policies. This is not a question.)

- ↳ Rewrite unreasonably overbroad noncompetes terms to make the terms reasonable and then enforce the revised agreement**
- ↳ Remove unreasonably overbroad terms from the noncompete contract, but enforce the rest**
- ↳ Not enforce a noncompete if *any* part of the contract is unreasonably overbroad**
- ↳ Enforce the noncompetes of workers who are fired from their jobs without cause**
- ↳ Enforce *only* the noncompetes of workers who either voluntarily leave or are fired for cause (not enforced if fired without cause)**
- ↳ Enforce a worker's noncompete even if the worker *only* received continued employment in exchange for signing**
- ↳ Only enforce the noncompete of a worker who is given additional benefits (such as additional compensation, training, or other benefits) *beyond* continued employment in exchange for signing a noncompete**
- ↳ Enforce *only* the noncompetes of executive or management-level employees and related professional staff**
- ↳ Either will not enforce or are unlikely to enforce noncompetes for physicians**
- ↳ Not enforce noncompetes for employees who leave to join or start a competing business, regardless of the circumstances**
- ↳ Not enforce noncompetes for employees leaving to join or start a competing business, but will restrict the ability of the employee to directly solicit clients from his/her former employer**
- ↳ Not enforce a signed noncompete if the employer did not notify the employee at least 14 days before the start of employment that a noncompete would be requested.**

Figure A6. Heterogeneity in Post-Experiment Beliefs by Pre-Experiment Beliefs, among workers not bound by Noncompetes

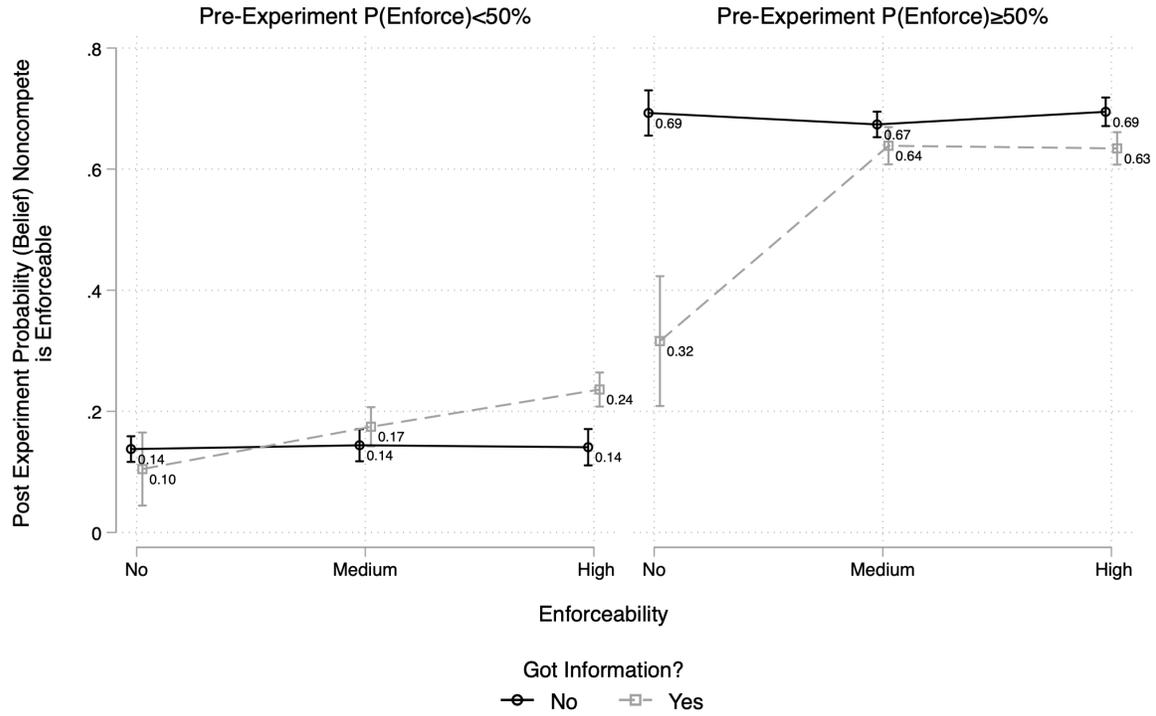


Figure A7. NCA Factor in Starting Business by NCA Enforceability and Treatment Status

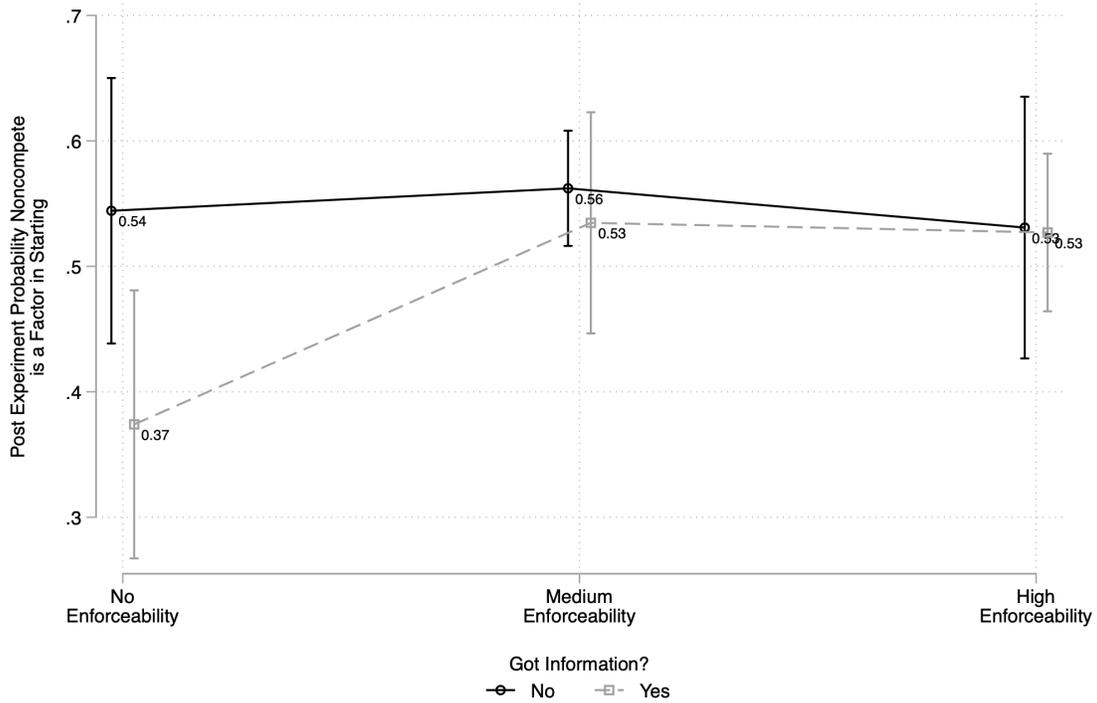


Figure A8. Heterogeneity in NCA Factor in Starting by Pre-Experiment Answer

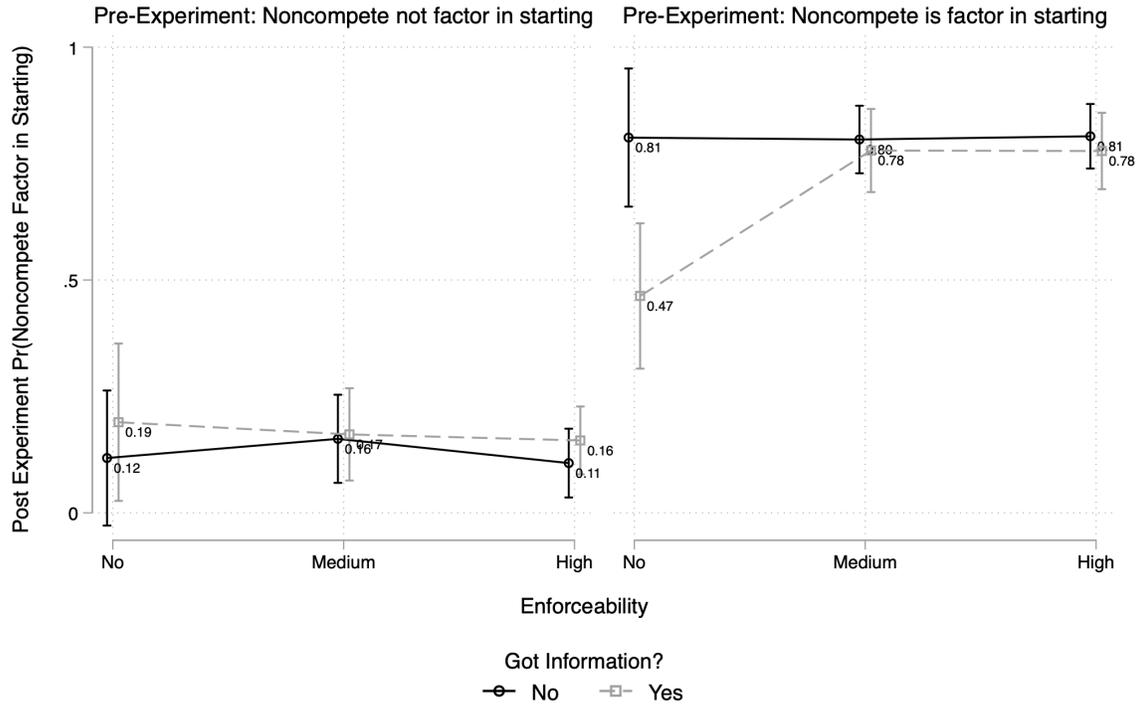


Figure A9. Post Experiment Negotiation over NCAs, by Treatment Status for noncompete signers

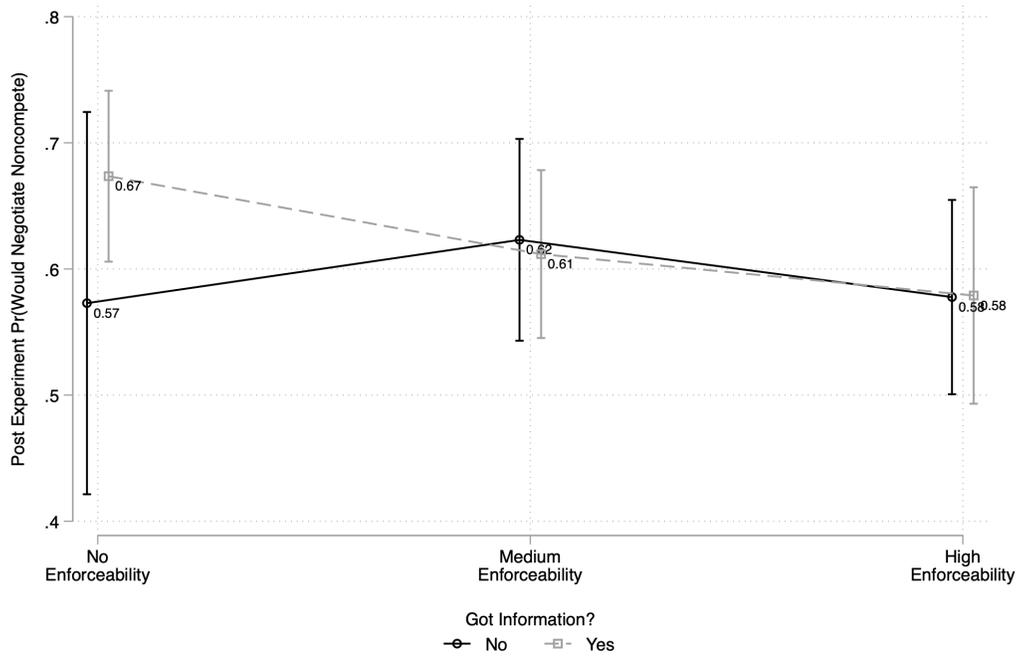


Figure A10. Post Experiment Negotiation over NCAs, by Treatment Status for those not bound by Noncompetes

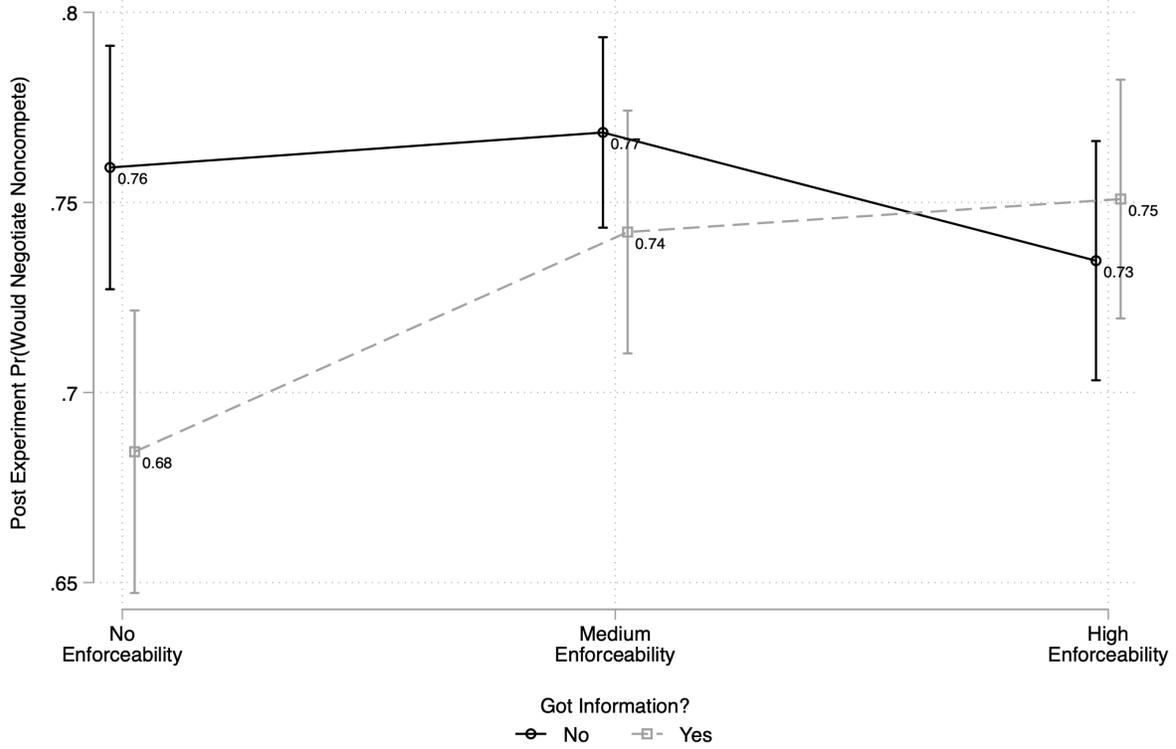
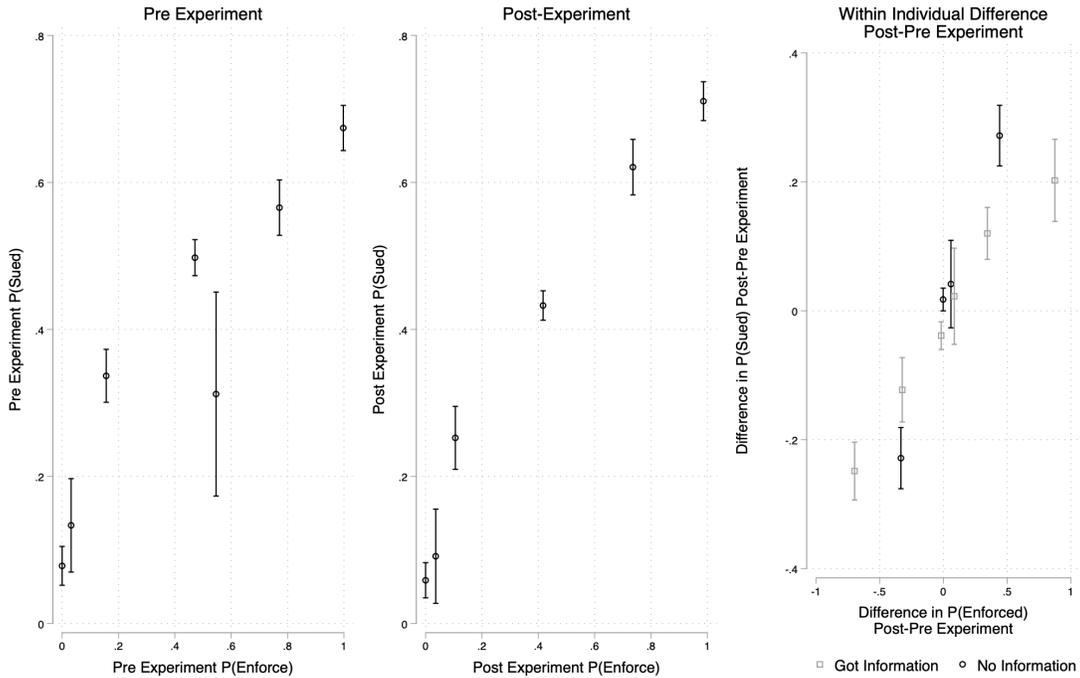


Figure A11. Correlation between Likelihood of Enforceability and Lawsuit



Sample includes only affirmed noncompete signers.

## Online Appendix

### OA. Data Appendix

This article's data derive from a labor force (i.e., employee) survey that we designed and implemented between April and July 2014. Our goal in conducting the survey was to understand the use and effects of covenants not to compete (“noncompetes”), both in a respondent's current job and over the course of a respondent's career. In this appendix, we describe the survey's origin, design, and sampling frame as well as our cleaning and processing of the data to clarify important aspects of this article's analysis. We draw heavily on an earlier technical article that describes these issues in meticulous detail Prescott et al. (2016) and virtually identical content can be found in the appendix of Starr et al. (2020a).

#### OA1. Sampling Frame and Data Collection Methodology

The sampling frame for this study are U.S. labor force participants aged 18--75 years who are working in the private sector (for profit or nonprofit), working for a public health system,<sup>30</sup> or unemployed and looking for work. We excluded individuals who reported being self-employed, government employees, non-U.S. citizens, or out of the labor force. To collect the data, we considered a few possible survey platforms and collection methods, including using RAND's American Life Panel (ALP), conducting a random-digit-dial survey, and adding questions to ongoing established surveys like the NLSY or the PSID. Ultimately, we concluded that our work required a nationally representative sample that was larger than the ALP could provide. We also determined that, to obtain a complete picture of an employee's noncompete experiences, we needed to collect too many different pieces of new information to build on existing surveys. Instead, it made more sense to design and draft a noncompete-specific survey ourselves so that we would be able to ask all of the potentially relevant questions. In the end, we settled on using Qualtrics, a reputable online survey company with access to more than 10 million *verified* panel respondents.<sup>31</sup>

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 years. We chose these particular thresholds either to align the sample with the corresponding sample moments for labor force participants in the 2012 American Community Survey (ACS) or to oversample certain populations of interest.

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<sup>30</sup> We initially considered focusing only on the private sector, but we recognized that public health systems (e.g., those associated with public universities) also use noncompetes extensively.

<sup>31</sup> The difference between verified and unverified survey respondents is important. The use of unverified survey respondents means that there is no external validation of any information the respondent provides (e.g., a Google or Facebook survey), while verified survey respondents have had some information verified by the survey company. We signed up with a number of these companies to see how they vetted individuals who agreed to respond to online surveys. A typical experience involves filling out an intake form and providing fairly detailed demographic information, including a contact number. A day or so after completing the intake form, the applicant receives a phone call from the survey company at the number the applicant provided. On the call, the applicant is asked a series of questions related to the information previously provided on the intake form. Verified respondents are those who are reachable at the phone number supplied and who corroborate the information initially supplied.

Respondents who completed the survey were compensated differently depending on the panel provider: some were paid \$1.50 and entered into prize sweepstakes, others were given tokens or points in online games that they were playing. Respondents took a median time of approximately 28 minutes to complete the survey. Due to the length of the survey, we used three "attention filters" spaced evenly throughout the survey to ensure that respondents were paying attention to the questions. Before we describe the cleaning process for our survey data, we briefly outline the costs and benefits of using online surveys.<sup>32</sup>

## OA2. Costs and Benefits of Online Surveys

Online surveys come with a variety of benefits. Relative to random-digit-dial or in-person surveys, the cost per respondent is orders of magnitude lower and the data-collection time is orders of magnitude faster. The interactive survey interface also allows the survey designer to write complicated, nested questions that are easy for respondents to answer through an online platform. Online surveys also allow individuals to respond at their leisure via their preferred method (e.g., computer, phone, tablet, etc.) from wherever they wish (e.g., work, home, or coffee shop). For these reasons, Reuters, the well-known national polling company, has conducted all of its polling since 2012 online, including its 2016 Presidential election polling.<sup>33</sup>

However, these benefits come at a potentially high cost: a sample of online survey takers may not be representative of the population of interest to researchers or policymakers. There are four sample selection concerns in particular. First, not all people in the U.S. labor force are online. Second, not all of those online register to take surveys. Third, not all of those who register to take surveys receive any particular survey. Fourth, not all of those who are invited to take a survey finish it. Among these sample selection concerns, only the second one is unique to online surveys.<sup>34</sup> With respect to the fourth, alternatives seem unlikely to be better. Kennedy and Hartig (2019) find that survey response to random-digit dialing fell to 6% in 2018, raising the very important question whether a sample resulting from a random-digit-dial survey is still a random sample of the population. We address each of these selection concerns in Prescott et al. (2016) and discuss the second concern in particular in Section A4.

## OA3. Survey Cleaning

Qualtrics fielded the survey and obtained 14,668 completed surveys. When we began to review this initial set of responses, we recognized that individuals with the same IP address may have taken the survey multiple times given there were incentives. To address this, we retained only the first attempt to take the survey from a given IP address and only if that attempt resulted in a completed survey, which produced a sample of 12,369 respondents. We next detected, by inspecting the raw data by hand, that some individuals appeared to have the exact same responses, even for write-in questions, despite the fact that the IP addresses recorded in the survey data were different. To weed these out, we compared individual responses for those with the same gender, age, and race, living in the same

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<sup>32</sup> The information contained in the following sections can be found in Tables 1--18 in Prescott et al. (2016).

<sup>33</sup> See the "About" tab at <http://polling.reuters.com/>.

<sup>34</sup> For example, random-digit-dial surveys miss those without a phone, those who have a phone but do not receive the survey call, and those who receive the call but decline to take the survey.

state and zip code, and working in the same county. We found 665 possible repeat survey takers; the majority of these respondents took the survey with two different panel partners. We reviewed these potential repeat survey takers by hand, and, among those identified as repeat takers from different IP addresses, we kept the first observation and dropped all others, leaving us with a sample of 12,090 respondents.<sup>35</sup>

In the next round of cleaning, we examined individual answers to identify any that were internally inconsistent or unreasonable in substance. In doing so, we developed a “flagging” algorithm that flagged individuals for making mistakes within or across questions, in addition to manually reading through text entry answers. In analyzing these answers, we discovered that some individuals were intentionally noncompliant (e.g., writing curse words or gibberish instead of their job title), while others simply made idiosyncratic errors (e.g., noting that their entire employer was smaller than their establishment—that is, their particular office or factory). We dropped respondents entirely if we deemed them to be intentionally noncompliant because their singular responses indicated that they did not take the survey seriously. This step left us with 11,529 survey responses.<sup>36</sup>

In the last round of cleaning, we began with those who had clean surveys and those who had made some sort of idiosyncratic error. From our flagging algorithm, we determined that 82.2% had no flags and that 16.05% had just one flag (see Table 6 in Prescott et al. 2016). The most common flag was reporting earnings below the minimum wage (often 0), which was true for 1,007 of the 11,529 respondents. The challenge we faced was how to handle these flagged variables. We adopted four approaches: the first was to do nothing—simply, retain all of offending values as they were. The second was to drop all observations with any flag. The third was to replace offending values as missing. The fourth was to impute or otherwise correct offending values. Our preferred method, and the one we use in this article (although our findings are not very sensitive to this choice), is to impute or correct these offending values. Specifically, we “repaired” entries that were marred by idiosyncratic inconsistency by replacing the less reliable, offending value with the value closest to the originally submitted value that would not be inconsistent with the respondent's other answers. When an answer was clearly unreasonable or missing, and there was no workable single imputation procedure, we applied multiple imputation methods to calculate substitute values for the original missing or unreasonable survey entries.

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

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<sup>35</sup> See Tables 3--5 in Prescott et al. (2016) for more details.

<sup>36</sup> See pp.412--14 in Prescott et al. (2016) for more details.

pendently code the 11,529 responses by taking job titles, occupational duties, and employer descriptions and matching them with the appropriate two-digit NAICS and SOC codes.<sup>37</sup> As part of this process, we found that 24 individuals in the sample were self-employed, worked for the government, or were retired, thus reducing our total number of respondents to 11,505.

#### OA4. Sample Selection

As we observe above, there are four primary sample selection concerns with an online survey like ours: (1) not everybody is online; (2) not everybody online signs up for online surveys; (3) not everybody who signs up for online surveys receives a particular survey; and (4) not everybody who receives a survey manages to complete it. We describe these issues in greater detail in Section II.E in Prescott et al. (2016). All survey research must confront issues (1), (3) and (4)---the only unique selection concern for online surveys is (2). The key question is why individuals sign up to take online surveys and whether that reason is associated with their noncompetete status or experiences.<sup>38</sup> To understand why the individuals who responded to our survey agreed to take online surveys, we asked them directly, and their responses were tabulated in Table 13 in Prescott et al. (2016). The two most common reasons individuals report to explain their interest in taking online surveys are that they enjoy the rewards (59%) and sharing their opinions (58%). Only 40% indicated that they wanted money, and only 23% claimed that they needed money. Taking these responses seriously, the crucial selection question is, conditional on observables, whether individuals who like the available rewards or sharing their opinions are less likely to be in jobs that require noncompetetes. We believe it is certainly plausible that there is no such relationship.

A related sample selection concern is that individuals who participate in a survey may for some reason lie or otherwise provide inaccurate information in a systematic way. We designed our cleaning strategy with the explicit goal of weeding out such individuals. But of course in any surveying effort legitimate concerns remain about the validity of the responses of the individuals who remain in the sample. To assuage these concerns, we present in Table A1. the self-described job title, self-described job duties, and self-described industries for 15 randomly selected observations. These randomly selected respondents include a sales rep, a nurse, an analyst, a pizza delivery driver, an optometrist, and a programmer analyst. Reading their job-duty descriptions reveals a striking amount of detail, suggesting not only that these respondents answered the survey's questions carefully but also that they were responding truthfully.

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<sup>37</sup> See p.422 of Prescott et al. (2016) for details.

<sup>38</sup> A look at the population of online survey takers (see Table 12 of Prescott et al. 2016) shows that relative to the average labor force participant they tend to be female and less likely to be in full-time employment.

**Table OA1. Self-Described Job Title, Duties, and Industry for 15 Randomly Selected Respondents**

	<b>Self-Described Job Title</b>	<b>Self-Described Job Duties</b>	<b>Self-Described Industry</b>
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 utility 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

## OA5. Weighting and Imputation

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

With respect to weighting, we considered and compared several candidate approaches,<sup>39</sup> including post-stratification, iterative proportional fitting (also called raking), and propensity score weighting. Details on these methods can be found in Kalton et al. (2003). For each method, we evaluated a variety of potential weighting variables, and then we examined the ability of each weighting scheme to match the distributions of variables within the 2014 American Community Survey (ACS) (see Table 17 in Prescott et al. 2016). Iterative proportional fitting, or raking, clearly performed better than alternatives in matching our data to the distributions of key variables in the ACS.

To assemble our analysis data, we began by using raking to calculate weights for our original nonmissing survey data. Next, we imputed our missing data. Our goal was to impute values for many different variables (see Table 18 in Prescott et al. 2016 for details), some of which were missing because of the cleaning process we describe above in Section A4 and others because we added the relevant question to the survey while the survey was in the field. In addition, as we explain in the article, we also aimed to impute whether the “maybe” individuals are currently or have ever been bound by a noncompete. Because we sought to impute missing values across multiple variables, we employed Stata's chained multiple imputation command, which imputes missing values for all variables in one step. As suggested in Sterne et al. 2009, we incorporated all of the variables that we planned to use in our empirical analyses into our imputation model. Doing otherwise would have produced attenuated estimates.<sup>40</sup> Indeed, a general rule of thumb is that all variables involved in the analysis should be included in the imputation model.

While imputing missing values just one time will allow for unbiased coefficient estimates, the associated standard error estimates will be too small because the predicted values will not convey the uncertainty implicit in those estimates (King et al. 2001). To generate unbiased standard error estimates, Graham et al. (2007) recommend conducting at least 20 imputations when the proportion missing is 30% (relevant for our “maybe” group). We added another 5 to increase power.

The exact mechanics for a given imputation step are as follows: First, we fit a regression model with our initial nonmissing data. Second, we simulated new coefficients based on the posterior distribution of the estimated coefficients and standard errors---this step is what gives us variation across the 25 datasets. Third, we combined these coefficients with the observed values of the covariates for the

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<sup>39</sup> See pp.436--46 in Prescott et al. (2016) for more details.

<sup>40</sup> Dependent variables should be included as controls in the imputation of an independent variable to avoid attenuation in the imputed estimates (Sterne et al. 2009). See also <http://thestatsgeek.com/2015/05/07/including-the-outcome-in-imputation-models-of-covariates/>.

missing observations to generate a predicted value. For continuous variables, we used predictive mean matching in the third step. Specifically, we took the average of the 15 nearest neighbors to the predicted value. For binary variables, we employed a logit model to create the predicted value. We repeated this process 25 times for all missing values, creating 25 separate datasets.

Once we had 25 imputed datasets in hand, we reweighted within each dataset using the raking procedure we discuss above, so that each individual dataset is nationally representative. In Table 2 of Starr et al. (2020), we present a comparison of the distribution of demographics between the 2014 ACS and our weighted and unweighted data. The table shows that the weighted data quite accurately match the distribution of contemporaneous ACS data and that the unweighted data indicate a much more skilled workforce, one that does not align closely with the U.S. labor force. This occurs because we employed quotas to ensure that more than 50% of our sample was composed of respondents with a bachelor's degree.

Estimation of our main analysis via multiple imputation involves running the regression model in question on each individual dataset and then aggregating the 25 different estimates using Rubin's rules, combining the within-imputation variance and the between-imputation variance into our standard error calculations. We note that standard regression statistics, like R-Squared, are not typically reported for regressions conducted with multiple-imputation data because there are 25 distinct estimates of each statistic. To give a rough approximation of fit, we report the mean of our R-Squared estimates.

Online Appendix OB. State Policies According to Beck (2014)

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
AL	Yes. Ala. Code Sec. 8-1-1	Trade Secrets; Customer Relationships	Protectable Interest; Restriction is Reasonably Related to the Interest; Restriction is Reasonable in Time and Space; No Undue Hardship on Employee	Professionals	Yes	Reformation	Yes
AK	Yes	Trade Secrets; Confidential Information; Customer Relationship (where employee was sole contact)	Factors: Limitations in Time and Space; Whether Employee Was Sole Contact with Customer; Employee's Possession of Trade Secrets or Confidential Information; Whether Restriction Eliminates Unfair or Ordinary Competition; Whether the Covenant Stifles Employee's Inherent Skill and Experience; Proportionality of Benefit to Employer and Detriment to Employee; Whether Employee's Sole Means of Support is Barred; Whether Employee's Talent Was Developed During Employment; Whether Forbidden Employment Is Incidental to the Main Employment.	-	Undecided	Reformation	Undecided
AZ	Yes	Trade Secrets; Confidential Information; Customer Relationships	No broader than necessary to protect the employer's legitimate business interest; not unreasonably restrictive; not contrary to public policy; ancillary to another contract.	Broadcasters; maybe Physicians	Yes	Blue Pencil	Undecided

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
AR	Yes	Special Training; Trade Secrets; Confidential Business Information; Customer Lists	Ancillary to Employment Agreement; Protectable Interest; Geographic Reach is not Overly Broad; Reasonable in Time; Not greater than reasonably necessary and does not injure a public interest.	-	Yes	Red Pencil	Undecided
CA	No, except maybe as to trade secrets. Cal. Business & Professions Code sec. 16600	Trade Secrets	Uncertain status as to trade secrets.	-	-	-	-
CO	Yes, as to executive or management employees and professional staff; limited as to rest. Colo. Rev. Stat. sec. 8-2-113.	Trade Secrets; Recovery of Training Expenses for Short-term Employees	Must fall within statutory exception; be reasonable; and be narrowly-tailored.	-	Yes	Reformation	Undecided
CT	Yes.	Trade Secrets; Confidential Information; Customer Relationships	Factors: time; geographic reach; fairness of protection afforded to employer; extent of restraint on employee; extent of interference with public interest.	Broadcasters; Security Guards	Yes, likely	Reformation	Yes
DE	Yes	Trade Secrets; Confidential Information; Customer Relationships	Reasonable in time and geographic reach; protects legitimate economic interests; survives balance of equities.	Physicians	Yes	Reformation	Yes

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
DC	Yes	Trade secrets; confidential knowledge; expert training; fruits of employment	Reasonable in time and geographic area; necessary to protect legitimate business interests; promisee's need outweighs promisor's hardship. [Follows Restatement (Second) of Contracts, secs. 186-88.]	Broadcasters	Likely	Reformation or Blue Pencil	No
FL	Yes. Fla. Stat. Ann. Sec. 542.335	Trade secrets; confidential business information; substantial customer relationships and goodwill; extraordinary or specialized training	Legitimate business interest; reasonably necessary to protect legitimate business interest. [Rebuttal presumptions exist.]	Mediators	Yes	Reformation (mandatory)	Undecided
GA	Yes. Ga. Const., Art. III, Sec. VI, Par. V(c), as amended.	Proprietary Confidential Information and Relationships; Goodwill; Economic Advantage; Time and Monetary Investment in Employee's Skill and Training	Not overbroad in time, space, and scope; interest of individuals in gaining and pursuing a livelihood; commercial concerns in protecting legitimate business interests; public policy.	-	Yes	Reformation	Yes, but it's a factor to be considered.
HI	Yes. Haw. Rev. Stat. sec. 480-4(c)	Trade Secrets; Confidential Information; Customer Contacts	Reasonable in time, space, scope.	-	Undecided	Reformation	Undecided
ID	Yes	Trade Secrets; Confidential Information; Customer Contacts	No broader than necessary to protect the employer's legitimate business interest; reasonable as to covenantor, covenantee, and public; not contrary to public policy.	-	Yes	Reformation	Yes

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
IL	Yes.	Legitimate business interests are based on the totality of the facts and circumstances of the case. Trade secrets, confidential information, and near permanent business relationships are factors.	Ancillary to a valid employment relationship; no greater than required to protect a legitimate business interest; does not impose undue hardship on the employee; not injurious to the public; and reasonable in time, space, and scope. [May require two years of continued employment before any non-compete can be enforced.]	Broadcasters; Government Contractors; Physicians	Yes (if employment continued for sufficient duration)	Reformation	Yes
IN	Yes.	Trade Secrets; Confidential Information; Goodwill; Special Training or Techniques	Clear and specific (not general) restraint must be reasonable in light of the legitimate interests to be protected; reasonableness is measured by totality of interrelationship of the interest, and the time, space, and scope of the restriction, judged by the needs for the restriction, the effect on the employee, and the public interest.	-	Yes	Blue Pencil	Yes
IA	Yes.	Trade Secrets; Goodwill; Specialized Training	Whether the restriction is reasonably necessary to protect the employer's business, unreasonably restrictive (time and space), and prejudicial to the public interest.	Franchisees (where franchisor does not renew)	Yes	Reformation	Yes, but it's a factor to be considered.
KS	Yes.	Trade Secrets; Loss of Clients; Referral Sources; Reputation; Special Training	Protects a legitimate business interest; not undue burden on employee; not injurious to public welfare; reasonable in time and space.	Accountants (limited)	Yes	Reformation	Yes

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
KY	Yes.	Confidential Business Information; Customer Lists; Competition; Employee Raiding; Investment in Training	Reasonable in scope and purpose; reasonableness determined by the time, space, and "charter" of the restriction; no undue hardship; does not interfere with public interest	-	Yes (if long enough and employee resigns)	Reformation	Undecided (but it can be a factor)
LA	Yes. La. Rev. Stat. Ann. Sec. 23:921.	Trade Secrets; Financial Information; Management Techniques; Extensive (Unrecouped Through Employee's Work) Training	No more than two years; specifies the specific geographic reach (by parishes, municipalities, or their respective parts); defines employer's business; strict compliance with statute.	Automobile Salesman; Real Estate Broker's Licensees (procedural requirements)	Yes	Blue Pencil, if allowed by the noncompete	Yes, likely.
ME	Yes	Trade Secrets; Confidential Information; Goodwill	No broader than necessary to protect the employer's legitimate business interest; reasonable as to time, space, and interests to be protected; no undue hardship to employee.	Broadcast Industry (presumption)	Yes	Reformation	Yes, likely.
MD	Yes	Trade Secrets; Routes; Client Lists; Established Customer Relationships; Goodwill; Unique Services	Duration and space no broader than reasonably necessary to protect legitimate interests; no undue hardship to employee or public; ancillary to the employment.	-	Yes	Blue Pencil, but undecided as to whether more flexible	No, likely.
MA	Yes	Trade Secrets; Confidential Information; Goodwill	Narrowly tailored to protect legitimate business interest; limited in time, space, and scope; consonant with public policy; harm to employer outweighs harm to employee.	Broadcasters; Physicians; Nurses; Social Workers; Psychologists	Yes	Reformation	Yes

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
MI	Yes. Mich. Comp. Laws sec. 445.774a.	Trade Secrets; Confidential Business Information; Goodwill	Must have an honest and just purpose and to protect legitimate business interests; reasonable in time, space, and scope or line of business; not injurious to the public.	-	Yes	Reformation	Yes
MN	Yes	Trade Secrets; Confidential Business Information; Goodwill; Prevention of Unfair Competition	No broader than necessary to protect the employer's legitimate business interest; does not impose unnecessary hardship on employee.	-	No	Reformation	Yes
MS	Yes	Trade Secrets; Confidential Business Information; Goodwill; Ability to Succeed in a Competitive Market	Reasonableness and specificity of restriction, primarily, in time and space; hardship to employer and employee; public interest.	-	Yes (though questioned if employee terminated shortly after)	Reformation	Yes
MO	Yes. 28 Mo. Stat. Ann. Sec. 431.202 (related)	Trade Secrets; Confidential Business Information; Customer or Supplier Relationships, Goodwill, or Loyalty; Customer Lists; Protection from Unfair Competition; Stability in the Workforce	Reasonably necessary to protect legitimate interests; reasonable in time and space; not an unreasonable restraint on employee; purpose served; situation of the parties; limits of the restraint; specialization of the business. [Absence of legitimate business interest impacts duration, which can be no more than one year.]	Secretaries (limited); Clerks (limited)	Yes, generally.	Reformation	Yes
MT	No. Mont. Code Ann. Secs. 28-703-05	Likely confidential information and goodwill; may be more broad.	Reasonable in time or space; reasonable protection for employer; does not impose unreasonable burden on the employee or public.	-	Undecided, likely requires additional consideration.	Blue Pencil, likely	No

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
NE	Yes	Trade Secrets; Confidential Information; Goodwill	Reasonably necessary to protect legitimate interests; not unduly harsh or oppressive to employee; not injurious to the public. Considerations include: inequality in bargaining power; risk of loss of customers; extent of participation in securing and retaining customers; good faith of employer; employee's job, training, health, education, and family needs; current employment conditions; need for employee to change his calling or residence; relation of restriction to legitimate interest being protected.	-	Yes	Red Pencil	Undecided
NV	Yes. Nev. Rev. Stat. sec. 613.200	Trade Secrets; Goodwill	Not greater than reasonably necessary to protect the business and goodwill of the employer; no undue hardship on employee. Time and space are considerations for reasonableness.	-	Yes	Reformation	Undecided
NH	Yes. RSA 275:70	Trade Secrets; Confidential Business Information; Goodwill; Employee's Special Influence Over the Employer's Customers	Not greater than necessary to protect the employer's legitimate business interests; no undue or disproportionate hardship to employee; not injurious to public interest; employee must be given a copy of the noncompete in with offer for employment or change in job classification.	-	Yes	Reformation	Undecided

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
NJ	Yes	Trade Secrets; Confidential Business Information; Goodwill in Existing Customers; Preventing Employee from Working with Customer at Lower Cost than Working through Employer	Protects a legitimate business interest; not undue burden on employee; not injurious to the public; not overbroad in time, space, and scope.	In-House Counsel; Psychologists.	Yes	Reformation	Yes
NM	Yes	Maintaining Workforce; Limitation of Competition (but not to stifle competition); Customer Relationships	Reasonable as applied to the employer, employee, and public; not great hardship to employee in exchange for small benefits to employer.	-	Yes, likely	Undecided	Undecided
NY	Yes	Trade Secrets; Confidential Information; Goodwill; On-Air Persona of Broadcasters; Employee's Unique or Extraordinary Services	Necessary to protect legitimate business interest; reasonable in time and space; not harmful to general public; not unreasonably burdensome to the employee.	-	Yes	Reformation	Yes, with exceptions.
NC	Yes. N.C. Gen. Stat. sec. 75-4; 21 N.C. Admin. Code sec. 29.0502(e)(5) (limitations on locksmiths)	Trade Secrets; Confidential Business Information; Goodwill	In writing; part of an employment contract; reasonably necessary to protect legitimate business interest; reasonable in time and space; not against public policy.	-	No	Blue Pencil	Yes, likely.
ND	No. N.D. Cent. Code sec. 9-08-06	-	-	-	-	-	-

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
OH	Yes	Trade Secrets; Confidential Information; Customer Relationships; Prevention of the Use of Proprietary Customer Information to Solicit Customers	Not greater than necessary to protect the employer's legitimate business interests; no undue hardship to employee; not injurious to public interest. Considerations: absence or presence of limitations as to time and space; whether employee is sole contact with customer; employee's possession of trade secrets or confidential information; purpose of restriction (elimination of unfair competition vs. ordinary competition and whether seeks to stifle employee's inherent skill and experience); proportionality of benefit to employer as compared to the detriment to the employee; other means of support for employee; when employee's talent was developed; whether forbidden employment is merely incidental to the main employment.	-	Yes	Reformation	Yes
OK	No. Okla Stat. ti. 15, sec. 219A	-	-	-	-	-	-

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
OR	Yes. Or. Rev. Stat. sec. 653.295	Trade Secrets; Confidential Business or Professional Information; Investment in Certain On-Air Broadcasters; Customer Contacts and Goodwill	Noncompete provided at least two weeks before employment or with bona fide advancement; employee meets minimum compensation threshold; no longer than two years; restricted in time or space; application of restriction should afford only a fair protection of the employer's interests; must not interfere with public interest. [Qualifying garden leave clauses are enforceable.]	-	No.	Reformation	Undecided
PA	Yes	Trade Secrets; Confidential Information; Goodwill; Investment in Specialized Training; Unique or Extraordinary Skills	Ancillary to employment relation or other transaction; reasonably necessary to protect the employer's legitimate interests; reasonable in time and space.	-	No	Reformation	Yes, but it's a factor to be considered.
RI	Yes	Trade Secrets; Confidential Information; Customer Lists; Goodwill; Special Training or Skills	Reasonable in light of protectable interests.	-	Undecided	Blue Pencil, but may allow Reformation	Undecided
SC	Yes	Business and Customer Contacts; Existing Employees; Existing Payroll Deduction Accounts.	Necessary to protect legitimate business interest; reasonably limited in time and space; not unduly harsh and oppressive to employee's efforts to earn a living; reasonable from standpoint of public policy.	-	No	Red Pencil, likely. (SC S.Ct rejected blue pencil doctrine by name, but case involved reformation.)	Undecided

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
SD	Yes. S.D. Codified Laws sec. 53-9-8, <i>et seq.</i>	Trade Secrets; Protection from Unfair Competition; Existing Customers	Restriction is in the same business or profession as that carried on by employer and does not exceed two years and in a specified geographic area; reasonableness in time, space, and scope is a factor only in certain circumstances.	-	Yes	Reformation, likely.	Yes, but it's a factor to be considered.
TN	Yes	Trade Secrets; Confidential Information; Retention of Existing Customers; Investment in Training or Enhancing the Employee's Skill and Experience	Restriction must be reasonable in time and space and necessary to protect legitimate interest; public interest no adversely affected; no undue hardship to the employee.	Physicians (in certain circumstances).	Yes (if employment continued for appreciably long period)	Reformation	Undecided
TX	Yes. Tex. Bus. & Com. Code secs. 15.50-.52	Trade Secrets; Confidential or Proprietary Information; Goodwill; Special Training or Knowledge Acquired During Employment;	Ancillary to an otherwise enforceable agreement; reasonable in time, space, and scope; does not impose a greater restraint than necessary to protect legitimate business interest. <i>*In December 2011, the Texas Supreme Court withdrew its June 2011 landmark decision, but still eliminated the requirement that the consideration given by the employer in exchange for the non-compete must give rise to the interest protected by the non-compete, and held that the consideration for the non-compete agreement must be reasonably related to the company's interest sought to be protected.</i>	Physicians (in certain circumstances).	No	Reformation (mandatory)	Yes

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
UT	Yes	Trade Secrets; Goodwill; Extraordinary Investment in Training or Education	No bad faith in the negotiations; necessary to protect legitimate business interest; reasonable in time, space, and scope; consideration of hardship.	-	Yes	Undecided	Yes
VT	Yes	Proprietary Confidential Information; Goodwill; Relationships with Customers; Investments in Special Training	Necessary to protect legitimate business interest; not unnecessarily restrictive to employee; limited in time, space, and/or industry; not contrary to public policy.	Beauticians and Cosmetologists (by their school)	Yes	Undecided	Yes, but it's a factor to be considered.
VA	Yes	Trade Secrets; Confidential Information; Knowledge of Methods of Operation; Protection from Detrimental Competition; Customer Contacts	No broader than necessary to protect the employer's legitimate business interest; reasonable in time, space, and scope; not unduly harsh in curtailing employee's ability to earn a living; reasonable in terms of public policy.	-	Yes	Red Pencil	Yes
WA	Yes	Customer Information and Contacts; Goodwill	Restriction is necessary to protect employer's business or goodwill; restriction is no greater than reasonably necessary to secure employer's business or goodwill; reasonable in time and space; injury to public does not outweigh benefit to employer.	Broadcasters (under certain circumstances)	No	Reformation	Yes, likely.

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
WV	Yes	Trade Secrets; Confidential or Unique Information; Customer Lists; Direct Investment in Employee's Skills; Goodwill	Ancillary to a lawful contract; not greater than reasonably necessary to protect legitimate business interest; reasonable in time and space; no undue hardship on employee; not injurious to public.	-	No, likely.	Reformation	Undecided
WI	Yes. Wis. Stat. Ann. Sec. 103.465	Trade Secrets; Confidential Business Information; Customer Relationships.	Necessary to protect legitimate business interest; reasonable in time and space; not harsh or oppressive to the employee; not contrary to public policy.	-	No, likely.	All or nothing. But, recent case law may suggest a judicial move toward a more tolerant approach. See <i>Star Direct, Inc. v. Dal Pra</i> , 767 N.W.2d 898 (Wis. 2009).	Undecided
WY	Yes.	Trade Secrets; Confidential Information; Special Influence of Employee Over Customers to the Extent Gained During Employment	Restraint must be ancillary to otherwise valid agreement and fair; no greater than necessary to protect legitimate business interests; reasonable in time and space; no undue hardship on employee; employer's need outweighs harm to employee and public; not injurious to public.	-	No	Reformation	Yes, likely.

State	Permitted	Protectable / Legitimate Interests	Standards	Exemptions	Continued Employment is Sufficient Consideration	Reformation Blue Pencil Red Pencil	Enforceable Against Discharged Employees
		Customer lists are frequently considered trade secrets or confidential information. Some states, however, separately identify them as protectable interests.	Consideration for the noncompete is always a requirement. That requirement is not typically an issue when the agreement is entered into at the inception of an employment relationship.	Attorneys and certain persons in the financial services industry are subject to industry regulations not addressed in this chart.	The continued employment issue addresses only at-will employment relationships.	Reformation is also sometimes called "Judicial Modification," the "Rule of Reasonableness," the "Reasonable Alteration Approach," or the "Partial- Enforcement" rule. Red Pencil is also sometimes called the "All or Nothing" rule.	Assumes no breach or bad faith by the employer.