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## Subjective Beliefs about Contract Enforceability

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### Abstract

This article assesses the content, role, and adaptability of subjective beliefs about contract enforceability in the context of postemployment covenants not to compete (“noncompetes”). We show that employees tend to believe that their noncompetes are enforceable, even when they are not. We provide evidence for both supply- and demand-side stories that explain employees’ persistently inaccurate beliefs. Moreover, we show that believing that unenforceable noncompetes are enforceable likely 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. Finally, we use an information experiment to inform employees about the enforceability of their noncompete. While this information matters for employee beliefs and prospective behavior, it does not appear to eliminate an unenforceable noncompete as a factor in the decision to take a new job. We discuss the implications of our results for the policy debate regarding the enforceability of noncompetes.

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## 1. INTRODUCTION

How individuals behave in response to law depends on their particular and sometimes mistaken beliefs about the law’s content, including the probability of enforcement. Under many circumstances, individuals are likely to have accurate beliefs about the law, such as in economic settings where the stakes are high and information is easy to access. Contracting may be one such setting. In other environments, however, baseline access to facts can be limited, and information gathering can be costly. Moreover, we know that a counterparty can sometimes benefit by investing in maintaining an individual’s specific mistaken beliefs (Gabaix and Laibson 2006). For this reason, the extent to which individual beliefs are inaccurate, the reasons they are inaccurate, and the implications of these inaccuracies, especially when they are systematic, remains an important area of research (Salop and Stiglitz 1977, Kim 1997, Wilkinson-Ryan 2017, Stantcheva 2020, Jäger et al. 2022). When persistently mistaken beliefs relate to the content of policies or law and are socially costly, interventions designed to disrupt such an equilibrium may be able to change behavior and improve welfare (Chetty 2015).

In this article, we consider beliefs regarding the legal enforceability of covenants not to compete (“noncompetes”) and the role such beliefs may play in explaining employee behavior. Noncompetes are employment provisions that prohibit departing employees from joining or starting a competitor under certain conditions. Our work is motivated by two recent findings that point to the possible influence of mistaken beliefs in this domain. First, employers use noncompetes heavily in states that explicitly refuse to enforce them (Starr et al. 2021, Colvin and Shierholz 2019). Second, noncompetes appear to influence employee mobility even in states where such provisions are unenforceable (Starr et al. 2020). While there are several reasons why employers might use and employees might comply with noncompetes even when employees *know* that a court will not enforce them (e.g., reputational harm or disutility from breaking a “promise”), one explanation for these results is that employees have mistaken beliefs about noncompete policies and that these beliefs matter to their choices.<sup>1</sup>

The possibility that employees are systematically uninformed or perversely misinformed about the law has important implications for the interpretation of existing noncompete research. Nearly all studies of the consequences of noncompetes leverage state-level policy changes to identify the effects of these provisions, essentially assuming that employees and employers are aware of, understand, and react to such policy changes.<sup>2</sup> Policy advocates also almost invariably (if implicitly) assume that em-

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<sup>1</sup> Catherine Fisk (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.” Another possibility is that employees are well informed about the law but other terms in their contract make any noncompete de facto enforceable (Sanga 2018).

<sup>2</sup> Bishara and Starr (2016) review this literature on “enforceability.” See, e.g., Garmaise (2009), Marx et al. (2009), Samila and Sorenson (2011), Marx et al. (2015), Starr (2019), Kang and Fleming (2020), Balasubramanian et al. (2022),

employees respond rationally to—or at least with awareness of—existing law when navigating noncompete-related choices. In fact, one common starting point has been the view that enforceable noncompetes must be beneficial to both employees and employers (Rubin and Shedd 1981, Posner et al. 2004) because otherwise they would not agree to such provisions. And yet the potential consequences of assuming that employees understand the legal ramifications of their noncompetes are significant. For example, mistaken beliefs about unenforceable noncompetes can be welfare reducing when they inhibit employees from moving to jobs in which they would be more productive.<sup>3</sup> Also, from a policy perspective, simply prohibiting court enforcement of such clauses—the traditional reform proposal—is unlikely to be effective if the *in terrorem* power of noncompetes remains available to employers notwithstanding any such enforcement “ban” (Starr et al. 2020).<sup>4</sup>

Our study uses 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 and to identify the causal effects of such beliefs on prospective decisions.<sup>5</sup> We document that employees tend to believe their noncompetes are enforceable regardless of actual noncompete enforceability. Specifically, 70% of employees with unenforceable noncompetes mistakenly believe their noncompetes are enforceable. Moreover, we find that subjective beliefs about the probability that a court will enforce a noncompete, conditional on an employer bringing a lawsuit, are not even positively correlated with actual enforceability. Surprisingly, and in contrast to the prevailing assumption, better-educated employees also appear largely misinformed about enforceability (Friedman 1991, Callahan 1985). Our data offer support for both supply- and demand-side hypotheses that might explain these persistently mistaken beliefs. First, individuals who mistakenly believe their noncompete to be enforceable are less likely to search for employment with a competitor, reducing their

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Lipsitz and Starr (2022), and Young (2020). It is also likely that prior research pays scant attention to beliefs about noncompete enforceability because data on employee beliefs are difficult to obtain.

<sup>3</sup> While it is beyond the scope of this paper to assess the welfare consequences of noncompetes and noncompete enforceability generally, it is worth noting that, to the extent noncompete efficiency benefits—including greater investment in or the development of valuable information—*depend* on a court enforcing such agreements (Rubin and Shedd 1981), unenforceable noncompetes are unlikely to lead to such investments in the first place. This concern dovetails with research that finds that noncompete *enforceability* generates training and investment benefits (Starr 2019, Starr et al. 2021, Jeffers 2019). More broadly, recent empirical work has identified significant negative externalities associated with noncompetes (Starr, Frake, and Agarwal 2019, Johnson, Lavetti, and Lipsitz 2020), implying that the use and enforcement of noncompetes is not merely a transfer of rights that affects only the contracting parties.

<sup>4</sup> Somewhat ironically, proponents of banning noncompete *enforcement* often make their case by alluding to the lack of sophistication or bargaining power on the part of employees subject to such provisions. 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 law when they are uninformed about the content or implications of the noncompete clause contained in their employment contract.

<sup>5</sup> We use data from the 2014 Noncompete Survey Project, the first nationally representative survey of noncompetes (Prescott et al. 2016). In previous work using these data, we describe the incidence of noncompetes across the U.S. labor force (Starr et al. 2021), how noncompetes relate to mobility (Starr et al. 2020), and how noncompetes create externalities even among those not bound by such agreements (Starr, Frake, and Agarwal 2018).

access to potentially correcting information. Second, we find that employees who do interact with competitors are actually *more* likely to believe their noncompete is enforceable, in part because individuals in states that do not enforce noncompetes are more likely to receive “reminders” of their supposed noncompete obligations from their current employer.

We next establish that mistaken beliefs can be countered by providing employees with accurate information about the law and, further, that such information causes employees to change their prospective employment mobility decisions. We find that employees with a noncompete update their beliefs markedly to more closely align with the information they receive—especially employees in states that do not enforce noncompetes. In this same vein, employees with unenforceable noncompetes report feeling much less constrained by their noncompete after receiving accurate information about noncompete enforceability in their state.<sup>6</sup> Using our information experiment as an instrument for an individual’s beliefs about noncompete enforceability, we estimate that believing noncompetes are enforceable increases the likelihood that an employee anticipates their noncompete would be a factor in choosing to start or join a competitor by approximately 60 percentage points relative to an employee who believes noncompetes are unenforceable.

To build on our evidence that an employee’s beliefs about noncompete enforceability influence whether the employee is willing to pursue or consider a job with their employer’s competitors, we also assess whether these beliefs might affect (prospective) negotiation over a noncompete provision during contracting as well as the extent to which our results are driven by changes in the perceived likelihood of being sued for violating a noncompete. Among those presently bound by noncompetes, we find no evidence that believing that a noncompete is enforceable causes employees to be more likely to negotiate over these provisions. We also estimate that 20–30% of the effect that enforceability beliefs have on whether a noncompete matters for accepting a new employment offer is attributable to changes in whether the employee anticipates a subsequent enforcement lawsuit. Nevertheless, we also find that among employees with unenforceable noncompetes who believe their noncompetes are unlikely to be enforced and who view the likelihood of being sued as low, 12–25% still consider their noncompete to be a factor in whether to take a position with a competitor—perhaps because of moral, reputational, or relational costs from breaking their word.

This research enriches our understanding of (mistaken) beliefs about law (Kim 1997, Wilkinson-Ryan 2017), “information shrouding” (Gabaix and Laibson 2006), and the use of unenforceable contract terms (Furth-Matzkin 2017, Koszegi 2014, Tirole 2009). It also contributes to the body of work

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<sup>6</sup> Interestingly, again, the effects of correcting beliefs in our information experiment appear to be concentrated among individuals in states that do not enforce noncompetes (versus individuals who initially view noncompetes as unenforceable in states that actually *do* enforce them). This asymmetry suggests that inaccurate initial beliefs that a noncompete is unenforceable may be driven less by some mistaken understanding about a state’s law than by other beliefs not affected by the new information—for example, that a lawsuit brought by a former employer is practically unlikely or that a court would likely find the respondent’s particular noncompete to be unreasonable.

on the behavioral effects of noncompete agreements and related reform proposals. To begin with, although prior research has documented mistaken beliefs about the law in other settings (e.g., Darley et al. 2001, Rowell 2017), we find that these mistaken beliefs can persist even when the stakes are high—i.e., when they operate to limit an employee’s professional opportunities. Moreover, consistent with firms “shrouding” information on prices to keep consumers in the dark (Ellison and Ellison 2009, Brown et al. 2010), we present evidence that employers may actively reinforce ignorance about the law when it benefits them.<sup>7</sup> Mistaken beliefs may also be self-reinforcing if employees who believe their noncompetes are enforceable simply opt out of searching for jobs with competitors. Second, we show that mistaken beliefs about enforceability explain at least some of the behavioral response of employees to *unenforceable* noncompetes (Sullivan 2009, Fisk 2002). Alternative theories, such as concern about reputation or the moral or relational costs of breaking a promise, also appear to have some merit (MacLeod 2007). One implication of these findings is that existing studies that exploit bans on noncompetes (Balasubramanian et al. 2022, Lipsitz and Starr 2022, Fallick et al. 2006) likely understate the effects of noncompetes themselves because some employees continue to adhere to newly unenforceable noncompetes (Starr et al. 2020). Third, given that beliefs and prospective decisions change when we supply people with information about the law, our research implies that educational campaigns as a form of regulation offer some promise—more effective, perhaps, than statutes that simply render noncompetes unenforceable in court. Alternatively, policymakers may succeed with laws that directly target the use of noncompetes, such as penalties for use or garden leave obligations.<sup>8</sup>

We organize the remainder of our article as follows: In Section 2, we review relevant literature—particularly research exploring ignorance about the law, the consequences of this ignorance, 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 common casual 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 to have

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<sup>7</sup> While we focus in this paper on noncompetition agreements, our results also have implications for other policies and provisions that limit *within-industry* mobility of employees, including the inevitable disclosure doctrine (Flammer and Kacpercyk 2019, Contigiani et al. 2018), trade secret laws (Png 2017), and other restrictive covenants in employment contracts (Balasubramanian et al. 2021).

<sup>8</sup> Garden leave refers to an employer keeping an employee on payroll but away from work obligations during the prohibition period of a noncompete—i.e., a soon-to-be-former employee is compensated to tend their proverbial garden (see Oregon Revised Statutes 653.295) while they wait out their noncompete term, after which they are free to work for their prior employer’s competitors.

serious ramifications. For example, Kim (1997) finds that job-seekers overwhelmingly overestimate the legal protections afforded by default (at-will) employment contracts. Mistaken beliefs of this sort are especially relevant to our work. In contrast to some consumer settings where the consumer's interest is modest and protecting oneself requires near-constant (unrealistic) vigilance, an employment relationship is central to many people's lives, the stakes are high, and there are relatively few salient and predictable points in time (e.g., hiring, promotion) when employment contract terms are negotiated and resolved. Thus, good reasons exist to predict that people will "read the fine print" of employment contracts. Yet Kim's study reveals that employees enter into employment relationships systematically misinformed about the extent of their protection from discharge. Kim's research also implicitly undermines an alternative theory that justifies the at-will rule as a reflection of the parties' preference for internal, non-contractual norms to prevent welfare-reducing terminations.

Kim (1997) identifies a particular legal doctrine about which most employees are mistaken, but her finding is no anomaly: other empirical research confirms that systematic mistakes about the content of law are a general phenomenon. Some of this work also makes 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 areas of law, explicitly testing whether people are aware of any "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 "best guess" estimates across jurisdictions with different laws. (This interpretation is consonant with the direction of mistaken beliefs in our data.) Darley et al. also uncover 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.<sup>9</sup> Rowell also detects varying degrees of informedness across six states regarding ten 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 fails to discover any relationship between the perceived importance of the law and the accuracy of respondents' beliefs, again consistent with the existence of systematic mistakes about weighty legal issues (Kim 1997).<sup>10</sup>

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<sup>9</sup> There is evidence that cuts against this view, however; at least in some contexts, legal intuitions do not seem to align with normative intuitions (Furth-Matzzkin and Sommers 2020).

<sup>10</sup> Other studies examine the problem of inadequate knowledge among actors who seek to assert their legal rights or entitlements. For example, in another context, Grisso (1980) empirically measures the capacity of juveniles to understand their *Miranda* rights and finds, overwhelmingly, that they could not understand these protections. Grisso contends that the law should adapt to this widespread confusion by developing a *per se* rule excluding juvenile waivers. Other studies, exposing similarly widespread misapprehensions about rights, maintain that governments can improve understanding of the law by simply enhancing "notice." For instance, Tymchuk et al. (1986) finds that user-friendly methods like the use of large print or videos can increase comprehension of patient rights by the elderly. Similarly, DeChiara (1995) argues that requiring employers to disseminate more and better legal information may reduce employee ignorance relating to their right to bargain.

These studies point to 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 of what seems most familiar. One implication of these conclusions is that people’s beliefs, and potentially their behavior, can be shaped, either unintentionally or with a particular purpose in mind. Relatedly, Stolle and Slain (1997), Hoffman and Ryan (2013), Wilkinson-Ryan (2015), Wilkinson-Ryan (2017), Furth-Matzkin and Sommers (2020), and Furth-Matzkin (2019), among others, demonstrate that actors can strategically influence beliefs about law and related behavior, showing in experimental settings that the inclusion of erroneous law (specifically, unenforceable provisions) in contracts and leases (or manipulating whether people believe they are a party to a contract or a lease with similar language) can deter individuals from exercising their actual legal rights—rendering them “demoralized by contractual fine print” (Furth-Matzkin and Sommers 2020).<sup>11</sup>

Research also indicates that the inclusion of terms in formal 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 that conflicts with these 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 that a court previously held a term in a contract to be unenforceable reduces an individual’s beliefs 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, specifically, the enforceability of unenforceable terms in contracts. Darley et al.’s (2001) findings hint that such manipulation will likely be easier to accomplish when unenforceable terms are actually enforceable in many or most other places.

Together, these lines of research imply that employers in jurisdictions where noncompetes are unenforceable may nonetheless include them in their employment contracts, and that employees may be likely to hold inaccurate beliefs about noncompete enforceability (and guide their behavior at least in part on the basis of these inaccurate beliefs)—though the character of any such mistakes is unclear

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<sup>11</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. (2021) demonstrate 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 finds widespread inclusion of either misleading or flat-out invalid terms within these lease agreements. Her work confirms empirically, at least in the residential lease 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. In related work, Hoffman and Strezhnev (2022) offer a different explanation to explain the existence of unenforceable terms. Our work here extends this literature to real-world, long-term employment contracts/relationships and future mobility intentions.



ex ante. If employees generally take noncompetes to be unfair, they may view them as unenforceable. Alternatively, because any noncompete is part of an employment contract, and because most states do enforce noncompetes, the typical employee in a state where noncompetes are unenforceable may nevertheless assume that employers in their state can lawfully enforce such provisions.

The potential benefits to employers of using unenforceable noncompete provisions when employees may mistakenly assume they are enforceable call to mind profitable “information shrouding” by firms under conditions of costly information acquisition (Salop and Stiglitz 1977, Gabaix and Laibson 2006). In these models, firms take advantage of consumers’ inaccurate beliefs and avoid debiasing them. Mistaken consumer beliefs can give retailers some degree of market power; the costs of obtaining correct information from the market prevent consumers from switching to another seller. In our context, employers wield “monopsony” power (Manning 2020). The cost of uncovering accurate information about enforceability may prevent employees from contravening unenforceable restrictions, allowing employers to reduce turnover and inhibit labor market competition with competitors. For instance, if the prevailing industry wage were to rise, employees who rely on the mistaken beliefs that their noncompete is enforceable when it is actually unenforceable will be less likely to take advantage of better outside options (Johnson, Lavetti, and Lipsitz 2020).

Extensive research indicates that unenforceable noncompetes are very common (Prescott et al. 2016, Colvin and Shierholz 2019, Starr et al. 2021, Balasubramanian et al. 2021), and Starr et al. (2020) find that unenforceable noncompetes affect employee mobility. These two findings suggest that noncompetes operate through channels other than actual enforceability. Below, we test whether mistaken beliefs about enforceability at least partially explain these two patterns.<sup>12</sup> Additionally, we seek to understand why, in the noncompete context, mistaken beliefs about the law appear to be persistent, focusing both on employee-side behaviors that may insulate or even reinforce inaccurate beliefs and employer-side behaviors that aim to keep employees misinformed. We also assess the consequences for beliefs, predictions, and intentions of directly providing employees with relevant and accurate information on noncompete enforceability in their jurisdiction.<sup>13</sup> All of this matters because the strategic (or just lazy, form-driven) use of unenforceable provisions may be quite socially costly in the context

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<sup>12</sup> Of course, there are alternative explanations. First, employees may not be mistaken about their noncompete being unenforceable and yet may comply because of the reputational or relational costs of not following through on their “promise” (MacLeod 2007). Second, even if there are no reputational consequences, employees may not violate a noncompete they *know* to be unenforceable because of some subjective cost of breaking one’s word (Sullivan 2009, Fried 2015). We are able to separate out these competing theories to some degree in our information experiment based on whether and how receiving accurate information changes behavior. An employee’s decision to continue to adhere to a noncompete after learning that noncompetes are unenforceable indicates that something beyond “enforceability” is driving compliance.

<sup>13</sup> In doing so, we extend Wilkinson-Ryan’s (2017) research by evaluating the impact of providing a more reform-friendly summary of settled state law about entire categories of provisions rather than a past court case finding a particular hypothetical term unenforceable.

of noncompete agreements (Sullivan 2009).<sup>14</sup> At the very least, unenforceable noncompetes may inhibit productivity-enhancing employee mobility without providing the proper incentives for employers to make investments in employees (Rubin and Shedd 1981). Accordingly, evidence that speaks to the potential value of an information campaign to reduce or eliminate mistaken beliefs about enforceability may be 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>15</sup> The sample population consists of 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>16</sup> Using these data, Starr, Prescott, and Bishara (2021) provide the first systematic evidence on the incidence of noncompetes across the U.S. labor force, finding that noncompetes bind roughly one in five labor force participants. Starr, Prescott, and Bishara (2020) add by demonstrating how noncompetes can and do influence the process of employee mobility, independent of whether noncompetes are actually enforceable.

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 take advantage of several novel aspects of our survey data. First, we analyze employees' *beliefs* about whether, if they took a job with a competitor and their prior employer sued them for violating their noncompete, a court would ultimately enforce their noncompete.<sup>17</sup> Second, we examine the results of an information experiment that we built into our survey in which we informed a random selection of respondents 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

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<sup>14</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 that strike the unenforceable portions but salvage the contract as a whole, leaving the contract drafter no worse off. He argues that this approach by courts does little to address the actual problem of these unenforceable provisions: the *in terrorem* deterrence of the many who view these terms in these contracts as enforceable.

<sup>15</sup> We provide a brief discussion of the data here and refer the interested reader to our Online Appendix for further information, with an even more detailed description appearing in Prescott et al. (2016).

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

<sup>17</sup> We can gauge these beliefs in two ways using our survey data. First, the survey asks, "*Are noncompetes enforceable in your state?*" Second, the survey asks respondents to assign a probability that a court would enforce their noncompete were they to 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, the survey asks 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?*"

campaign or as improved access to legal information, but the experiment also functions as a source of exogenous variation in beliefs about noncompete enforceability, which allows us to identify the effects of *beliefs* about enforceability on prospective 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>18</sup> which captures the conditions under which states will (and will not) enforce noncompetes, including any exemptions under state law. We summarize these dimensions in Table OA1,<sup>19</sup> which shows which states have adopted which policies 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 an employer terminates an employee 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 that a court will enforce a noncompete. We then add a fourth dimension: whether the state will enforce noncompetes at all (the three states that essentially do not enforce at all are California, North Dakota, and Oklahoma). Next, we 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. Thus, the final score for each respondent is between “0” and “1.”<sup>20</sup>

For purposes of this article 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 (and state-occupations) fall into each category and provides summary statistics across the full sample and the sample of individuals with a noncompete, which will be our focus in most of our analyses. In Figure OA1, we present a map of the U.S. shaded according to the level of enforceability.

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<sup>18</sup> See our Online Appendix C for the exact documentation in Beck (2014).

<sup>19</sup> The language we use to describe enforceability in Table OA1 with respect to each particular aspect of noncompete policy is also identical to the language we use in our information experiment.

<sup>20</sup> If a state does not have a policy on any particular dimension of enforceability (e.g., whether the state will enforce a noncompete for an employee terminated without cause), we exclude that dimension from the calculation of that state’s overall index, dividing the state-specific score by the maximum of the non-missing scores for that state. There are other ways to aggregate these measures into a useful index (see, e.g., Bishara 2011 and Starr 2019), but our approach cleanly identifies nonenforcing states and does not presume any linear relationships.

## 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. 2020)—but also to identify the causal relationship between an employee’s beliefs about enforceability and their future expectations and intentions regarding their noncompete-related behavior. Our various research questions require a range of empirical tools, so we describe our empirical methods as needed along the way.

### 4.1 Employee Beliefs about Noncompete Enforceability

To begin, Table 2 tabulates responses to the following 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, noncompete policies are and historically have been under the purview of states (Bishara 2011). Only 24% of respondents—just four percentage points higher than guessing at random—are aware of the legal primacy of states in this domain. The proportion of respondents who answer correctly in our survey scales somewhat with education; a larger share of those with education beyond a bachelor’s degree recognize that noncompetes are enforced at the state level (32%) in comparison to those with less than a bachelor’s degree (21%). A slightly larger share of those who have a noncompete with their current employer recognize that state law governs their noncompete (30%) relative to those who are not bound by a noncompete (23%). Taken together, Table 2 suggests that the majority of employees, regardless of their education level and even if they are presently subject to a noncompete, are unaware that noncompete enforceability is state-level policy.

Panel A of Table 3 presents a summary analysis of answers to the following question: “*Are non-competes 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 low, considering that 13% of the population resides in states that either 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, less than 10% of the sample believes that noncompetes are unenforceable, suggesting that the conventional set of beliefs in the population are that noncompetes are enforceable—especially for those presently subject to one (Wilkinson-Ryan 2017).

Panel B of Table 3 investigates the accuracy of these beliefs, using our broad classification in which we treat California, North Dakota, and Oklahoma as the only states that refuse as a policy matter to enforce noncompetes.<sup>21</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 presently bound by noncompetes.<sup>22</sup> In contrast, the “informed”—those who correctly estimate noncompete enforceability in their state—amount to 53% of the population and 67% of those working under noncompetes. The apparently high proportion of “informed” employees 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 individuals with a noncompete according to actual state policies, where the “no enforceability” states are those that entirely deny enforcement for all categories of employees (i.e., California, North Dakota, and Oklahoma) and where medium/high enforceability states are the complement. The figure shows that while 74.8% of those with a noncompete in states that enforce noncompetes are informed, 70.2% of those with a noncompete in states that do not enforce noncompetes are misinformed (8.4% are uninformed). Figure 2 presents these patterns by education level (among those affirmatively bound by a noncompete). While highly educated employees appear to be slightly better informed in states that do not enforce noncompetes, more than 70% of those with above a bachelor’s degree are either misinformed (64.6%) or uninformed (6.5%). Taken together, Table 3 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 confirm that this pattern is relatively stable across education levels.<sup>23</sup>

We can assess the robustness of these findings by turning to a more nuanced measure of beliefs about noncompete 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*

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<sup>21</sup> We do not incorporate the occupation-specific carve-outs in this measure because the question refers to state law broadly and is not specific to the respondent’s occupation. Also, it is important to note that these states will enforce noncompetes incident to the sale of business but not for an employee’s move between employers. Our survey is limited to employees (we drop self-employed individuals), making this omission less of a concern. Our main continuous measure of enforceability is specific to employee mobility (as opposed to business sales).

<sup>22</sup> We classify as misinformed those in California, Oklahoma, or North Dakota who answer that noncompetes *are* enforceable and those in the rest of the states who state that noncompetes are *not* enforceable. Note that not all noncompetes are enforceable even in states that will generally enforce them; the terms of any noncompete in an enforcing state must still survive the state’s “reasonableness” test before a court will enforce it (Bishara 2011).

<sup>23</sup> See Figure OA2 for a cut by occupation, conditional on having more than 20 individual respondents in that occupation in both enforcing and non-enforcing states. Lawyers are the most likely to be aware that their noncompete is unenforceable. However, because these estimates are underpowered, we recommend viewing them with caution.

*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)?*” An answer to this question thus provides 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 we document in Table 3. The graph plots subjective beliefs as a function of categorical beliefs and whether the employee is presently bound by a noncompete. Figure 3 shows that employees who believe noncompetes are unenforceable also 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 OA2, columns (1) and (2) for regression results with and without “basic” controls).<sup>24</sup>

Using this individual-specific measure of enforceability (i.e., respondent’s beliefs about likely enforcement in their own situation), Figure 4 assesses whether beliefs about enforceability unconditionally correspond with actual enforceability by noncompete status.<sup>25</sup> Generally speaking, if employees are accurately informed about noncompete enforceability, the lines in Figure 4 should be at least weakly upward sloping. But the relationships we uncover are relatively flat. Employees with a noncompete believe that a court will enforce their noncompete somewhere between 40% and 46% of the time, regardless of actual enforceability in their jurisdiction (with the highest estimate of enforceability coming from those in states that do *not* enforce noncompetes). Employees without noncompetes report similarly invariant beliefs across jurisdictions, though the levels differ (see columns (3) and (4) of Table OA2). These figures suggest that, as before, employees living in states where courts would not countenance their noncompete agreements remain generally unaware of the unenforceability of such provisions. To explore this pattern more closely, Figure 5 addresses only the noncompete population to determine whether more highly educated employees are more likely to be informed. 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 OA2).<sup>26</sup>

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<sup>24</sup> In our regression work, “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). We use the adjective “basic” because, in prior papers using these data, we distinguish between more plausibly exogenous “basic” controls and other “advanced” controls that may be endogenous to the contracting process and therefore potentially problematic to include (Starr et al. 2020, Starr et al. 2021).

<sup>25</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.

<sup>26</sup> One potential critique of our approach here is that employers with establishments in multiple states could use noncompetes with choice-of-law clauses incorporating another state’s law. We find no evidence that beliefs about noncompete enforceability vary by whether the employer is a multi-state operation, an employer characteristic that we collect

## 4.2 The Persistent Inaccuracy of Employee Beliefs

The prior section establishes that employees with unenforceable noncompetes are largely unaware 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 1) the existence of a default presumption among employees that contracts generally and noncompetes specifically are enforceable and 2) a pervasive inference that any particular noncompete is likely enforceable given that noncompetes are enforceable in a “majority” of jurisdictions (Darley et al. 2001). However, both of these hypotheses fly in the face of traditional views about the advantages of learning the truth (which seem significant), the relatively low costs of obtaining freely available information, and the information-diffusing benefits of labor markets. Employment contracts are high stakes, and employees looking for a new position will presumably meet potential new employers 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.

Our supply-side hypothesis is simply that many employees who mistakenly believe their noncompete is 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 a noncompete, we regress this measure of search effort on indicators for whether the employee is informed about the law, interacted with actual noncompete enforceability, and employer and employee controls. The results, shown in Figure 6, offer some support for this hypothesis. Conditional on our basic controls, 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 these two groups (see columns (1) and (2) of Table OA3 for unconditional and conditional model estimates).

An important limitation of this analysis is that it does not exploit any exogenous variation in an employee’s beliefs or in the accuracy of their beliefs about enforceability. Accordingly, these results should be interpreted as descriptive; some unobservable factor may exist that affects *both* how well informed an employee is about the enforceability of their noncompete *and* their level of search effort toward competitors. Reverse causation may also drive the relationship we observe—those who exhibit more search effort toward competitors may be more likely to learn about the law. While we

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using our survey instrument. We classify employees based on the state where they work, however, and we do not know if their contract invokes another state’s law.

acknowledge these concerns, our results nevertheless make clear 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—put less effort into searching for new positions at competing firms, necessarily limiting their ability to learn about the law governing their contract from competitors. This finding reminds us that certain mistakes—even mistakes about the law—may cause agents to refrain from activities that facilitate error correction and thus can become persistent.

Our demand-side hypothesis emerges from the information-shrouding literature. Employers in states that do not enforce noncompetes may have relatively weak incentives to inform employees at competing employers 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 employer wants to poach employees with unenforceable noncompetes, one would guess it need only give these employees offers and inform them that their existing noncompetes are unenforceable. However, such “informative” recruiting may be either unattractive to the poaching employer or unlikely to succeed without substantial effort (Gabaix and Laibson 2006). The recruiting employer may not benefit on net from successfully informing a prospective employee about their noncompete’s unenforceability for two reasons. For one, once the focal employee appreciates the unenforceability of their noncompete, the recruiting employer may face greater competition for that employee, who might now be more open to offers from, for instance, more obvious competitors to their current employer. Moreover, the recruiting employer may itself use unenforceable noncompetes with its existing employees, who may *also* mistakenly believe such provisions are enforceable (as seems likely given Section 4.1). Thus, “informative” recruiting may produce a pyrrhic victory—i.e., higher turnover and wage costs—if the new hire eventually informs the employer’s entire workforce about the unenforceability of noncompetes (from the employer’s “own mouth,” as it were). Finally, convincing a prospective employee that their unenforceable noncompete is actually unenforceable may be too difficult to justify in many cases. For example, an employee’s current employer may implicitly (or explicitly) threaten potentially departing employees with litigation by reminding them that they agreed to a noncompete clause (or by actually suing them), which may render employees *more* (not less) likely to believe their noncompete is enforceable—perhaps specifically when it is unenforceable.

To assess whether there is 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 reports receiving a job offer from a competitor in the last year. The second is an indicator for whether, if an employee’s present employer became 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 using data from noncompete-bound employees, including basic controls, of employee beliefs regarding the level of noncompete enforceability interacted with whether the employee in question received a job offer from a competitor within the last year. The results furnish some



support for the demand-side hypothesis: we find that employees who receive offers from competitors actually believe their noncompetes are enforceable to a somewhat greater degree on average relative to those who do not receive offers from competitors (55% vs 47%), though the difference is not statistically significant (see columns (3) and (4) of Table OA3).

Figures 8 and 9 attend to the potential role of strategic reminders by current employers in keeping employees misinformed about the unenforceability of their noncompete. Figure 8 shows that, comparing two observationally equivalent employees (per our basic controls) who are subject to a noncompete and who have received job offers from competitors, an employee with an *unenforceable* noncompete is approximately 40 percentage points more likely to receive a reminder about their (unenforceable) noncompete (71% vs 32%, 34%) from their employer.<sup>27</sup> Figure 9 documents that reminders alone are associated with stronger beliefs about the enforceability of noncompetes, regardless of the level of enforceability (see columns (1)–(4) of Table OA4).<sup>28</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 subsequent reminders or threats from current employers—may actively prevent employees from learning that their noncompete is unenforceable.

A key limitation of our analysis of noncompete reminders is that relatively few employees with a noncompete in our sample received offers from competitors that became known to their employer—which is necessary for their employer to respond to the competing offer by issuing a reminder (237 total observations). To supplement our analysis, we turn to a 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?*”<sup>29</sup> Logically, reminders are a likely precursor to a lawsuit, so knowledge of a prior lawsuit (or at least a letter threatening legal action) may operate much the same as a reminder in terms of reinforcing an employee’s beliefs in enforceability. It also reflects the idea that employee beliefs may respond not only to what the employee experiences personally (as in the reminders analysis) but also to the experiences of their present and former coworkers. Figure OA3 shows that approximately 20–24% of individuals with a noncompete are aware of (or believe they are aware of) their employer suing others over noncompetes, and this relationship is relatively flat with respect to actual enforceability (see columns (5) and (6) of Table OA4). Interestingly, however, Figure OA4 shows that employees who believe their employer has sued former employees are significantly more likely to believe that their noncompete is enforceable (see columns (7) and (8) of Table OA4), and this effect appears to be especially pronounced for employees with a noncompete that is actually *unenforceable*.

<sup>27</sup> These results are robust to dropping observations from California. Without data from California, 62.2% of employees still receive reminders about unenforceable noncompetes.

<sup>28</sup> Both Figures 8 and 9 graph results from the regression estimates we report in Table OA4.

<sup>29</sup> We acknowledge that it is not entirely clear whether respondents interpreted this question as asking whether their employer actually filed a legal complaint or, alternatively, whether hearing that one or more fellow employees had received a “threatening letter” or other warning would suffice for respondents to answer “yes.”

Thus, with reminders and hints of (frivolous) lawsuits, employers seem endowed with at least some ability to convince individuals with an unenforceable noncompete that their noncompete is in fact enforceable, countering whatever effect competing firms may have if they attempt to disabuse these employees of their mistaken beliefs about enforceability.<sup>30</sup>

### 4.3 Information Experiment Design and Balance Tests

Whatever the *reasons* for persistently mistaken beliefs about noncompete enforceability among employees, an important question is whether effective policy responses exist. Policymakers might deter employers from using unenforceable noncompetes by imposing financial penalties for their use or by requiring compensation during any noncompete prohibition period (i.e., garden leave). An alternative, possibly more effective solution to inaccurate beliefs about enforceability is an educational campaign—such as the regular posting of employee contractual rights and information at the workplace or elsewhere—and mandatory legal disclosures that are comprehensible, easy to verify, and conspicuous. There is considerable debate over the value of disclosures as a means of positively influencing behavior. Ben-Shahar and Schneider (2011), for example, describe many of the drawbacks—indeed the harms—of such an “educational approach,” and yet other work, for example, Wilkinson-Ryan (2017), Furth-Matzkin (2019), and Furth-Matzkin and Sommers (2020), finds clear benefits. To gauge the potential effects of providing accurate information to employees about enforceability, we simulate a (rough) disclosure policy for correcting mistaken beliefs via an information experiment within our survey. Researchers use this empirical strategy in many contexts. Recent studies, for example, examine the impact of information on business economic expectations over time (Coibion et al. 2018), college major choices (Wiswall and Zafar 2015), and settlement decisions (Sullivan 2016).

Our information experiment analysis proceeds in three steps. First, we assess our respondents’ baseline expectations about noncompete enforceability (which we describe and analyze at length above) and how they regard the effects of any 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 about 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 influence of these provisions on the respondent’s behavior by re-administering questions from the first stage of the information experiment—even to those who do not receive the information treatment.<sup>31</sup>

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<sup>30</sup> An employer bringing a lawsuit to enforce a clearly unenforceable term can, at least in some jurisdiction, be subject to a countersuit on the part of the employee for unfair labor practices (as in California). However, taking advantage of this right of action can be costly and risky for an employee, leaving employers at least some room to posture in a way that might reduce mobility that conflicts with the terms of a noncompete.

<sup>31</sup> By asking those who do not receive information the same questions, we can alleviate concerns that those in the treatment group are changing their answers simply because they must answer the same questions twice.

We gather the specific information about the law that we supply to respondents in the experiment from the characterization of state-level noncompete regimes contained in Beck (2014), which we provide in some detail in Online Appendix C. We summarize these laws in Table OA1. We outline the actual information that we present to those who receive information (treatment) in Figures OA5 and OA6. In the survey, the information appears in the order indicated in those figures. Figure OA5 explains that noncompete policy is designed and enforced at the state level and that only a few states do not enforce such provisions.<sup>32</sup> It also describes the typical reasonableness test that state courts employ when they decide whether to enforce a noncompete in a particular case. Figure OA6 displays all of the state-specific information the survey delivers to respondents, where the blue arrows indicate our experiment’s “display logic” by which we ensure that we introduce only appropriate information (depending on the state in which the respondent works) to respondents as part of the treatment (see Table OA1 to link specific policies to individual states).<sup>33</sup>

In Table 4, we present the results of a balance test to verify that individuals with a noncompete 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 likely to be in the group that receives information (and the medium enforceability category drives this difference)—there are no statistically significant differences between the (unweighted) treatment and control groups in the full sample or any subsample.

#### 4.4 Information Effects on Employee Beliefs

Figure 10 reports the distribution of beliefs among individuals with a noncompete across the treatment and control groups—i.e., according to whether the individual receives information on actual noncompete enforceability in their state. The top row of Figure 10 shows, not surprisingly but reassuringly, that the distributions of beliefs before and after the experiment among those who do not receive any information are nearly identical. In contrast, for those who receive information in the “no enforceability” group, we observe a large leftward shift in the distribution of beliefs. This swing indicates that employees can actually read and absorb the information in our treatment. In 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

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<sup>32</sup> In Figure OA5, we only list California and North Dakota as nonenforcing states. This is discordant with Beck (2014), which includes Oklahoma as a nonenforcing state. We exclude Oklahoma from Figure OA5 because, in the literature, we found competing views on whether Oklahoma is truly a nonenforcing state in 2014 (see Bishara 2011). Nevertheless, we include Beck’s (2014) characterization in the state-specific information we provide regarding Oklahoma. As a result, employees in Oklahoma (we only have 118 such individuals in the full sample—of whom only 13 indicate having a noncompete) may be *undertreated* by our experimental choices.

<sup>33</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 excludes that information. There are only 25 respondents with a noncompete from Alabama, although 12 of these are professionals. Fortunately, this error does not materially influence our results.

to regression results in Table OA5 columns (1) and (2)). Consistent with Figure 10, we find that those who receive information that their noncompetitor is unenforceable are far less likely to believe that their noncompetitor is enforceable (24%) relative to those who do not receive information (46%). These effects appear muted for the medium and high enforceability groups. Taken together, Figures 10 and 11 demonstrate that information delivery is most effective at changing beliefs among those with an entirely unenforceable noncompetitor, which is the population entertaining the bulk of mistaken beliefs in this domain. Notably, providing information that noncompetitors are unenforceable—at least as we do in our experiment—does not completely free the informed from their mistaken beliefs.

Importantly, the raw distributions and mean effects we present in Figures 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 an 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 receiving information, their responses would line up along the 45-degree line (shown in thick black 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 that those who receive information update *differently* given initial beliefs and actual enforceability. For example, respondents who initially estimate their noncompetitor to be enforceable with certainty reduce their post-experiment beliefs considerably: those with an unenforceable noncompetitor reduce their estimate to approximately 35%, while those in medium and high enforceability states reduce their beliefs to 75–80%. These latter shifts imply that accurate and precise information even for medium and high enforceability states may give employees some doubt that their noncompetitor can or will be enforced. We see a similar pattern among those who initially view their noncompetitor as largely unenforceable—these individuals update their beliefs upward, especially if they live in a state where noncompetitors are moderately or easily enforceable.

Figure 13 characterizes the mean effects of information on beliefs among individuals with a noncompetitor 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 OA5 columns (3) and (4)). The results show that the drop in mean beliefs in Figure 11 is almost entirely attributable to the changing beliefs of those who initially view their noncompetitor 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 noncompetitor is actually unenforceable, and even causes drops of 8–10 percentage points in medium and high enforceability states. In contrast, those who initially believe

their noncompete is unenforceable (left panel of Figure 13) are largely unmoved by the information—even in medium and high enforceability states.<sup>34</sup>

#### 4.5 Information Effects on Prospective Employee Behavior

In this section, we examine whether the delivery of accurate information about noncompete 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. 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>35</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. Moreover, because our information treatment is less polished and credible than a professionally designed educational campaign would be, our assumption is that our estimates are conservative.

To collect a broad measure of how a noncompete might influence employee mobility, our survey presents respondents with the following question both before and after our experimental treatment: *"If you received a much better offer from a comparable, competing employer, would your noncompete be a factor in preventing you from moving?"* (Starr et al. 2020). In Figure 14, we calculate how responses to this question differ depending on treatment status and the level of enforceability.<sup>36</sup> For individuals with an unenforceable noncompete, 51% of those who do *not* receive information indicate that their noncompete would be a factor in whether they would accept the job offer, versus 26% among those who receive accurate information about lack of enforceability. For individuals with a moderately enforceable noncompete, the difference is smaller (46% vs 38%), while there is no difference for those with a highly enforceable noncompete. Figure 15 breaks out this analysis based on individual responses to this same question before the experiment, conditional on basic controls (see Table OA6 columns (3) and (4)). In the right panel, we find that individuals who initially report that their noncompete *would* be a factor in leaving their current employer but who live in a state where noncompetes are actually unenforceable experience the largest drop to 51%. Notably, the control group (which does not receive information)

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<sup>34</sup> Figure OA7 shows the same heterogeneity for the sample of employees not bound by a noncompete. Those who receive information and mistakenly believe pre-treatment that any noncompete 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 a noncompete, however, those who mistakenly believe any noncompetes would not have been enforceable also update their beliefs moderately when those noncompetes are highly enforceable (left panel).

<sup>35</sup> Anecdotally, several of the survey participants who received information thanked us at the end of the survey for letting them know that their noncompete was unenforceable. This suggests that real learning about the content of the law in such a format can affect future employment-related decisions.

<sup>36</sup> The sample is limited to individuals with a current noncompete, and the underlying regression specification includes basic controls. We report the full results in Table OA6.

also shifts downward a little as well, suggesting that control respondents answer the question differently the second time. In the left panel, we detect fewer differences by treatment status in the sample of individuals who initially report that their noncompete would not be a factor.<sup>37</sup>

One important and interesting result of this analysis is that, even after employees learn that their noncompete is unenforceable, many still indicate (in our survey, at least) that they will weigh their noncompete as a factor in deciding whether to take a better job at a competing employer. 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—may influence employee mobility decisions through other channels as well.<sup>38</sup> Formally agreeing to a noncompete, for example, might increase the subjective cost of violating one’s word, the reputational cost of breaking a nonbinding “promise,” or even the financial cost of defending oneself against a frivolous lawsuit (Sullivan 2009). We return to this issue in Section 4.7.

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

In the previous section, we examine the effects of our simple information treatment on (1) beliefs about noncompete enforceability and on (2) various prospective mobility outcomes. These findings are relevant to policymaking discussions about how best to correct mistaken beliefs about enforceability and about whether such interventions can influence mobility, either by changing beliefs or through other mechanisms. In this section, we study the relationship between (1) and (2) directly. Specifically, we leverage our information treatment to identify the causal effects of *beliefs* about noncompete enforceability on prospective mobility outcomes (as opposed to the effects of the information treatment itself). If someone believes that their noncompete is more rather than less enforceable, how much does that matter to their prospective mobility decisions? In theory, beliefs about enforceability might matter very little, if questions about enforceability are absent from an employee’s mobility-related decision making, perhaps because many other considerations (like reputation) matter far more.<sup>39</sup> Alternatively, employees may put weight on enforceability in making their mobility decisions, either in the abstract or by breaking down the separate practical facets of “enforceability,” like whether their employer might sue them if they depart to a competitor or, if a lawsuit does occur, whether a court would enforce their noncompete. In that case, employee *beliefs* about enforceability seem likely to matter to mobility, though how *much* they might matter remains unclear.

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<sup>37</sup> Figures OA8 and OA9 show the same patterns hold for whether a noncompete 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?”

<sup>38</sup> We acknowledge that one concern with this conclusion is that our respondents (in the right panel) initially state affirmatively that their noncompete *would* be a factor in deciding whether to leave their employer for one of its competitors, whereas in a real-world educational campaign, no preliminary mental choice would be required. Therefore, any post-educational choice would not be a “change” from a prior position.

<sup>39</sup> Of course, this possibility seems remote, given the results we report in Section 4.5.

To study the relationship between beliefs about noncompete enforceability and employee mobility decisions, we could simply check to see whether one correlates with the other. Controlling for observables, for instance, we might find that an employee's beliefs that their noncompete is enforceable are positively correlated with an employee's reporting that their noncompete would be a factor in their decision to leave for a competitor. This approach suffers from endogeneity concerns, however. For example, relatively sophisticated employees may be both more likely to believe that noncompetes are unenforceable (because such employees may be more knowledgeable about the law in jurisdictions where noncompetes are unenforceable) and more likely to attract outside offers. Another possibility is that relatively mobile employees who have had many conversations with friends about transitioning to other jobs may be more likely to have accurate beliefs about enforceability—i.e., low or at least lower estimates of enforceability in states where noncompetes are unenforceable.

Due to these endogeneity concerns, we use an instrumental variable approach that exploits the fact that the information experiment exogenously causes employees to update their beliefs about noncompete enforceability. The idea is that randomly deploying information causes some employees to update their beliefs when their initial beliefs are wrong, as in Figure 13. Accordingly, we instrument for post-experiment beliefs with a set of instrumental variables that capture the main effect of the information experiment and its interaction with the actual enforceability of the respondent's noncompete and an indicator for the respondent's pre-experiment beliefs about enforceability (above or below 50%).<sup>40</sup> Figure 13 (which effectively reports the first-stage 2SLS estimates) reveals that the compliant subpopulation driving any local average treatment effects is primarily individuals who have an unenforceable noncompete but who initially believe their noncompete is enforceable. The identifying assumption underlying these instruments is that the information shock affects mobility only through its effects on beliefs about the enforceability of noncompetes. In our view, this assumption seems at least plausible because the content of the information relates only to the circumstances under which a court in their state would enforce a noncompete. That is, it is difficult to conceive of a reasonable way in which new information about the content of law would affect mobility through some channel that does not depend on a change in what individuals believe about the law.<sup>41</sup>

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<sup>40</sup> This approach produces four total instruments: (1) receipt of information; (2) receipt of information×pre-experiment beliefs; (3) receipt of information×actual enforceability; (4) receipt of information×pre-experiment beliefs×actual enforceability. Note that we include the respondents' pre-experiment beliefs, actual state law, and the interaction of these two variables as controls in the 2SLS model.

<sup>41</sup> In Section 4.7, we explore one potential mechanism for how changing beliefs about enforceability might influence mobility—through changing beliefs about the likelihood of an employer filing a lawsuit. We acknowledge that there may be other ways that changing beliefs can affect mobility outcomes and that some of these scenarios might not be particularly policy relevant. One possibility is that the information in our experiment might engender an emotional response in respondents, such as anger, because they learn that their employer has been threatening them over an entirely unenforceable contract, which may then cause them to be more likely to want to leave their employer as they continue with the survey. While this anger response only arises because the information treatment changes these individuals' beliefs about

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 deciding to take a better job or start a competing enterprise. In all cases, we find that believing that a noncompete is enforceable causes a sizable increase in feelings that the noncompete limits job opportunities. These estimates are also quite large in magnitude. For example, an employee who believes their noncompete is enforceable with certainty is 43 percentage points more likely to feel like their noncompete limits their future job options (186% of the sample mean) and 66 percentage points more likely to report their noncompete would be a factor in joining a competitor (159% of the sample mean) relative to an employee who does not believe their noncompete is enforceable.<sup>42</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 might lead at least some employees to negotiate over the terms of their noncompete or to seek other benefits in exchange for agreeing not to compete. That is, if employees who believe their noncompete is 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? Starr et al. (2021) find that only 10% of workers overall negotiate over the terms of their noncompete,<sup>43</sup> so large effects seem unlikely, unless most or many of these bargaining employees were to live in states that do not enforce noncompetes. In Figure 16, we show that, comparing observationally equivalent individuals with a noncompete, the likelihood that people report negotiating over their noncompete does not differ dramatically across states that do and do not enforce noncompetes.<sup>44</sup> Column (4) of Table 5 reports IV results for the effects of beliefs in noncompete enforceability on negotiation expectations. Consistent with Figure

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the law’s content (otherwise, why an angry response?), such a mechanism may only operate in environments in which some employers engage in actively misleading their employees in equilibrium.

<sup>42</sup> Table OA7 explores the robustness of these relationships by exploiting answers to a series of questions about the importance of various factors in an employee’s decision whether to move to a comparable competing company. Columns (1), (2), and (3) show that believing that a noncompete is enforceable increases the importance of the employee simply having 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 enforce 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>43</sup> Rothstein and Starr (2022) find that employees with a noncompete do not appear more likely to bargain over wages, conditional on employee and employer characteristics, though they have relatively higher wages.

<sup>44</sup> Figure OA10 examines whether an information treatment might lead employees to update their estimate of the likelihood that they would negotiate in the future over noncompetes. While there is an enormous difference in levels between Figure 16 (which reflects actual reported negotiation behavior) and Figure OA10 (which reflects prospective negotiation behavior), the information treatment does not appear to differentially cause individuals to change their negotiation predictions 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 a noncompete *actually* negotiated over their current noncompete.



16, we detect 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 arguments often made in favor of enforcing noncompetes—that applicants and employees are rational and reasonably sophisticated agents with the power to negotiate for compensating differentials.<sup>45</sup>

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

Whether and how such beliefs about the enforceability of noncompetes matter to an employee's behavior may depend in part on what the employee believes about the likelihood that their employer will actually sue them for violating their noncompete in the first place—whether or not a court would enforce the noncompete. 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 litigate. In other words, legal enforceability does not translate one-to-one to the costs and consequences that might follow from deviating from the terms of a noncompete—distinct beliefs about the practical likelihood of a lawsuit may be important, too. Furthermore, a noncompete may still matter even when an employee believes it to be unenforceable and *further* believes that, regardless, their employer would never attempt to litigate over it. For example, employees may experience moral or reputational costs for violating the provision's spirit. 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 both that a noncompete is unenforceable *and* that, in any event, their employer will not cause a fuss by litigating the point. Figure 17 considers this question by categorizing employees based on whether they view their noncompete as enforceable and on whether employees perceive a lawsuit as likely (based on whether the reported likelihood of litigation is above or below 25%). We then cut the data by actual noncompete enforceability and further by whether a respondent receives information on the actual noncompete policies in their state.<sup>46</sup>

We uncover two strong patterns, both for those who do and do not receive information. First, individuals with a noncompete who believe that their noncompete is enforceable and that their employer is likely to sue them for breaching it are much more likely to see their noncompete as a factor in deciding whether to join a competitor (57%–78% depending on the level of actual enforceability)

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<sup>45</sup> In contrast, column (5) of Table 5 shows that those who are not bound by a noncompete 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 a noncompete report being less likely to negotiate when they receive information about noncompetes being unenforceable (Figure OA11). It is not clear *ex ante* why these answers differ from the noncompete sample in both direction and statistical significance. One possibility is that because these employees do not have a noncompete, they may be unfamiliar with the typical contracting process around noncompetes and therefore may make different assumptions about the costs and effectiveness of negotiation.

<sup>46</sup> As before, we include our basic controls and cluster standard errors at the state level.

relative to those who see neither possibility as very likely (5%–25%). Second, even when employees know that their noncompete is unenforceable in court and do not believe their employer is likely to sue them anyway if they depart, a non-negligible proportion of employees still view their noncompete as a factor in deciding whether to accept a competitor's offer: 12% among those who are informed about the law and 25% among those who do not receive information.<sup>47</sup> This evidence indicates that while beliefs about enforceability and the likelihood of an enforcement lawsuit can explain a substantial proportion of the variation in whether employees view their noncompete as a factor in deciding whether to accept a position with a competitor, other reasons likely remain important in their viewing a noncompete as an impediment. Two natural explanations, which we unfortunately cannot address further with our data, are the subjective disutility and the reputational costs of breaking a promise or otherwise upsetting a relational contract.

This joint analysis of beliefs about court enforcement and beliefs about employer litigation propensity is limited, however, because it treats the two as independent; it ignores the potential for beliefs about noncompete enforceability to *influence* beliefs about the likelihood of a lawsuit. It may be, for instance, believing that noncompetes are legally enforceable causes one to believe that their employer will sue them for violating one. We examine binned scatterplots in Figure OA12 relating beliefs about enforceability to beliefs about the likelihood of facing a lawsuit. The left and middle panels reveal an (unconditional) positive correlation between beliefs about noncompete enforceability and the likelihood of a lawsuit, both before and after the information experiment. The right panel, in turn, shows that this positive relationship holds within-individual, both for those who do and do not receive information. Because we randomly shock the former group's beliefs with information, we can interpret this positive relationship causally. More formally, in column (1) of Table 6, we use the same instrumental variables strategy we deploy 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 their noncompete is enforceable will also believe that their 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. Put another way, employees appear to assume that law at least partially determines employer litigation behavior.

Given that changes in an employee's beliefs about enforceability cause changes in beliefs about litigation risk—and that both seem to relate to whether a noncompete will be a factor in an employee's decision to transition to a competitor per Figure 17—we next explore to what extent beliefs about the

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<sup>47</sup> We note that these percentages may be too low when we take into account the fact that answering a survey is not the same as breaking a promise made to coworkers with whom one has had a long relationship. The latter is likely to be more socially or morally costly than the former.

likelihood of a lawsuit mediate the relationship between beliefs about enforceability and mobility intentions. Specifically, we test whether the relationship between beliefs about enforceability and mobility is driven entirely, in part, or not at all by changes in an employee's beliefs about the possibility of being sued over their noncompetes. Practically speaking, all this requires is that we examine models both with and without a post-experiment control for the perceived likelihood of a lawsuit.

First, we explore the robustness of our earlier information experiment results to the inclusion of controls for post-experiment beliefs about the likelihood of an employer lawsuit. We present our findings in columns (2) and (3) of Table 6. To reiterate our earlier results, we estimate that those who receive information on the lack of enforceability of their noncompetes are 25 percentage points less likely to report that their noncompetes 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 in column (3), the estimate falls to 15 percentage points. Thus, changes in employee beliefs about litigation risk account for 40.5%  $((0.252 - 0.150)/0.252)$  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 find that the mere inclusion of the perceived likelihood of a lawsuit causes the interaction between information in medium enforceability states to fall from 0.164 to 0.071, while the interaction between high enforceability and information falls from 0.256 to 0.171.

We perform one final test to assess how strongly the perceived threat of a lawsuit mediates the relationship between beliefs about enforceability and behavioral outcomes. Columns (4)–(7) examine OLS and 2SLS models, comparing whether beliefs about noncompetes enforceability relate to whether a noncompetes would be a factor in accepting an offer with a competitor. The OLS specifications suggest that 32.5% of the overall relationship between beliefs and our prospective mobility measure can be explained by how much employee beliefs about enforceability drive changes in beliefs about litigation risk (i.e., the effect of  $P(\text{Enforce})$  falls from 0.578 to 0.390 when controlling for  $P(\text{Lawsuit})$ ). Columns (6) and (7) report the same analysis, except in those specifications, we use the instrumented measure for post-experiment beliefs.<sup>48</sup> A similar pattern arises, with the likelihood of a lawsuit accounting for approximately 32.8% of the relationship between beliefs about the law and the extent to which noncompetes matter for taking a competing job.

Taken together, this section documents three facts regarding how beliefs about a lawsuit relate to beliefs about enforceability and mobility choices. First, a boost in one's beliefs that a noncompetes is

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<sup>48</sup> To perform the mediation analysis in column (7), we follow the Instrumental Variable mediation approach in equations (10) and (11) of Dippel et al. (2020). As an alternative, we construct the 2SLS estimate in column (7) of Table 6 “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 stage. With this approach, the coefficient on post-experiment beliefs falls 18%.

enforceable also increases one's beliefs that their employer will sue in response to a violation of the noncompete. Second, perceptions about the likelihood of a lawsuit mediate the relationship between beliefs about enforceability and mobility outcomes, explaining roughly 20–30% of the overall effect. Third, a nontrivial minority (12–25%) of those who see it as unlikely that their employer will sue them and *also* see it as unlikely that a court will enforce their noncompete still treat their noncompete as a factor in whether to take a job with a competitor. This last result suggests that being a party to a noncompete can still have chilling effects on an employee's mobility decisions, perhaps for reputational or relational reasons, even when the employee assumes a court would not uphold the noncompete and, in any event, the employer would never seek to enforce it.

## 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, a result that adds to existing work about mistaken beliefs about the law. We study mechanisms that may support persistently mistaken beliefs by circumventing normal pathways for correction. First, employees who are unaware that their noncompete is unenforceable may opt out of important “corrective” labor market activity by searching for jobs at competitors less often. We also build on the information shrouding literature, which emphasizes that firms can benefit from hiding certain pricing information from consumers, to show that recruiting employers may counterintuitively have reasons to keep applicants in the dark about the law. Finally, employers can (and often do) remind their employees of their noncompete—especially those with unenforceable noncompetes—to render them more likely to (mistakenly) believe their noncompete is enforceable.

We also show that an information treatment, which roughly simulates an educational campaign, can cause employees to update their beliefs—especially employees whose noncompetes are unenforceable. After receiving information, employees with unenforceable noncompetes report that their noncompete would be less of a factor in their choice whether to accept employment with a competitor than they indicate under mistaken beliefs of enforceability. However, employees as a group do not *fully* adjust their mobility intentions (i.e., they do not report that their noncompete would no longer be a factor whatsoever in leaving for a competitor). In fact, a nontrivial fraction of employees who see their noncompetes as unenforceable and who view a lawsuit as unlikely continue to consider their noncompete to be a factor in deciding whether to take a job offer at a competitor. This result suggests that moral, reputational, and perhaps financial costs remain for violating even entirely unenforceable contract provisions. We also show that stronger beliefs in enforceability cause employees to be more concerned about their noncompete when considering an offer from a competitor, and we present evidence that this effect may be due in part to perceptions that a lawsuit is more likely. At the same

time, noncompete-bound employees appear no more likely to negotiate over the terms of their non-compete or for other benefits in exchange for agreeing not to compete when they believe their non-compete is enforceable.

Our study has several limitations. 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 collecting and analyzing the long-term outcomes of similar information experiments. Second, our experiment is convoluted in its design and specific to the context of a survey. To the extent that the medium and the specifics of the language itself were responsible for our findings (or lack thereof), our results may not extend to other types of educational campaigns (Armantier et al. 2016, Hertwig et al. 2014). Third, our study is one about *employee* beliefs. We know little about what employers know about the law and how their beliefs matter (or do not) for their choices. Finally, while we took great pains to clean and weight our data appropriately, our analysis nevertheless builds on a selected sample. Future work should examine these issues using alternative samples.

Our empirical results contribute to the important and growing literature on postemployment restrictive covenants. This body of work relies mostly on the legal enforceability of noncompetes, exploiting bans or other smaller changes in noncompete laws at the state level (Marx et al. 2009, Garmaise 2009, Balasubramanian et al. 2022, Lipsitz and Starr 2022, Johnson et al. 2020, Jeffers 2020). One goal of this article is to emphasize that researchers pay too little attention to the impact of *unenforceable* noncompetes and the role of individual beliefs about the law (Starr et al. 2020). Our work stresses—with respect to noncompete research as well as all research examining state policy shocks without accounting for underlying beliefs—that voiding contracts in court *ex post* may have little practical effect if employees continue to believe that anything that appears in a contract must be enforceable (Chetty 2015). As a result, studies examining bans on noncompetes (Balasubramanian et al. 2022, Marx et al. 2009, Marx et al. 2015, Lipsitz and Starr 2022) that assume such bans end the use of noncompetes may understate the effect of noncompetes since (a) employers may still use noncompetes and (b) employees may still view these noncompetes as enforceable.

As a result, policymakers and antitrust agencies (Posner 2020) concerned about the potential ill effects of (unenforceable) noncompetes may need to consider reforms that induce employers to reduce the *use* of noncompetes in the first place as opposed to policies that limit their enforceability in court or simply inform employees that they are unenforceable (since at least some employees seem likely to continue to adhere to them). Two natural options include statutory penalties for inappropriate noncompete use or requiring employers to pay former employees during the prohibition period (known as garden leave). Oregon, for example, adopted garden leave in 2008 (see Lipsitz and Starr 2022) and Virginia's recent noncompete law (Va. Code Ann. § 40.1-28.7:8) requires employers to pay \$10,000 for each illegal noncompete. Both of these policies are not without their challenges, however—employers may skirt paying garden leave, and it may be difficult to identify employers using

unenforceable noncompetes. A third approach, recently highlighted in California, is for state bars to view using unenforceable contractual clauses as unethical, which may encourage lawyers to actively eliminate such restrictions (Gerstein and Shearer 2019). The effectiveness of each of these approaches in deterring the use of unenforceable provisions is an important avenue for future research.

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## Tables

Table 1. Summary Statistics By Actual Enforceability

	(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)		Arizona, Arkansas, Indiana, Louisiana, Maryland, Minnesota, Montana, Nebraska, New Mexico, North Carolina, Oregon, Pennsylvania, Rhode Island, South Carolina, Texas, Utah, Vermont, Virginia, West Virginia, Washington, Wisconsin, Wyoming		Alabama, Alaska, Colorado (Professionals), Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Iowa, Kansas, Kentucky, Maine, Massachusetts, Michigan, Mississippi, Missouri, Nevada, New Hampshire, New Jersey, New York, Ohio, South Dakota, Tennessee	
<i>Sample</i>	Full Sample	Noncompete Sample	Full Sample	Noncompete Sample	Full Sample	Noncompete Sample
Observations	1,484	205	4,376	685	5,645	857
Age	40.51	42.43	40.11	39.33	40.48	40.45
Hours Worked Per Week	39.25	42.44	37.24	40.61	37.34	41.50
Weeks Worked Per Year	48.79	49.84	47.90	47.46	47.41	48.65
1(Male)	0.56	0.72	0.52	0.55	0.52	0.56
1(Multi-Unit Employer)	0.64	0.78	0.64	0.77	0.62	0.67
1(Employer > 1K Employees)	0.39	0.49	0.38	0.45	0.37	0.40
1(Highest Degree is ≥ BA)	0.44	0.68	0.27	0.47	0.30	0.52
Pre-Experiment P(Enforce)	0.43	0.46	0.42	0.40	0.43	0.44

Note. We present sample means for each sample, cut by actual noncompete enforceability.

Table 2. Beliefs about the Locus of Noncompete Enforcement Policy

	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
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	0.24	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. Survey Question: "Non-competition policy is determined at what level?" The table displays percentages that sum to 100% within each column. Education level refers to employee's highest educational degree (BA = bachelor's degree).

Table 3. Beliefs about Noncompete Enforceability in Employee's 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
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
Misinformed	0.11	0.10	0.13	0.15	0.13	0.10	0.12
Informed	0.53	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 refers to employee's highest educational degree. Uninformed includes those respondents who do not know, while misinformed includes those who select the wrong policy. We consider California, Oklahoma, and North Dakota to be states that do not enforce noncompetes. All others enforce them (to some degree).

Table 4. Balance Test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Panel A: Full Sample of Individuals with a Noncompete</i>									
	No Info	Info	<i>p</i> -value						
Age	41.87	41.47	0.51						
Hours Worked Per Week	42.16	42.59	0.38						
Weeks Worked Per Year	48.63	48.91	0.33						
1(Male)	0.53	0.58	0.05						
1(Multi-Unit Employer)	0.74	0.72	0.21						
1(Employer > 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 Actual Enforceability</i>									
	No Enforceability			Medium Enforceability			High Enforceability		
	No Info	Info	<i>p</i> -value	No Info	Info	<i>p</i> -value	No Info	Info	<i>p</i> -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	42.35	0.71	41.90	43.02	0.12
Weeks Worked Per Year	48.21	49.42	0.18	48.65	48.79	0.76	48.71	48.88	0.69
1(Male)	0.58	0.59	0.94	0.49	0.56	0.07	0.55	0.58	0.31
1(Multi-Unit Employer)	0.76	0.77	0.82	0.77	0.74	0.34	0.72	0.69	0.31
1(Employer > 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

Note. Our sample is limited to 1,747 individuals who have a noncompete. The *p*-value column reports the results of a test of the null hypothesis of no mean difference between the information and no-information groups. We construct these unweighted comparisons using Stata's "orth\_out" command.

Table 5. Instrumenting for Post-Experiment Enforceability Beliefs

	(1)	(2)	(3)	(4)	(5)
Model: 2SLS	$\mathbb{1}(\text{Current Noncompete Limits Future Job Options})$	$\mathbb{1}(\text{Noncompete Is a Factor in Joining Competitor})$	$\mathbb{1}(\text{Noncompete Is a Factor in Starting Competitor})$	$\mathbb{1}(\text{Employee Would Negotiate Over Noncompete})$	
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 Noncompete
Controls	Yes	Yes	Yes	Yes	Yes
Pre-Experiment Dependent Variable	Yes	Yes	Yes	Yes	No
Observations	1,747	1,747	1,747	1,709	9,758
F-Stat	54.29	51.49	50.25	51.64	51.86
Mean of Dependent Variable	0.233	0.415	0.523	0.603	0.744

Note. We report robust standard errors in parentheses, clustered at the state level. Our sample for columns (1)–(4) is limited to individuals with a noncompete, while column (5) focuses on those without a noncompete. All models except for column (5) include main effects of the pre-experiment measure of the particular dependent variable, which we measure a second time after the experiment (both for those who do and do not receive enforceability 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%, indicators for living in a no, medium, or high enforceability state, and whether the individual randomly receives information. Controls include pre-experiment beliefs about enforceability, indicators for enforceability (no, medium, high) interacted with an indicator for pre-experiment enforceability beliefs being greater than 50% (as in the instrument), and other demographics we describe in text. The F-Stat reports the Kleibergen-Paap Wald rk F statistic, which tests for weak instruments with clustered standard errors.

\*\*  $p < .01$ .

Table 6. The Mediating Effect of the Likelihood of Lawsuit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	P(Employer Would Sue Over Noncom- pete if Violated)		1(Noncompete Is a Factor in Joining Competitor)				
Post-Experiment P(Enforce)	0.411** (0.087)			0.578** (0.036)	0.390** (0.051)	0.659** (0.127)	0.443* (0.191)
Post-Experiment P(Lawsuit)			0.570** (0.056)		0.287** (0.071)		-0.363 (0.389)
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	42.67					51.49	
Mean of Dependent Variable	0.389	0.415	0.415	0.415	0.415	0.415	0.415
% of Main Effect Driven by P(Lawsuit)			40.5		32.5		32.8

Note. We report robust standard errors in parentheses, clustered at the state level. Our sample is limited to individuals with a noncompete. The instrument for post-experiment beliefs is a three-way interaction of an indicator for pre-experiment beliefs about enforceability being greater than 50%, an indicator for living in a no, medium, or high enforceability state, and whether the individual randomly receives information. Controls include pre-experiment beliefs about enforceability, indicators for enforceability (no, medium, high) interacted with an indicator for pre-experiment enforceability beliefs greater than 50% (as in the instrument), and other demographics we describe in text. The F-Stat reports the Kleibergen-Paap Wald rk F statistic, which tests for weak instruments with clustered standard errors. For column (7), we apply the IV Mediation analysis recommended by Dippel et al. (2020).

+ $p < .10$ .

\*  $p < .05$ .

\*\*  $p < .01$ .

Figures

Figure 1. Accuracy of noncomplete enforceability beliefs by actual enforceability

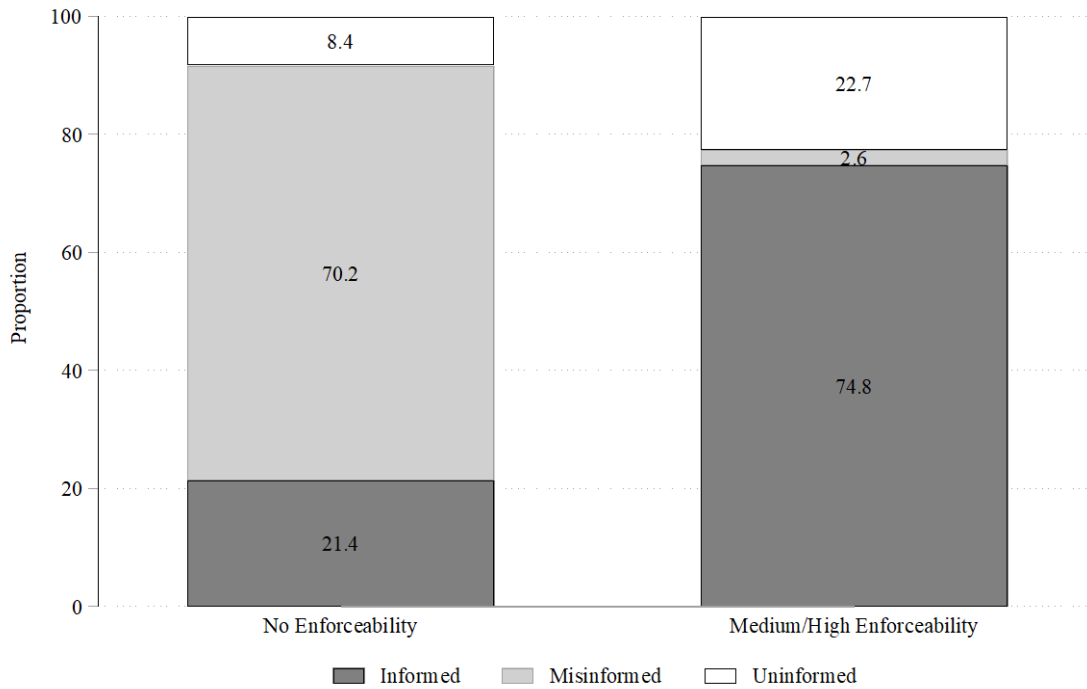


Figure 2. Accuracy of noncomplete enforceability beliefs by actual enforceability and education

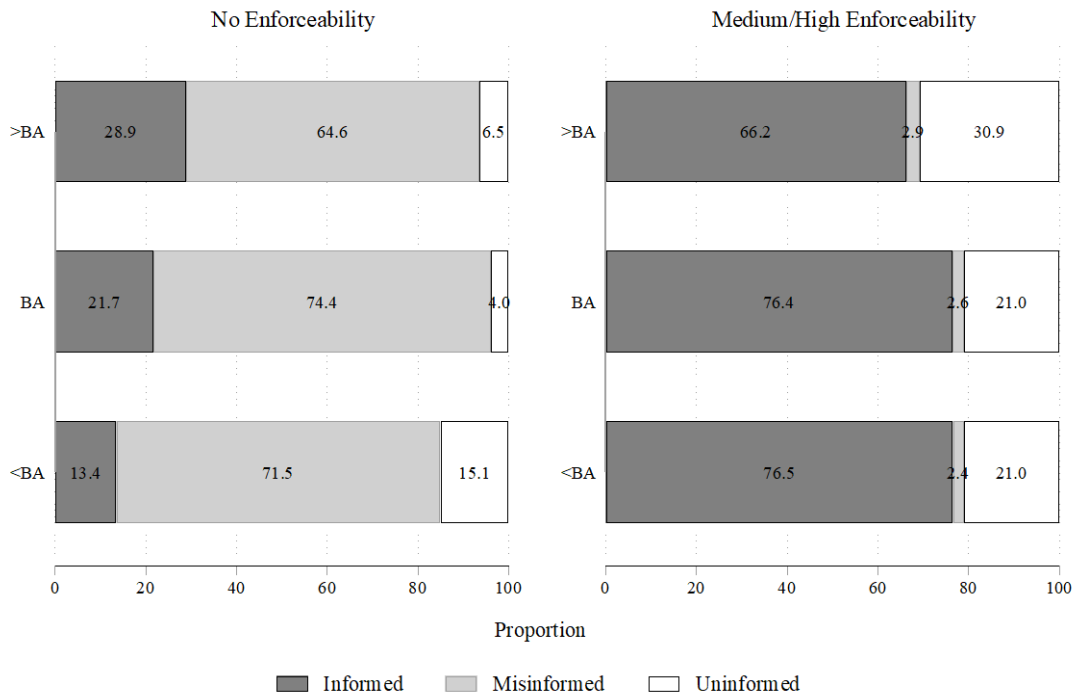


Figure 3. Categorical and continuous beliefs about noncompete enforceability

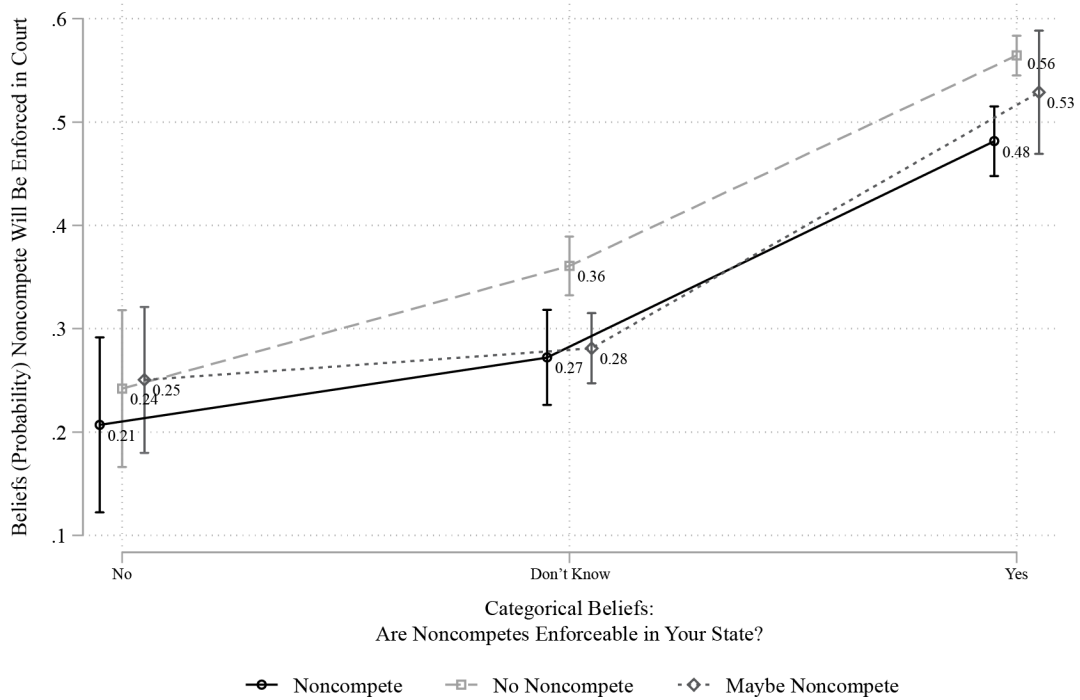


Figure 4. Noncompete enforceability beliefs by actual enforceability and noncompete status

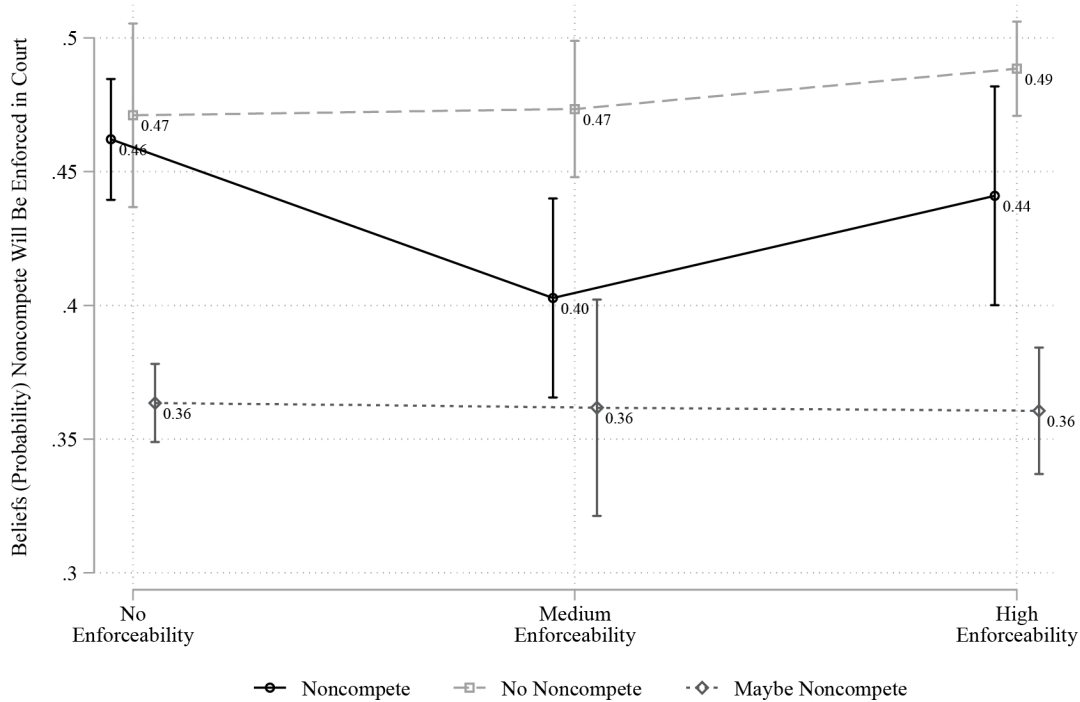




Figure 5. Noncomplete enforceability beliefs held by individuals with a noncomplete by actual enforceability and education

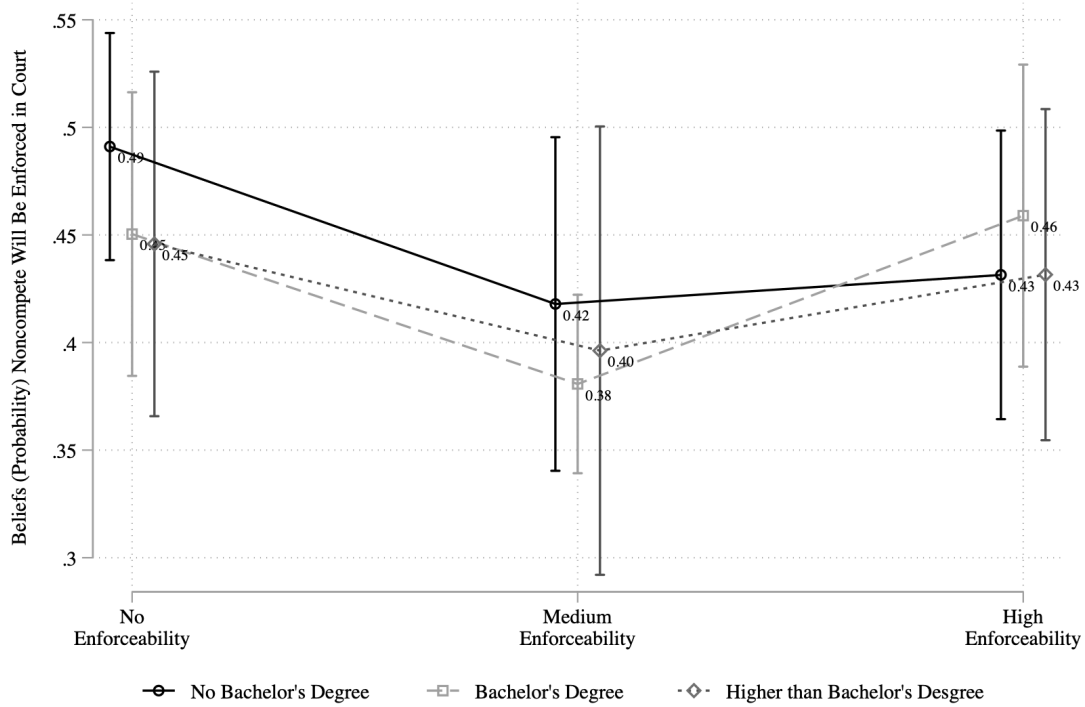


Figure 6. Search effort toward competitors and noncomplete enforceability beliefs

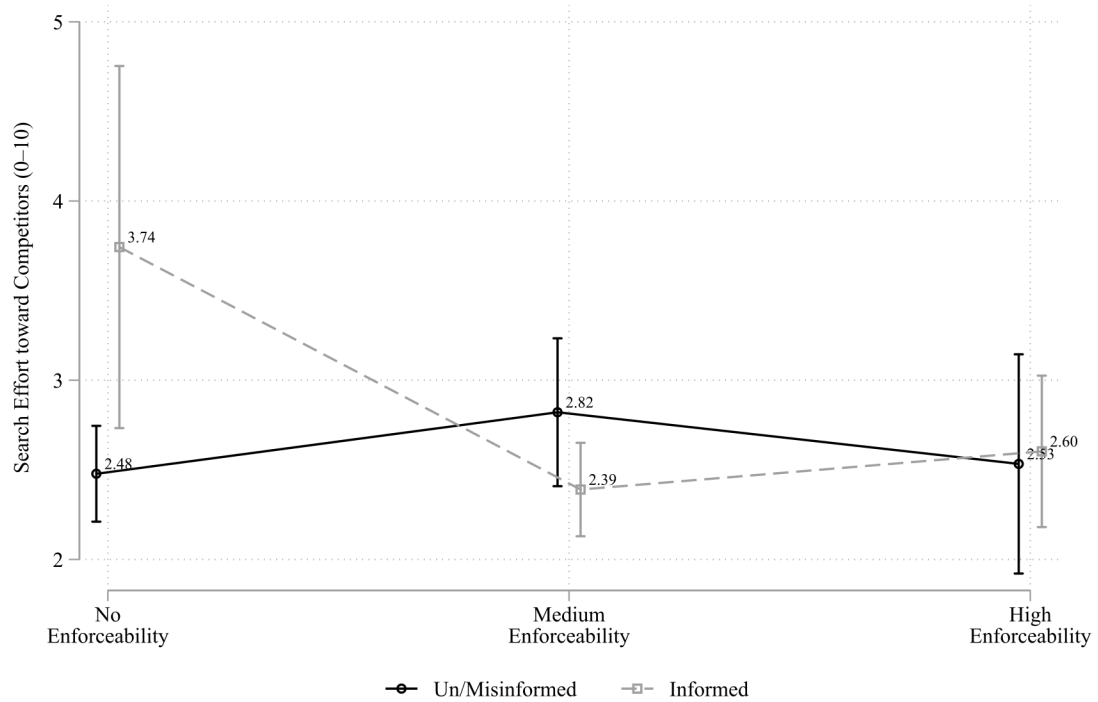


Figure 7. Noncompete enforceability beliefs by actual enforceability and competitor-offer receipt

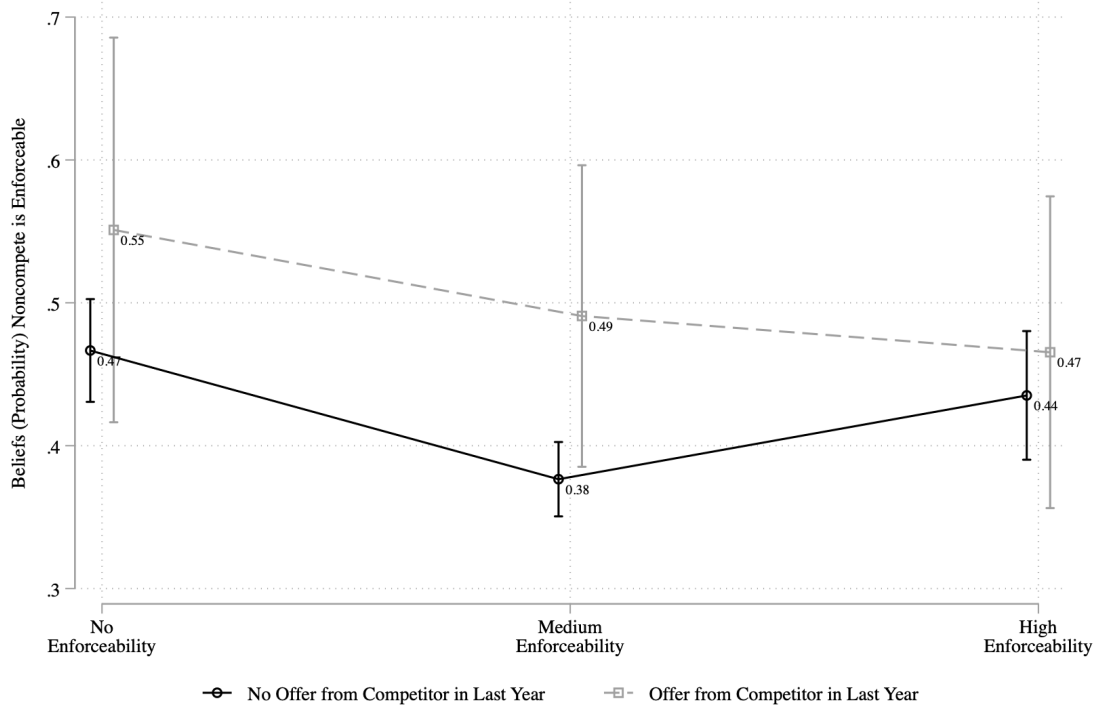


Figure 8. Probability employer reminded employee about noncompete by actual enforceability

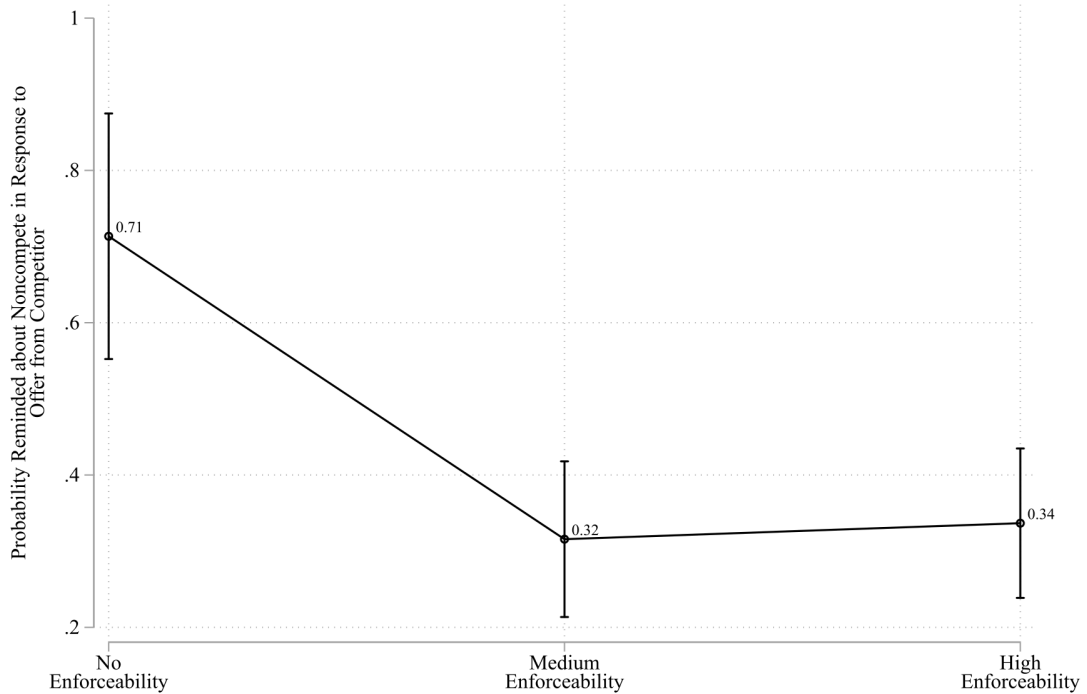


Figure 9. Reminders and beliefs about noncompete enforceability

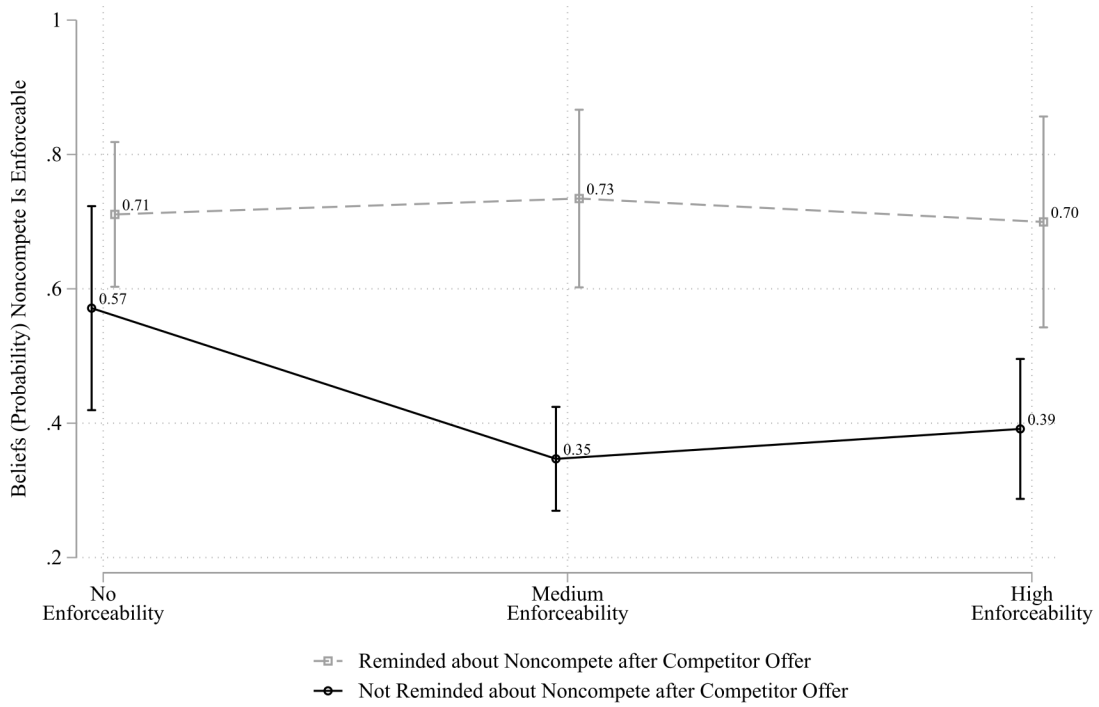


Figure 10. Distribution of noncompete enforceability beliefs before and after experiment

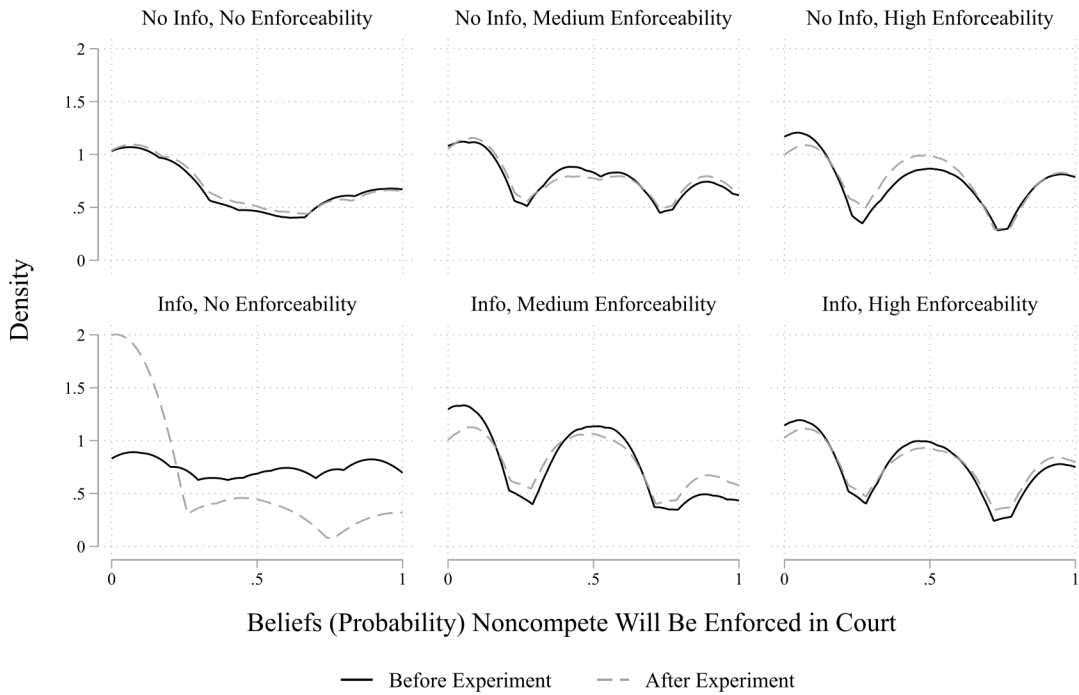


Figure 11. Average post-experiment beliefs by actual enforceability and treatment status

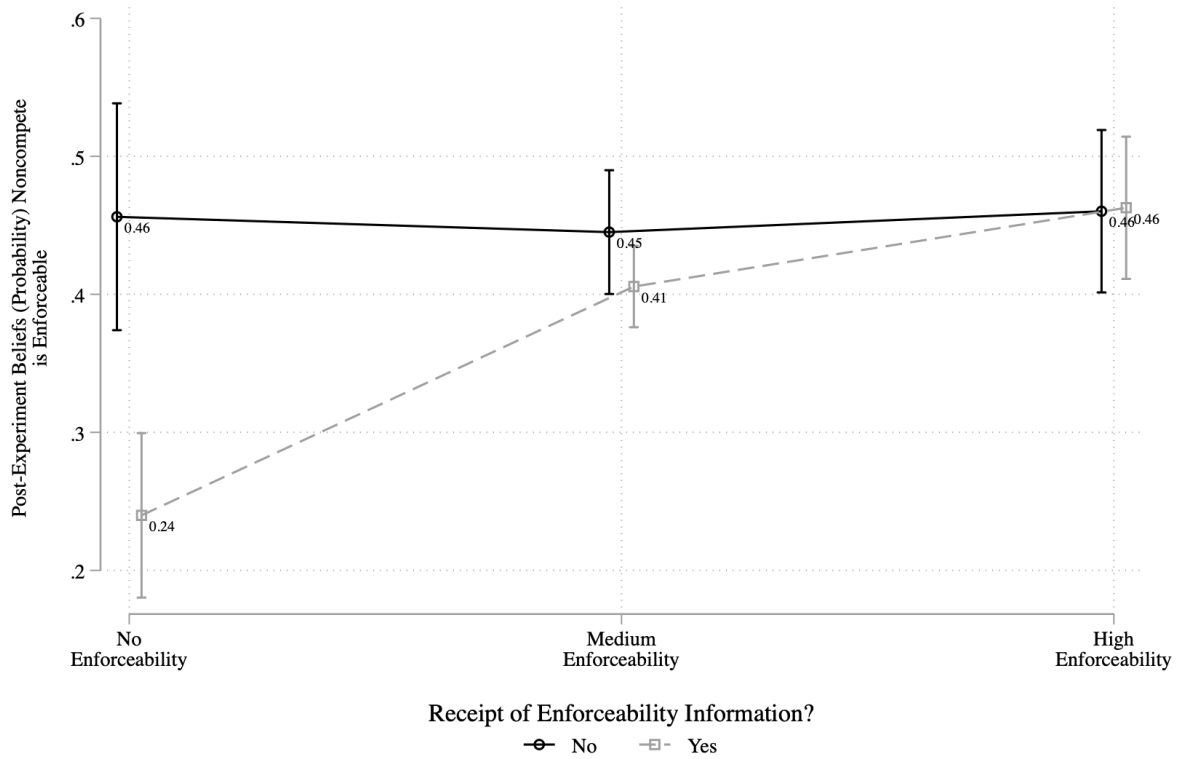


Figure 12. Relationship between pre-experiment and post-experiment beliefs

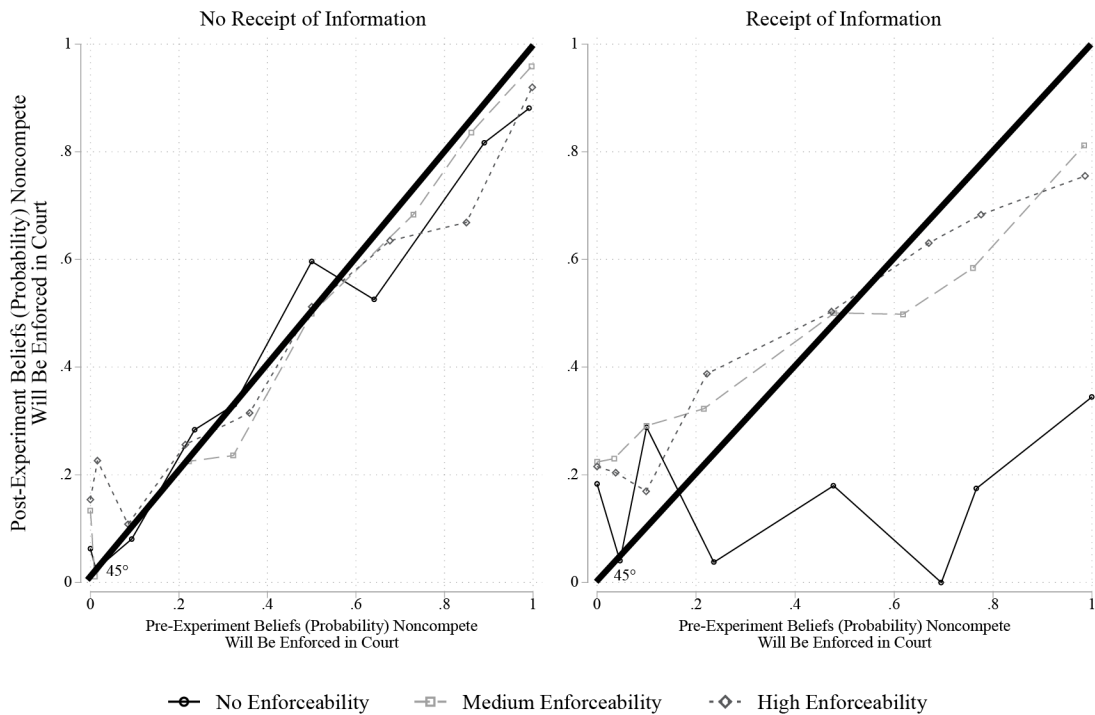


Figure 13. Heterogeneity in post-experiment beliefs and pre-experiment beliefs among employees with a noncompete

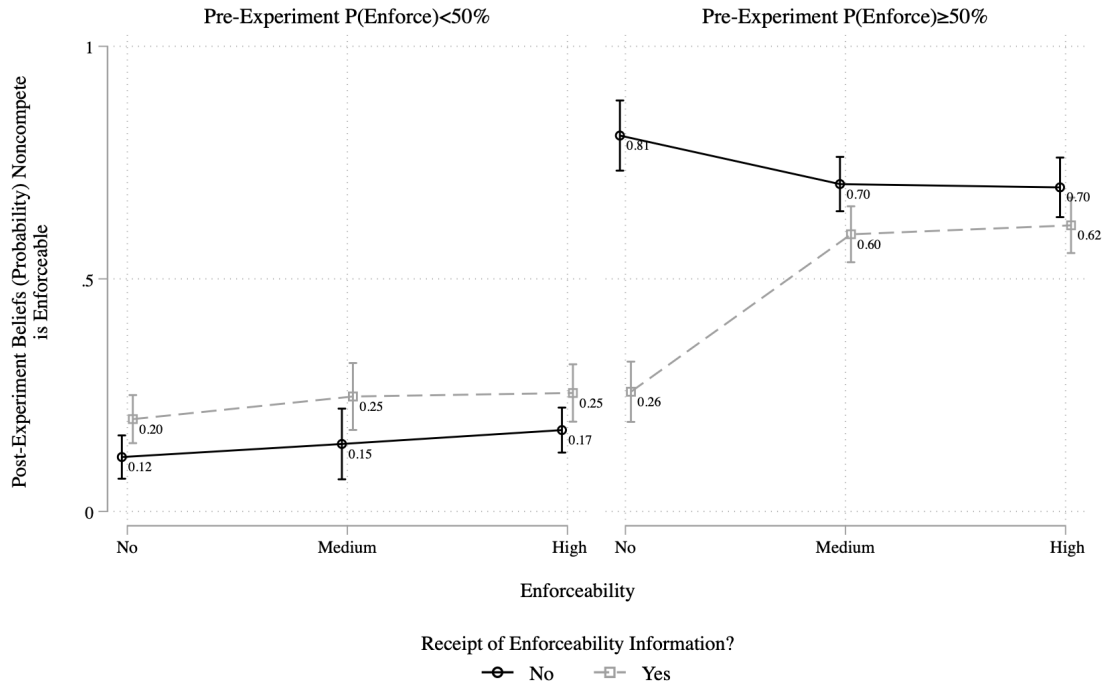


Figure 14. Noncompete as a factor in leaving by noncompete enforceability and treatment status

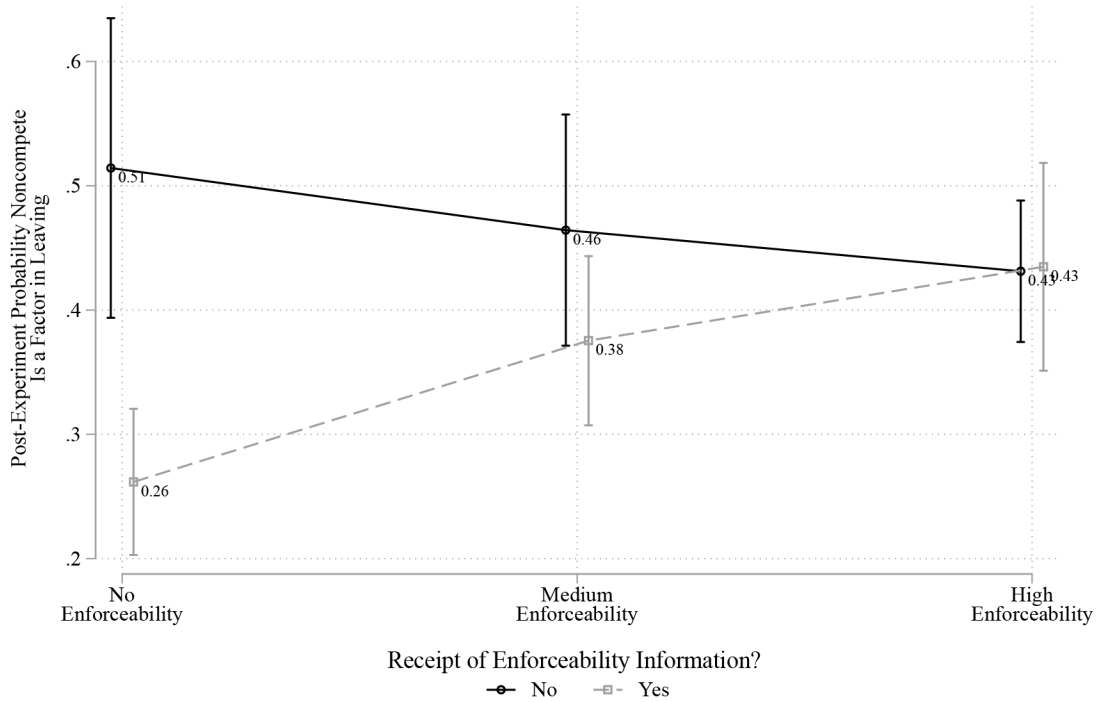


Figure 15. Post-experiment heterogeneity in noncompete as a factor in leaving by pre-experiment answer

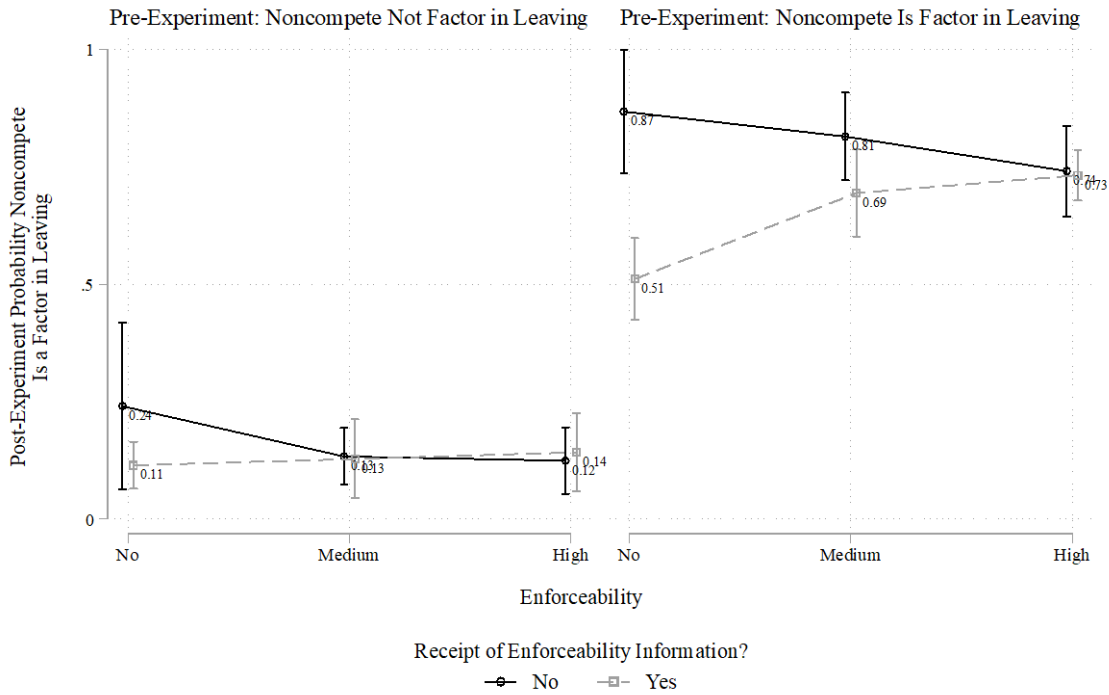


Figure 16. Negotiation over noncompetes and noncompete enforceability among employees with a noncompete

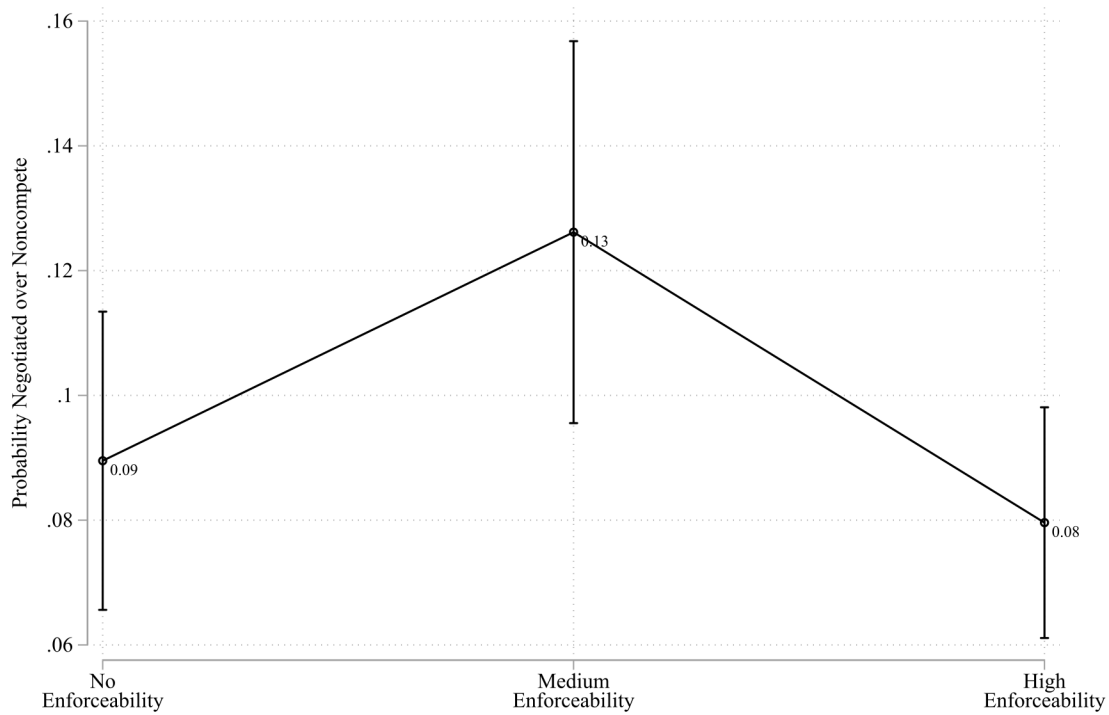
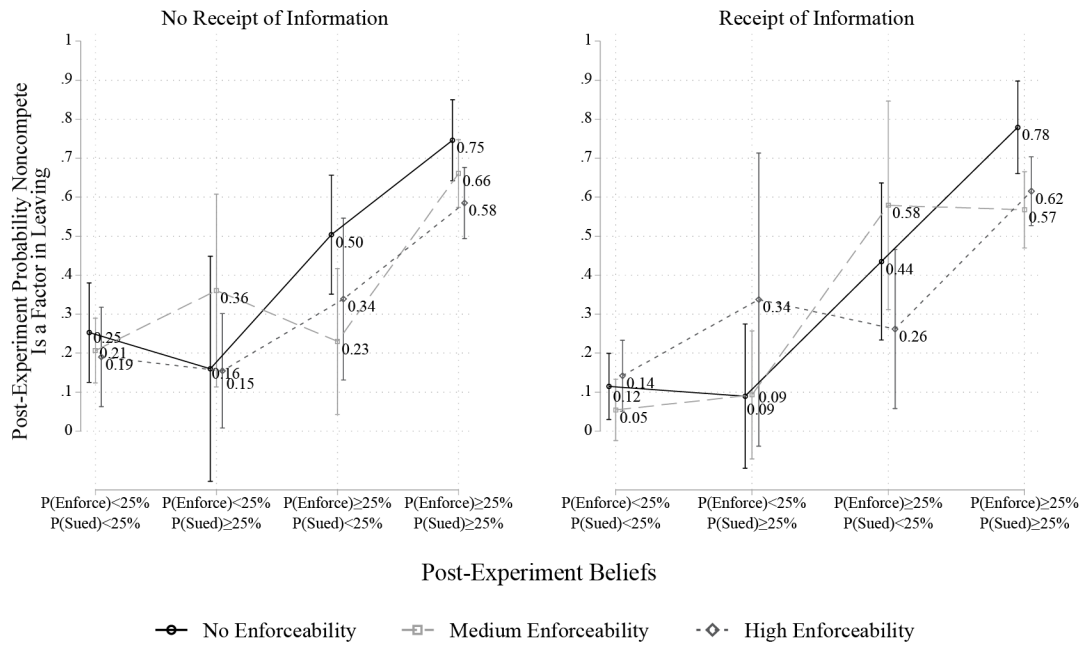


Figure 17. Noncompete as a factor in leaving by beliefs about enforceability and likelihood of lawsuit



## Online Appendix A. Additional Figures and Tables

Table OA1. Noncompete Policies by State

Score		
<i>Panel A. Handling of Overbroad Covenants</i>		
1	Rewrite unreasonably overbroad non-compete terms to make the terms reasonable and enforce the revised noncompete against the employee	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 a noncompete contract but enforce the rest of the provision	Arizona, Indiana, Louisiana, Maryland, Montana, North Carolina, Rhode Island
0	Refuse to enforce a noncompete against an employee if <i>any</i> part of the contractual provision is unreasonably overbroad	Arkansas, Nebraska, South Carolina, Virginia, Wisconsin
<i>Panel B. Enforce if Employee is Terminated Without Cause?</i>		
1	Enforce a noncompete against an employee even when the employee is terminated from their job 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	Refuse to enforce a noncompetes against an employee unless the employee voluntarily leaves their job or is terminated without cause	DC, Maryland, Montana
<i>Panel C. Enforcement Dependent on Consideration?</i>		
1	Enforce a noncompete against an employee even if the employee <i>only</i> received continued employment in exchange for agreeing to the noncompete	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	Refuse to enforce a noncompete against an employee unless the employee is given additional consideration (such as additional compensation, training, or other	Minnesota, Montana, North Carolina, Oregon, Pennsylvania, South Carolina, Texas, Washington, West Virginia, Wisconsin, Wyoming



benefits) *beyond* continued employment in exchange for agreeing to the noncompete

0 Refuse to enforce a noncompete against an employee if the employer did not notify the employee at least 14 days before the start of employment that the employer would request the noncompete

Oregon

*Panel D. Exemptions*

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1 Enforce a noncompete against an employee *only* if the employee is an executive or management-level employee or related professional staff

Colorado

0 Refuse to enforce (or be very unlikely to enforce) a noncompete against an employee who is a physician

Arizona, Colorado, Delaware, Illinois, Massachusetts, Tennessee, Texas

0 Refuse to enforce a noncompete against an employee who leaves to join or start a competing business, regardless of the circumstances

California, North Dakota

0 Refuse to enforce a noncompete against an employee who leaves to join or start a competing business but restrict the ability of the employee to directly solicit clients from their former employer

Oklahoma

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Note. We report the actual language we use in the experimental treatment in Figure OA6. We derive this classification from Beck (2014). See Online Appendix C for more details. The overall measure of enforceability adds each score for each state and adds an additional one (1) point for states that enforce noncompetes under any circumstances. As a result, the maximum score a state can receive is four (4). We normalize this measure by dividing by the maximum score for each state, such that nonenforcing states (or nonenforcing state-occupation combinations) receive a score of zero (0) and states that robustly enforce noncompetes receive a score of one (1).

Table OA2. Enforceability Beliefs by Categorical Beliefs, Noncompete Status, and Education

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
Probability Noncompete Enforced	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	0.066	0.155	0.022	0.048	0.006	0.0967

Note. We report standard errors in parentheses, clustered at the state level, using least squares estimation. Our sample is limited to individuals with a noncompete for columns (1), (2), (5), and (6). 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. Mean R-Squared is the mean of R-Squared statistics generated by our multiple-imputation analysis as we explain in Online Appendix B.

+  $p < .10$ .

\*  $p < .05$ .

\*\*  $p < .01$ .

Table OA3. Search Effort and the Receipt of Job Offers from Competitors

Model: OLS	(1)	(2)	(3)	(4)
	Search Effort Toward Competitor		P(Enforce)	
Constant	2.759** (0.131)	-0.442 (3.988)	0.459** (0.011)	0.766 (0.548)
1(Medium Enforceability)	-0.324 (0.343)	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(Information)	1.535** (0.350)	1.265* (0.476)		
1(Medium Enforceability) × 1(Information)	-1.651** (0.486)	-1.696** (0.486)		
1(High Enforceability) × 1(Information)	-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 R-Squared	0.014	0.178	0.012	0.102
Mean of Dependent Variable	2.573	2.573	0.428	0.428

Note. We report robust standard errors in parentheses, clustered at the state level. Our sample is limited to individuals with a noncompete. 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. Mean R-Squared is the mean of R-Squared statistics generated by our multiple-imputation analysis as we explain in Online Appendix B.

\*  $p < .05$ .

\*\*  $p < .01$ .

Table OA4. Reminders and Lawsuits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Model: OLS	1(Employer Reminded Employee about Noncompete)		P(Enforce)		1(Employee Aware of Employer Suing Oth- ers Over 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 Reminded about Noncompete)			0.331** (0.088)	0.140 (0.098)				
1(Medium Enforceability) × 1(Noncompete Re- minder)			-0.074 (0.123)	0.248* (0.115)				
1(High Enforceability) × 1(Noncompete Reminder)			-0.052 (0.196)	0.169 (0.130)				
1(Employee Aware of Other Suits)							0.224** (0.045)	0.280** (0.041)
1(Medium Enforceability) × 1(Employee Aware of Other Suits)							-0.092 (0.080)	-0.142* (0.060)
1(High Enforceability) × 1(Employee Aware of Other Suits)							-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 R-Squared	0.034	0.522	0.151	0.601	0.001	0.141	0.038	0.129
Mean of Dependent Variable	0.392	0.392	0.519	0.519	0.216	0.216	0.428	0.428

Note. We report robust standard errors in parentheses, clustered at the state level. Our sample is limited to individuals with a noncompete. Our sample for columns (1)–(4) is limited to individuals with a noncompete who received job offers from competitors. 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. Mean R-Squared is the mean of R-Squared statistics generated by our multiple-imputation analysis as we explain in Online Appendix B.

\*  $p < .05$ .  
\*\*  $p < .01$ .

Table OA5. Information Experiment and Post-Experiment Beliefs About Enforceability

	(1)	(2)	(3)	(4)
Model: OLS	Post-Experiment Beliefs P(Enforce)			
Constant	0.418** (0.040)	0.619 (0.384)	0.101** (0.018)	-0.015 (0.307)
$\mathbb{1}(\text{Medium Enforceability})$	0.026 (0.047)	-0.011 (0.047)	0.051 (0.040)	0.028 (0.051)
$\mathbb{1}(\text{High Enforceability})$	0.045 (0.049)	0.004 (0.053)	0.076+ (0.039)	0.058+ (0.031)
$\mathbb{1}(\text{Information})$	-0.215** (0.032)	-0.216** (0.034)	0.068* (0.026)	0.082* (0.038)
$\mathbb{1}(\text{Medium Enforceability}) \times \mathbb{1}(\text{Information})$	0.202** (0.037)	0.177** (0.039)	0.041 (0.075)	0.020 (0.084)
$\mathbb{1}(\text{High Enforceability}) \times \mathbb{1}(\text{Information})$	0.214** (0.045)	0.219** (0.051)	0.022 (0.070)	-0.002 (0.058)
$\mathbb{1}(\text{P(Enforce)} \geq 50\%)$			0.665** (0.032)	0.691** (0.044)
$\mathbb{1}(\text{P(Enforce)} \geq 50\%) \times \mathbb{1}(\text{Medium Enforceability})$			-0.109+ (0.055)	-0.132+ (0.076)
$\mathbb{1}(\text{P(Enforce)} \geq 50\%) \times \mathbb{1}(\text{High Enforceability})$			-0.142* (0.059)	-0.169** (0.063)
$\mathbb{1}(\text{P(Enforce)} \geq 50\%) \times \mathbb{1}(\text{Information})$			-0.604** (0.046)	-0.632** (0.066)
$\mathbb{1}(\text{P(Enforce)} \geq 50\%) \times \mathbb{1}(\text{Medium Enforceability}) \times \mathbb{1}(\text{Information})$			0.380** (0.109)	0.422** (0.126)
$\mathbb{1}(\text{P(Enforce)} \geq 50\%) \times \mathbb{1}(\text{High Enforceability}) \times \mathbb{1}(\text{Information})$			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	0.039	0.122	0.400	0.460
Mean of Dependent Variable	0.425	0.425	0.425	0.425

Note. We report robust standard errors in parentheses, clustered at the state level. Our sample is limited to individuals with a noncomplete. The independent variable  $\mathbb{1}(\text{P(Enforce)} \geq 50\%)$  is the pre-experiment measure. 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. Mean R-Squared is the mean of R-Squared statistics generated by our multiple-imputation analysis as we explain in Online Appendix B.

+  $p < .10$ .

\*  $p < .05$ .

\*\*  $p < .01$ .

Table OA6. Information Experiment and Noncompetes as a Factor in Moving to Competitor

Model: OLS	(1)	(2)	(3)	(4)
	Post-Experiment $\mathbb{1}(\text{Noncompete Factor in Moving})$			
Constant	0.467** (0.054)	2.721** (0.665)	0.194+ (0.106)	1.871** (0.560)
$\mathbb{1}(\text{Medium Enforceability})$	-0.015 (0.087)	-0.050 (0.079)	-0.089 (0.124)	-0.107 (0.093)
$\mathbb{1}(\text{High Enforceability})$	-0.015 (0.066)	-0.083 (0.065)	-0.057 (0.112)	-0.116 (0.092)
$\mathbb{1}(\text{Information})$	-0.251** (0.046)	-0.252** (0.064)	-0.105 (0.104)	-0.126 (0.103)
$\mathbb{1}(\text{Medium Enforceability}) \times \mathbb{1}(\text{Information})$	0.205* (0.098)	0.164* (0.077)	0.170 (0.121)	0.121 (0.109)
$\mathbb{1}(\text{High Enforceability}) \times \mathbb{1}(\text{Information})$	0.224** (0.063)	0.256** (0.081)	0.130 (0.130)	0.144 (0.124)
$\mathbb{1}(\text{Noncompete Factor in Moving})$			0.629** (0.089)	0.627** (0.071)
$\mathbb{1}(\text{Noncompete Factor in Moving}) \times \mathbb{1}(\text{Medium Enforceability})$			0.081 (0.104)	0.054 (0.086)
$\mathbb{1}(\text{Noncompete Factor in Moving}) \times \mathbb{1}(\text{High Enforceability})$			0.004 (0.117)	-0.011 (0.108)
$\mathbb{1}(\text{Noncompete Factor in Moving}) \times \mathbb{1}(\text{Information})$			-0.304** (0.104)	-0.230** (0.080)
$\mathbb{1}(\text{Noncompete Factor in Moving}) \times \mathbb{1}(\text{Medium Enforceability}) \times \mathbb{1}(\text{Information})$			0.116 (0.117)	0.115 (0.088)
$\mathbb{1}(\text{Noncompete Factor in Moving}) \times \mathbb{1}(\text{High Enforceability}) \times \mathbb{1}(\text{Information})$			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	0.019	0.150	0.372	0.464
Mean of Dependent Variable	0.415	0.415	0.415	0.415

Note. We report robust standard errors in parentheses, clustered at the state level. Our sample is limited to individuals with a noncompete. The independent variable  $\mathbb{1}(\text{Noncompete Factor in Moving})$  is the pre-experiment measure. 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. Mean R-Squared is the mean of R-Squared statistics generated by our multiple-imputation analysis as we explain in Online Appendix B.

+  $p < .10$ .

\*  $p < .05$ .

\*\*  $p < .01$ .

Table OA7. Beliefs about Enforceability and the Importance of a Noncompete

	(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”					
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 exciting work”	“The increase in my compensation or other benefits”	“The location of the new job and other lifestyle 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 Dependent Variable	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,747	1,747	1,747	1,747	1,747	1,747
F-Stat	50.30	46.48	46.34	55.03	53.50	51.86
Mean of Dependent Variable	4.448	4.525	4.543	1.038	1.566	1.277

Notes. We report robust standard errors in parentheses, clustered at the state level. Our sample is limited to individuals with a non-compete. All models include main effects of the pre-experiment measure of the particular dependent variable, which we measure a second time after the experiment (both for those who do and do not receive enforceability 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%, indicators for living in a no, medium, or high enforceability state, and whether the individual randomly receives information. Controls include pre-experiment beliefs about enforceability, indicators for enforceability (no, medium, high) interacted with an indicator for pre-experiment enforceability beliefs being greater than 50% (as in the instrument), and other demographics we describe in text. The F-Stat reports the Kleibergen-Paap Wald rk F statistic, which tests for weak instruments with clustered standard errors.

\*\*  $p < .01$ .

Figure OA1. Noncompete enforceability in 2014 for contiguous United States

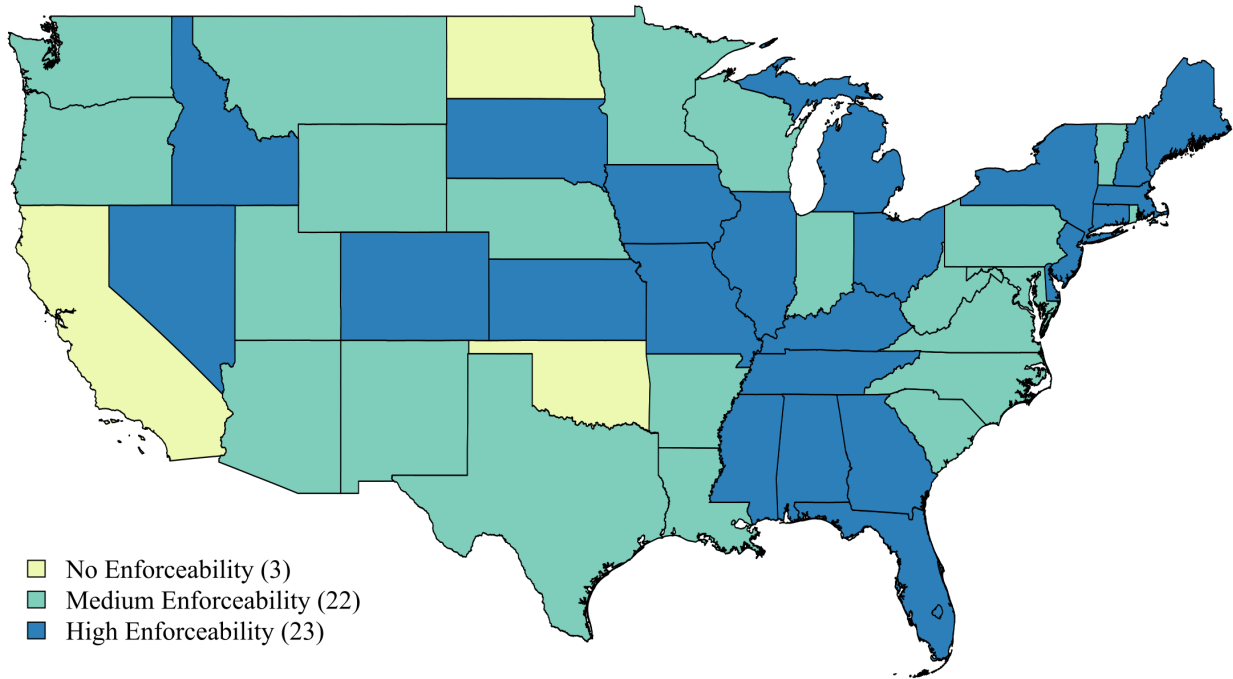


Figure OA2. Beliefs about noncompete enforceability in state by occupation

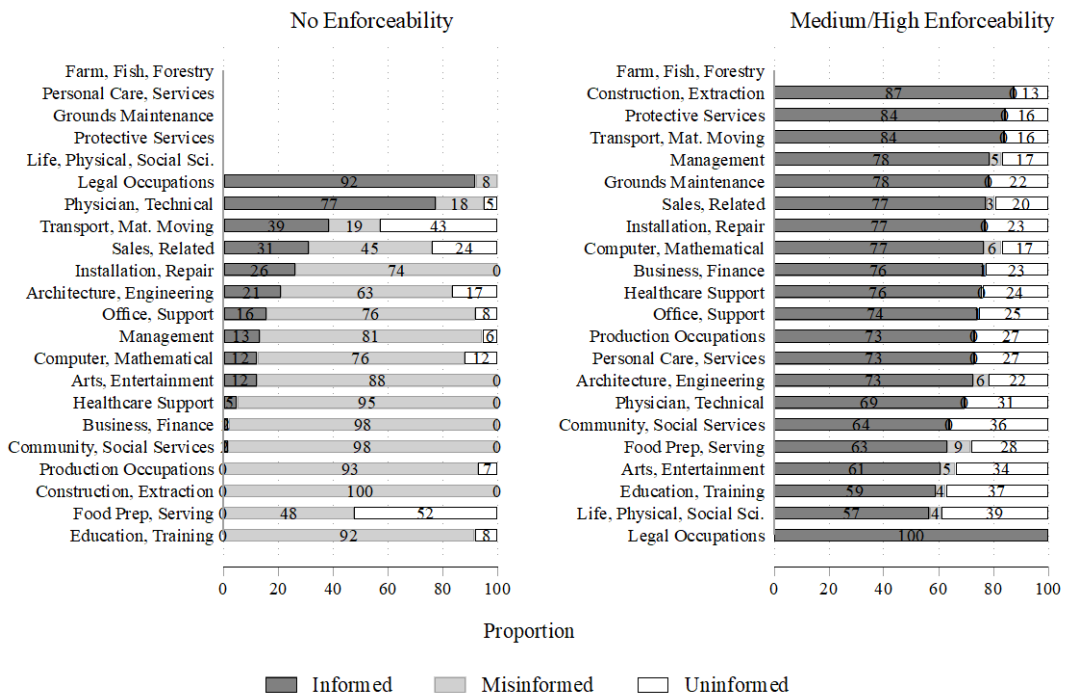
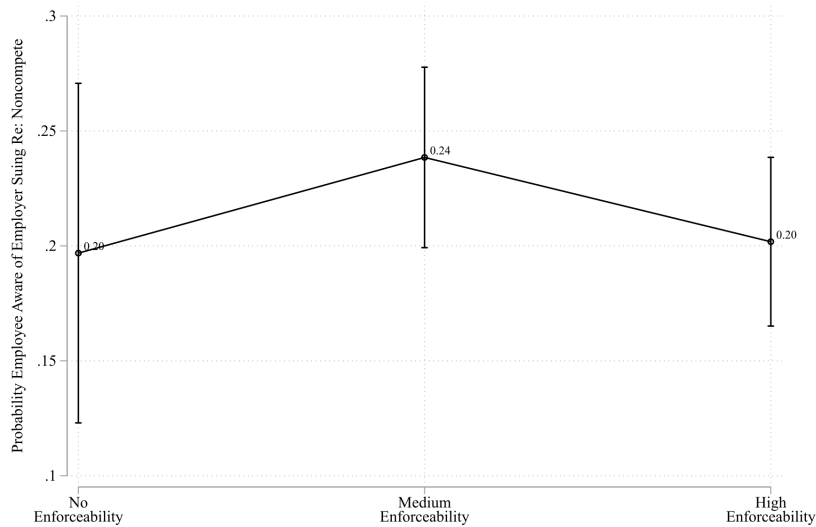


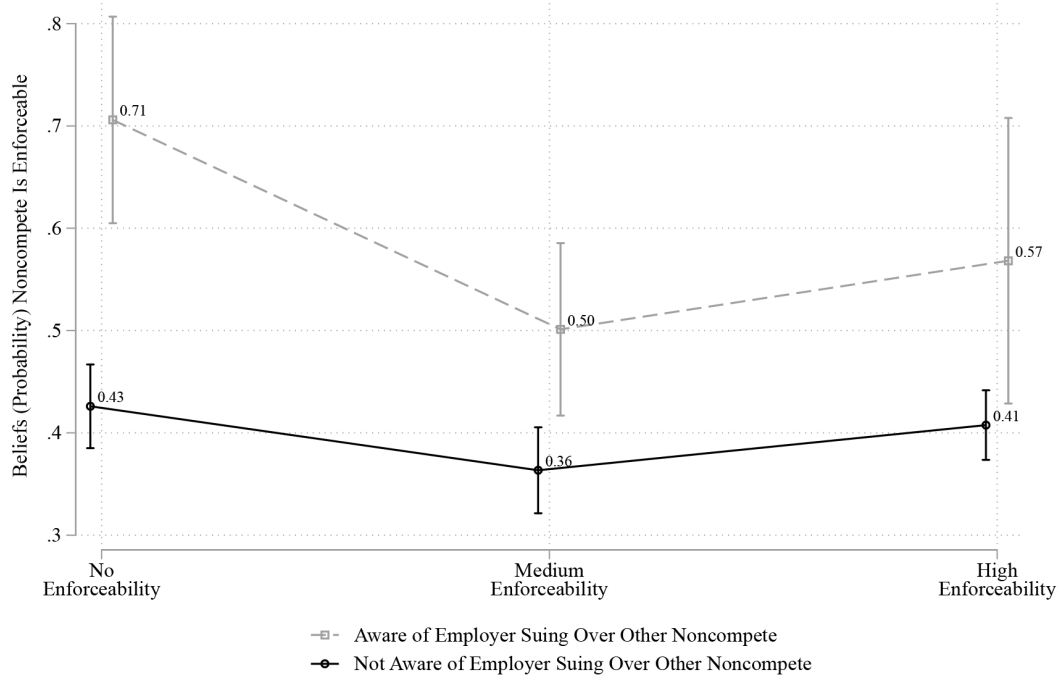


Figure OA3. Awareness that employer has sued others to enforce a noncompete



Note. The figure shows how actual noncompete enforceability relates to the likelihood that an employee reports their employer has legally pursued others for violating a noncompete. Our sample is limited to individuals with a noncompete. We present results as predicted values (with 95% confidence intervals) from a model with basic controls (corresponding to Table OA4 column (6)—see column (5) for an uncontrolled model), using sample weights.

Figure OA4. Awareness of other noncompete lawsuits and beliefs about enforceability



Note. The figure shows how employee beliefs about noncompete enforceability relate to the likelihood that an employee reports their employer has legally pursued others for violating a noncompete, cut by actual enforceability. Our sample is limited to individuals with a noncompete. We present results as predicted values (with 95% confidence intervals) from a model with basic controls, an interaction between awareness of respondent’s employer suing another over a noncompete and actual enforceability (corresponding to Table OA4 column (8)—see column (7) for an uncontrolled model), using sample weights.

Figure OA5. General noncompete enforceability information treatment

**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 OA6. State-specific noncompete enforceability information treatment

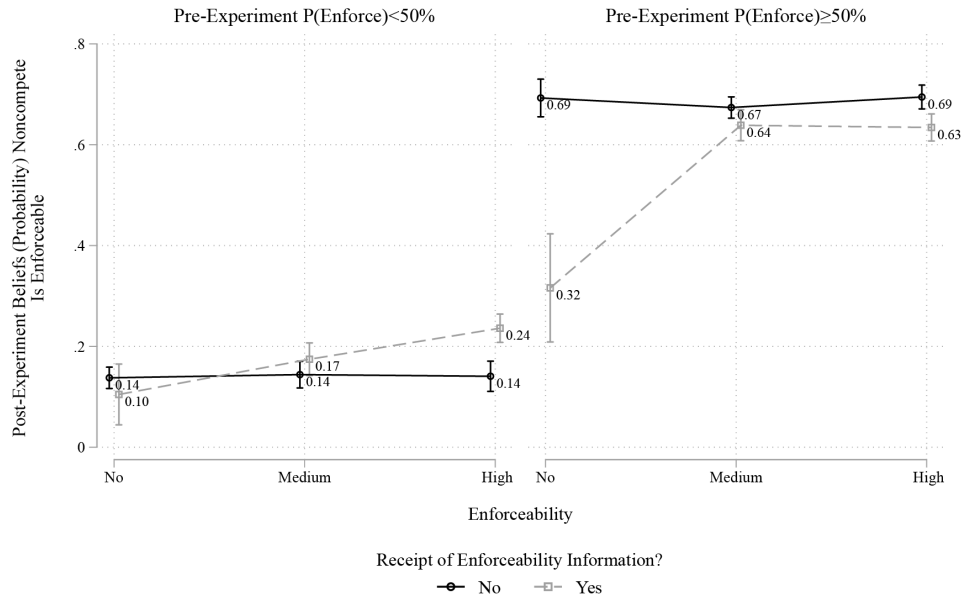
**Noncompete Enforcement Information Specifically For  $\{q://QID406/ChoiceGroup/SelectedChoices\}$** **Under  $\{q://QID406/ChoiceGroup/SelectedChoices\}$ 's noncompete policy, courts will typically:**

(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.

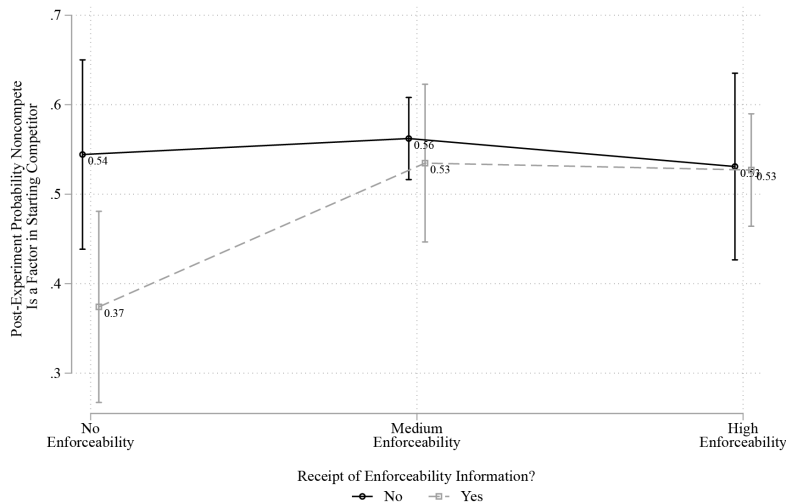
Note. Blue arrows indicate that the survey will only display the bullet point if the respondent's answers and demographics meet certain criteria. The survey shows the respondent only the bullet points that are relevant for a given respondent-selected state using the classification in Beck (2014).

Figure OA7. Heterogeneity in post-experiment beliefs and pre-experiment beliefs among employees without noncompetes



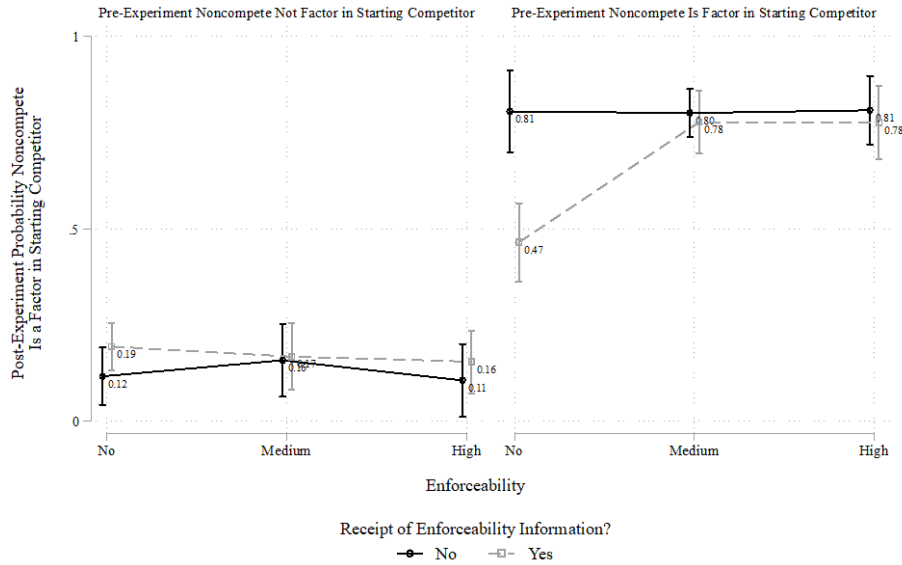
Note. The figure shows how average post-experiment beliefs about noncompete enforceability differ between those who receive enforceability information and those who do not receive information, cut by actual enforceability and by pre-experiment beliefs (above or below 50%). Our sample is limited to individuals without a noncompete. We present results as predicted values (with 95% confidence intervals) from a model with basic controls, a three-way interaction (with all the double interactions as well) between actual enforceability, an indicator for receiving information, and an indicator for pre-experiment beliefs about enforceability being greater than 50%, using sample weights.

Figure OA8. Noncompete as a factor in starting a competitor by actual enforceability and treatment status



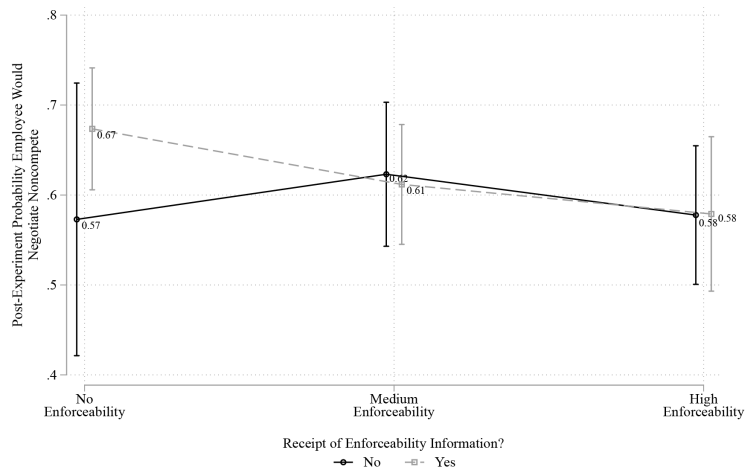
Note. The figure shows how the post-experiment likelihood a noncompete would be a factor in starting a competitor differs between those who receive enforceability information and those who do not receive information, cut by actual enforceability. Our sample is limited to individuals with a noncompete. We present results as predicted values (with 95% confidence intervals) from a model with basic controls and an interaction between receiving information and actual enforceability, using sample weights.

Figure OA9. Heterogeneity in noncompete as a factor in starting a competitor by pre-experiment answer



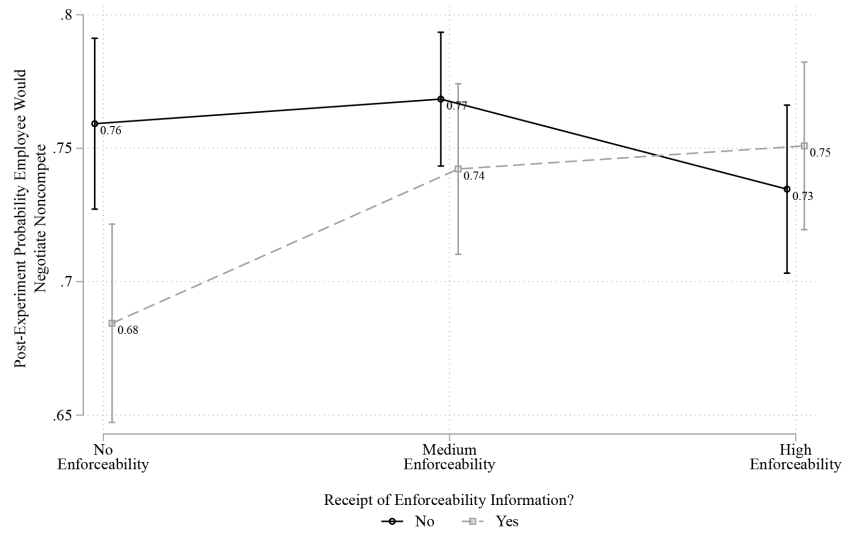
Note. The figure shows how the post-experiment likelihood a noncompete would be a factor in starting a competitor differs between those who receive enforceability information and those who do not receive information, cut by actual enforceability and by the respondent’s pre-experiment answer to the same question about whether their noncompete would be a factor in starting a competitor. Our sample is limited to individuals with a noncompete. We present results as predicted values (with 95% confidence intervals) from a model with basic controls, a three-way interaction (and all the double interactions) between actual enforceability, receiving information, and a pre-experiment indicator for whether the noncompete would be a factor in starting a competitor, using sample weights.

Figure OA10. Post-experiment negotiation over noncompetes by treatment status among employees with a noncompete



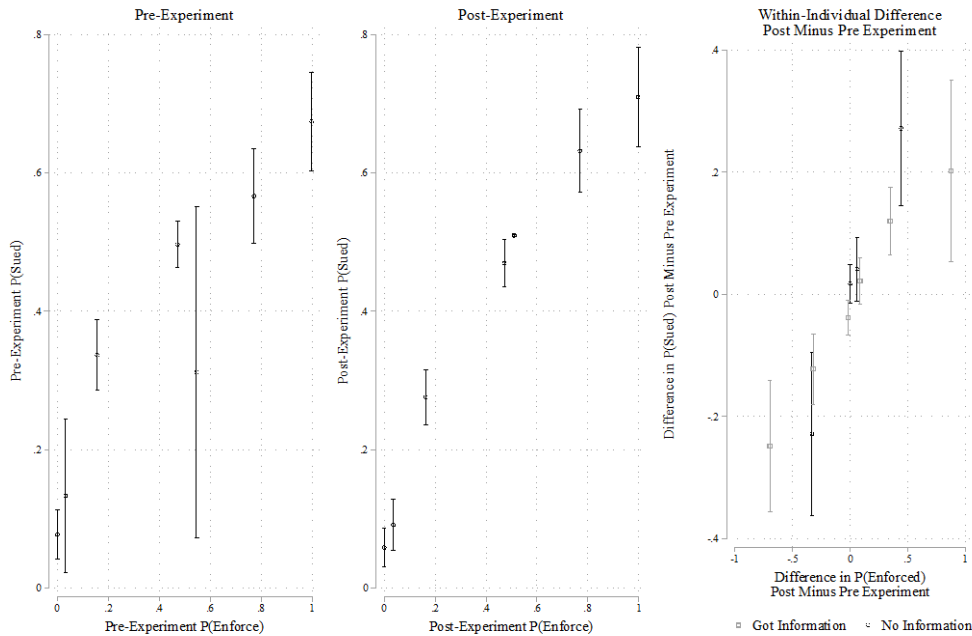
Note. The figure shows how the post-experiment likelihood of negotiating over a prospective noncompete differs between those who receive enforceability information and those who do not receive information, cut by actual enforceability. Our sample is limited to respondents without noncompetes. We present results as predicted values (with 95% confidence intervals) from a model with basic controls, an interaction between getting information and actual enforceability, using sample weights.

Figure OA11. Post-experiment negotiation over noncompetes by treatment status among employees without a noncompete



Notes. The figure shows how the post-experiment likelihood of negotiating over a prospective noncompete differs between those who receive enforceability information and those who do not receive information, cut by actual enforceability. Our sample is limited to individuals without noncompetes. We present result as predicted values (with 95% confidence intervals) from a model with basic controls and an interaction between receiving information and actual enforceability, using sample weights.

Figure OA12. Correlation between beliefs about the likelihood of enforceability and lawsuit



Notes. The figure shows the unconditional relationship between beliefs about the likelihood of enforceability and the average beliefs about the likelihood that a respondent's employer would legally pursue them if they violate the terms of their noncompete, before the experiment (left panel), after the experiment (middle panel), and the within-individual difference before and after the experiment (right panel, cut by whether they receive information). The sample is limited to individuals with a noncompete. We show 95% confidence intervals and use sample weights.

## Online Appendix B

### *OB. 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 those who are interested can find virtually identical content in the appendices of Starr et al. (2020) and Starr et al. (2021).

#### *OB1. 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>49</sup> or unemployed and looking for work. We exclude individuals who report 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>50</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>49</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>50</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>51</sup>

### *OB2. 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 recent Presidential election polling.<sup>52</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>53</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 OB4.

### *OB3. Survey Cleaning*

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

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<sup>51</sup> The information contained in the following sections can be found in Tables 1–18 in Prescott et al. (2016).

<sup>52</sup> See the methodology discussion linked at <http://polling.reuters.com/>.

<sup>53</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.



from different IP addresses, we kept the first observation and dropped all others, leaving us with a sample of 12,090 respondents.<sup>54</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>55</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 independently code the 11,529 responses by taking job titles, occupational duties, and employer descriptions and matching them with the appropriate two-digit NAICS and SOC codes.<sup>56</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.

<sup>54</sup> See Tables 3–5 in Prescott et al. (2016) for more details.

<sup>55</sup> See pp.412–14 in Prescott et al. (2016) for more details.

<sup>56</sup> See p.422 of Prescott et al. (2016) for details.

*OB4. Sample Selection*

As we observe above, there are four primary sample selection concerns with an online survey like ours: (1) not everybody is online; (2) not everybody online signs up for online surveys; (3) not everybody who signs up for online surveys receives a particular survey; and (4) not everybody who receives a survey manages to complete it. We describe these issues in greater detail in Section II.E in Prescott et al. (2016). All survey research must confront issues (1), (3) and (4)—the only unique selection concern for online surveys is (2). The key question is why individuals sign up to take online surveys and whether that reason is associated with their noncompete status or experiences.<sup>57</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 noncompetes. We believe it is certainly plausible that there is no such relationship.

A related sample selection concern is that individuals who participate in a survey may for some reason lie or otherwise provide inaccurate information in a systematic way. We designed our cleaning strategy with the explicit goal of weeding out such individuals. However, 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 OB1 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>57</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 OB1. Self-Described Job Title, Duties, and Industry for 15 Randomly Selected Respondents**

	Self-Described Job Title	Self-Described Job Duties	Self-Described Industry
1	Associate Analyst	My current job duties are to review and evaluate telephone recordings between our customers and customer contact representatives.	My current employer is a regional 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

*OB5. 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>58</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, performed clearly 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>59</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 simulate 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 combine these coefficients with the observed values of the covariates for the missing observations to generate a predicted value. For continuous variables, we used predictive mean matching in the third step. Specifically, we took the average of the 15 nearest neighbors to the

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<sup>58</sup> See pp.436–46 in Prescott et al. (2016) for more details.

<sup>59</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-out-come-in-imputation-models-of-covariates/>.

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. (2021), 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.

### Additional References

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Online Appendix C. 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 noncompete 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 noncompetete	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. tit. 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 noncompete must give rise to the interest protected by the noncompete, and held that the consideration for the noncompete 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.

