Clause and Effect:*
Theory and Field Experimental Evidence on Noncompete Clauses

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March 2025 (First Version: January 2024)

Abstract

We study worker noncompete clauses in a large field experiment with two finance firms. In about 14,000 job offers, we randomize wages and the presence, salience, and duration of noncompetes. We find that a noncompete in a job offer lowers mobility between competing employers by 30%-57%, and lowers workers' total earnings from the two firms by 12%-16%. We find no evidence that noncompetes lower information leakage between the two firms, and can reject small effects. We also find no evidence for compensating differentials or bargaining over noncompetes (even when made salient). The data suggests workers often fail to notice noncompetes before firms' post-employment communication. The results are consistent with a theoretical framework with inattention and uncertainty about contract enforcement.

JEL Classification: J42, M5, J31, J41, K31, L41, C93.

Keywords: Noncompete clauses, earnings, knowledge diffusion, mobility, contracting, inattention, enforcement risk.

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1 Introduction

Noncompete clauses ("noncompetes") have been a topic of debate since at least 15th century (Blake, 1960). By prohibiting departing workers from joining competitors, noncompetes create a tension between workers' freedom to contract and their freedom to move. Within this debate, two persistent claims are: (a) Noncompetes should make workers better off, as they can request better compensation or decline to sign them, and (b) noncompetes are necessary to prevent workers from sharing innovative firm secrets, thus preserving firm incentives to develop such innovations.

This paper assesses these claims theoretically and empirically. The core of our paper is a large field experiment with contract HR recruiters in the finance industry. Working with one company (Firm A), we randomize the presence, salience, and duration of noncompetes in about 14,000 job offers, while cross-randomizing wages. We also work with a competitor (Firm B) to test workers' willingness to violate noncompetes and share Firm A's private information (violating an NDA). With this design, we can estimate the causal effect of the firms' adoption of noncompetes, and the effects of a worker signing one.

To guide our analysis, we develop a theoretical framework about workers' choice to sign noncompetes. At the core of this model is uncertainty: If reading contracts is costly, workers might not know if their offer contains a noncompete, and thus face risk. Even when workers notice the noncompete, uncertainty arises about enforcement. The model shows how this uncertainty suppresses compensating differentials, and lead to noncompetes generating *ex post* harm, despite voluntary contracting. Adding behavioral factors (e.g., present-bias, base-rate neglect, DellaVigna, 2009) raises this worker harm.

To study this framework empirically, our experiment includes two manipulations that shock noticing and enforcement risk. First, we randomize which workers are shown the noncompete "saliently" (on the first page of the contract, requiring an extra signature), rather than the seventh page ("hidden"). In either case, the web-based contracting sys-

tem in our experiment measures how long each subject spends reading each page of their employment contract. Second, we study how Firm A's exit-message *reminder* of the noncompete changes behavior.

We find that workers who are randomly offered salient noncompetes are 15% less likely to accept Firm A's job. This is the equivalent of paying workers $12/hr less. We detect no differences in job acceptance rates for offers with "hidden" noncompetes versus no noncompete (on average). This is largely due to not seeing the noncompete. In the hidden noncompete condition, 50% of workers who opened the contract spent 2.5 seconds or less on the noncompete page, and approximately one third skip it altogether. However among workers who spend more than 5 seconds on page 7, those with a hidden noncompete are about 10 percentage points less likely to sign it (than those with no noncompete).

We find no evidence that those who worked for Firm A under a noncompete (salient or hidden) are paid a compensating differential, and can rule out small effects. Workers are not more likely sign noncompetes they are offered a higher wage (relative to the control group). We can also rule out small effects on bargaining attempts.

In our work with Firm B (a competitor) we find that Firm A's noncompetes lower mobility, both when hidden and salient, and even when the noncompetes are not legally enforceable in the workers' jurisdiction. Firm A's exit-messages about the noncompetes appear to be a key driver of this lower mobility. Moreover, workers who agree to salient noncompetes, are adversely selected against abiding by them: After the reminders, the salient group works at Firm B at higher rates (27.2%) than the hidden group (17%).

Approximately 4% of workers are willing to leak proprietary information provided by Firm A (resumes of qualified candidates) to Firm B. Randomly higher wage offers Firm B increases the rate of leaking Firm A's confidential information to Firm B. However in our noncompete tests, we find no evidence that adding a salient or hidden noncompete to the baseline contract (which included an NDA) lowered information leakage.

As a robustness check of our mobility results, we randomly release some workers from

their noncompetes early at the end of our experiment. Workers released from their noncompete early are +20% more likely to apply to a competitor's job opening, compared to not-yet-released individuals.

Lastly, we study how noncompetes affect worker earnings. Putting a noncompete in a job offer reduces earnings by 12% when it is hidden, and 16% when it is salient. In theory, these earnings differences could come from (i) workers turning down Firm A when offered a noncompete, (ii) any compensating differentials at Firm A or B arising from the noncompete, and (iii) the noncompete's effect on mobility (future earnings from Firm B). Among these, our earlier results find (ii) no evidence of compensating differentials.

For the remaining two reasons, the earnings decline comes from different sources for hidden vs salient signers. For hidden noncompete signers, we see little effect on (i) declining Firm A. However, there is a larger effect from (iii) earning less from Firm B. We find that signing a hidden noncompete reduces earnings 4.2%. Among the hidden group, workers who skipped the noncompete page entirely suffered earnings losses of 7.5%. By contrast, the workers offered a salient noncompete lost earnings mostly because of (i) rejection of Firm A's offer. Those who sign the noncompete anyway were somewhat deterred from Firm B after the reminder. However, they remained much more likely to work for Firm B than those who signed a "hidden" noncompete.

Taken together, this study makes three main contributions. First, we document causal effects of noncompetes. The direction and magnitude of our results are inconsistent with models of workers noticing noncompetes, requiring wage premiums, and being better off under noncompetes. Our results are also inconsistent with the view that workers who select into noncompetes would not be working anyway (e.g., workers who are about to retire may be more willing to sign a noncompete). To the contrary, we find that workers who agree to a salient noncompete are more willing to do future work with a competitor (relative to workers with a hidden noncompete). Our interpretation is that the salient noncompetes disproportionately select for workers who believe they will not be caught.

Second, we propose an alternative theory that is consistent with our results: understanding contracts and their likelihood of enforcement is costly. Some workers are uninformed, and are exposed to risk when accepting a contract. Even if average wage levels adjust to the random presence of noncompetes and expected enforcement, workers with noncompetes that are enforced are not sufficiently compensated *ex ante* (and are thus harmed *ex post*). This framework may apply to myriad similar employment terms (e.g., NDAs, training repayment agreements, non-solicitation agreements, etc.).

Third, we find no evidence that adding noncompetes to a baseline contract (which includes an NDA) reduces information leakage, and we can rule out small effects. Knowledge flows are a key part of both support and opposition to noncompetes, but are difficult to measure. Our findings suggest that firms can maintain incentives to invest in private information without noncompetes. At least in our setting, noncompetes reduced mobility and earnings without reducing the spread of firm secrets.

In the remainder of this section, we review related literature. Section 2 develops a simple model of wages, inattention, and noncompete adoption. Section 3 presents an overview of our empirical setting. Sections 4 through 6 describe our experiment and results. We conclude with a discussion in Section 7.

1.1 Related Literature

This paper is rooted broadly in the economics literature focusing on the adoption and efficacy of different management practices (Ichniowski et al., 1995; Bloom et al., 2014; Mas and Pallais, 2020; Roberts and Shaw, 2022; Hoffman and Stanton, 2024; Benson and Shaw, 2024) in the context of employment law and contracting (MacLeod, 2011). We consider the choice to put a noncompete in an employment contract, which itself is the focus of a growing literature in law, economics, and management on restrictive covenants.

In 2014, new evidence revived a longstanding debate over noncompetes: Approximately 20% of workers were covered by noncompetes (including extreme examples), and

about 50% of firms used them (Starr et al., 2021; Rothstein and Starr, 2022; Balasubramanian et al., 2024). Since then, approximately a dozen U.S. states banned noncompetes for some types of workers (e.g., low wage), and the U.S. Federal Trade Commission (FTC) adopted a national ban on all noncompetes in 2024 (currently contested in court). Other countries, including the United Kingdom, Italy, Austria, Canada, Norway, and Australia are also debating regulatory approaches to noncompetes.[1] In light of these developments, a fast-growing literature has grappled with the economics of noncompetes.

Contracting, Monopsony, and Earnings. Theoretical perspectives about noncompetes and earnings yields mixed results. Classic contracting models suggest that workers will agree to noncompetes only if they expect to benefit (Posner et al., 2004; Friedman, 1991), implicitly assuming workers read and understand their contracts. These models build on classic ideas of specific investments and hold-up problems common in the organizational economics literature (Klein et al., 1978; Grossman and Hart, 1986; Williamson, 1989; Bentley MacLeod and Parent, 2015), portraying noncompetes as tools to generate surplus that can be shared with workers. In this arrangement, noncompetes could make both sides better off and improve efficiency (Rubin and Shedd, 1981; Meese, 2022; Callahan, 1985). Several studies suggest that noncompetes are correlated with benefits to workers.[2]

Economists have also conceptualized noncompetes as strengthening monopsony power (Boal and Ransom, 1997; U.S. Council of Economic Advisors, 2016; Manning, 2021; Caldwell et al., 2024), and thus harming workers. Within-industry competition for workers could keep wages tethered to productivity; however, noncompetes might shield firms from such competition by imposing competitor-specific search frictions (Burdett and Mortensen, 1998; Starr et al., 2020), keeping wages below marginal products, and misallocating workers (Shi, 2023; Mueller, 2023). Research leveraging changes to state enforceability finds

[1] For additional detail on these countries' noncompetes, see Boeri et al. (2023) (Italy), Young (2021) (Austria), Ross (2024) (Australia), and Torgnes and Terjesen (2024) (multiple countries).

[2] For example, workers with noncompetes tend to have higher earnings than workers without noncompetes (Starr et al., 2021; Rothstein and Starr, 2022; Lavetti et al., 2020) and firms tend to invest more in such workers, including where noncompetes are more enforceable (Starr et al., 2018a; Starr, 2019; Jeffers, 2024).

that banning noncompetes increases mobility and wages (Lipsitz and Starr, 2022; Balasubramanian et al., 2022; Johnson et al., 2023a).

Our paper contributes a novel, tractable theoretical framework about why workers may *not* be compensated for the downsides of noncompetes *ex ante*. This provides a theoretical counterpart to empirical papers suggesting (i) workers and consumers often do not fully understood their contracts before signing them (Schwartz and Wilde, 1983; Bakos et al., 2014; Starr et al., 2021), and (ii) reminders by firms about noncompetes can influence behavior even when the noncompetes are unenforceable (Prescott and Starr, 2024). Our model can make predictions about wages, selection into noncompetes, and the effects of interventions by firms or policymakers; we use it to study policy proposals requiring noncompetes to be made salient (Meese, 2022).

Noncompete Research Designs. The two prior theoretical perspectives above have been supported by contrasting research methodologies. In the first, researchers survey workers about their noncompete experience (along with other characteristics and outcomes), and use the resulting data to infer relationships. This approach often finds that workers with noncompetes are paid better (Starr et al., 2021; Lavetti et al., 2020; Rothstein and Starr, 2022). However, this approach struggles to isolate the effects of noncompetes because of the endogeneity of adoption (both by workers and firms), and because noncompetes are often bundled with other restrictions (Balasubramanian et al., 2024).

The second approach uses changes in state policies as a shock to noncompete enforceability (Lipsitz and Starr, 2022; Balasubramanian et al., 2022; Johnson et al., 2023a). This design identifies the effect of the policy, but not the effect of firm adoption of noncompetes (e.g., in jurisdictions where they are unenforceable). Prior research suggests that *unenforceable* noncompetes are common and appear to limit worker mobility (Starr et al., 2020; Prescott and Starr, 2024; Sanga, 2018). Summarizing the empirical noncompete literature as a whole, a recent review by FTC economist McAdams (2019) concludes: "Further

research . . . is necessary to establish the causal impact [of] such agreements [...]."

We complement this research by using a field experiment for identification. Like other experiments, our results come from a stylized setting, but our design facilitates high quality measurement of key relationships and mechanisms. Our experiment addresses endogeneity not only in firm adoption of noncompetes, but also in workers signing them. If the results of this field experiment results generalize, they suggest that positive correlations between noncompetes and earnings are likely due to selection (rather than causation).

Innovation and Knowledge Sharing. Noncompetes could impede the spread of knowledge or best practices between firms. A broad literature considers mobility restrictions and economic dynamics. Several papers assess California's ban on noncompetes and its effect on the rise of Silicon Valley (Saxenian, 1996; Gilson, 1999; Hyde, 2003; Lobel, 2013). Fallick et al. (2006) find the mobility is higher in California. Related work in other states links noncompete enforcement with lower mobility, lower entrepreneurship, lower firm growth, and lower knowledge diffusion (Marx et al., 2009; Samila and Sorenson, 2011; Starr et al., 2018b; Franco and Filson, 2006). Recent papers by Johnson et al. (2023b) and Rockall and Reinmuth (2024) find that on net enforcing noncompetes reduces innovation.

An alternative perspective claims that "knowledge flows" between firms diminish incentives for investment (Rubin and Shedd, 1981) because of the potential leakage of confidential information. When the U.S. Chamber of Commerce—the largest business advocacy organization in the United States—argued against the FTC's noncompete ban, the loss of confidential information was the primary reason offered for their opposition.[3]

Measuring the flow of knowledge is a persistent challenge. Some research uses patent citations as a proxy for knowledge flows (Alcacer and Gittelman, 2006; Fadeev, 2023). By contrast, we measure knowledge flow directly by testing whether specific documents given by one firm are shared with another. We find no evidence that noncompetes deter secret leakage, suggesting that firms can maintain incentives to invest in private infor-

[3]See the Chamber of Commerce's complaint against the FTC here.

mation without restricting mobility. This also raises the possibility that banning noncompetes may *not* lead to increased inter-firm knowledge flows—though it may help spread other, legal forms of knowledge flows besides trade secrets (e.g., best practices).

To limit "secret sharing," firms have alternatives to noncompetes, like nondisclosure agreements or trade secret law. Comparative data about the effectiveness of these other protections with noncompetes is limited.[4] However, if e.g., NDAs effectively prevent workers from sharing company secrets, then adding a noncompete may just restrict mobility without adding any additional protection to company secrets. We contribute a direct experimental test of noncompetes + NDAs versus NDAs only, and find that while noncompetes limit mobility they do not help limit knowledge leakage.

Policy Responses. Several studies argue that the optimal policy is to ban noncompetes (e.g., Shi, 2023; Baslandze, 2022). Even when read and negotiated, noncompetes could generate an externality on third parties (Aghion and Bolton, 1987), or could lower innovation and entrepreneurship in ways a ban would eliminate. Policymakers also argue for noncompete bans by stating that less restrictive alternative measures can be used to protect secrets.[5] Other studies emphasize a ban may be suboptimal because of increases in costly turnover (Gottfries and Jarosch, 2023). Another approach is to require more transparency and salience around noncompetes (Meese, 2023), which several states have adopted in recent years.[6]

Our firm-side randomization of noncompetes does not necessarily generate the same effects as a ban. Jurisdiction-level bans would generate long-term adjustment and general equilibrium effects as workers and firms react to the ban. These longer-term adjustments

[4] A few papers contain suggestive evidence. Greenwood et al. (2024) find that trade secret litigation does not rise after noncompetes are banned. Hiraiwa et al. (2024) find that firms have low willingness-to-pay for enforcing noncompetes. In both cases, the availability of other tools is a potential interpretation.

[5] The FTC's final noncompete rule (FTC Final Rule, 89 Fed. Reg. 38342, 2024) concluded (p.38424) "Employers have alternatives to non-competes for protecting valuable investments that burden competition to a less significant degree."

[6] Eight states plus Washington D.C. have such laws (see here). See also language in Section 4 on 'Notice Requirements' in the Uniform Law Commission's Uniform Restrictive Employment Agreement Act here.

could differ from the localized experimental effects observed in our study.

While it is beyond the scope of our study to pursue a full welfare analysis of noncompetes, we contribute to some of these questions. We study how Firm A's noncompetes affect a third party—a competitor firm (Firm B). We also explicitly study the role of transparency, or salience, in noncompete contracting (Marx, 2011). Starr et al. (2021) suggests that "early notice" noncompetes are correlated with higher earnings. However, no prior empirical work has causally identified how making noncompetes more noticeable affects wages, sorting, or bargaining. Here we characterize a new tradeoff: Consistent with our theoretical framework, we find that noncompete salience deters workers from taking the job ex ante, but selects workers more likely to violate the noncompete ex post.

2 Conceptual Framework

Below we develop a wage-setting model in the presence of asymmetric information (based on Myerson, 1981; Riley and Zeckhauser, 1983; Bulow and Roberts, 1989) as a tool for interpreting our empirical results. The players are a mass of workers and an employer. We study how workers and firms select into noncompetes, and how this choice affects later outcomes. We outline our setup below and results, leaving proofs to Appendix A.

2.1 Setup

Workers' Utility. Workers gain utility from wages, and lose utility from working. Workers have an outside option normalized as zero, and a privately-known total distaste for the employer's job ($d_i < 0$) drawn from a continuous, regularly-distributed CDF F_d. If employed by the firm, they gain a wage w. Workers also lose utility from noncompetes. If they sign a noncompete that is enforced against them, they lose a net present value of $k > 0$ representing foregone wages in future periods.

Workers face uncertainty along two dimensions. First, they can be imperfectly informed

about the presence of a noncompete in a contract. Detecting a noncompete can be prohibitively costly because modern contracts are often lengthy and opaque. The exogenous term $\hat{p}_n$ reflects the workers' subjective probability that a contract includes a noncompete.

Second, workers can be uncertain about enforcement of a signed noncompete. Enforcement risk could arise if workers are not perfectly monitored after their employment, or if workers are not sure if firms find it worthwhile to sue them. Workers may also be uninformed about the legal enforceability of noncompetes in their local jurisdiction (e.g., courts in California do not enforce noncompetes, but Prescott and Starr (2024) show that workers generally do not know this). The exogenous term $\hat{p}_e$ represents the subjective probability that a signed noncompete will be enforced.

A workers' total distaste for work is: $\theta_i = d_i - \hat{p}_n \hat{p}_e k$. This term combines the distaste for the work d minus the noncompete penalty k, multiplied by its probability of appearing and being enforced. A worker takes job if $w + \theta_i \geq 0$. Worker utility is thus:

$$u_i = \max\{ \underbrace{w + \theta_i}_{\text{Payoff from Working}} , \underbrace{0}_{\text{Outside option}} \}. \tag{1}$$

The total distaste for work θ_i is distributed F_Θ. Note that F_Θ is simply a leftward shift of F_d by $\hat{p}_n \hat{p}_e k$. *Ex ante*, the worker takes the job with probability $1 - F_\Theta[-w]$.

Employer's Utility. Employers generate a payoff v from employing a worker, and pay a wage w to all hired workers. Let the terms $p_n, p_e \in \{0, 1\}$ represent whether the employer uses a noncompete and if noncompetes are enforced (as contrasted from the workers' beliefs about them, $\hat{p}_n$ and $\hat{p}_e$). If the employer does use a noncompete, the noncompete generates $\eta > 0$ of additional employer value (e.g., protects secrets). The firm's utility is:

$$u_f = \underbrace{(v + \eta - w)}_{\text{Employer surplus}} \cdot \underbrace{(1 - F_\Theta(-w))}_{\text{Labor supply}}, \tag{2}$$

Equation 2 codifies the idea that offering higher wages increases the supply of workers, while decreasing employer surplus per worker.

If the firm does not use a noncompete, we assume that $\eta = 0$. If the firm does use a noncompete, we assume that the firm knows the workers' disutility from it ($\hat{p}_n \hat{p}_e k$) and can incorporate this into its offer. We also assume the business value of the noncompete η is large enough to offset the workers' perceived disutility (i.e., $\eta > \hat{p}_n \hat{p}_e k$) if the firm chooses to offer the noncompete. We now have the technology for a simple model of wage-setting with noncompetes.

2.2 Results

We begin with a model of full information about noncompetes. Let w_{NC} represent the wage offers with noncompetes, and $w_{\emptyset}$ represent the offer without.

Proposition 1 (Full Information). *If $\hat{p}_n = p_n$ and $\hat{p}_e = p_e$, then $w_{NC} > w_{\emptyset}$.*

Proposition 1 shows that, when workers accurately detect noncompetes and their enforcement, wages are higher when the employer uses noncompetes. The result shows it is optimal for employers to use part of the η surplus generated by the noncompete to raise wages for their workers, offsetting their k loss from the noncompete.

Corollary 1 (No Regret). *If $\hat{p}_n = p_n$ and $\hat{p}_e = p_e$, then $u_i(w_{NC}) \geq 0$ for all workers who sign a noncompete.*

Corollary 1 shows all workers who sign noncompetes are ex post better off than if they had not signed the contract at all (taking the outside option), assuming workers can accurately detect noncompetes and know if they will be enforced. These baseline results codify a perspective in the noncompete literature (Rubin and Shedd, 1981; Callahan, 1985; Meese, 2022), emphasizing the benefits from free contracting, and the employers' need to pay a premium to sign a noncompete. Our baseline results show how these factors can mitigate the downsides of noncompetes.

Undetected Noncompetes. For contracting without noncompete clause detection, we assume that $\hat{p}_n = \bar{p}_n$; that is, workers correctly know the base rate of employers who offer noncompetes ($\bar{p}_n$), but do not know if any specific employment contract requires a noncompete. As such, they assume that any specific employer has the base rate $\bar{p}_n$ probability of containing a noncompete. Let w_H denote the wage offer made when noncompetes are undetected or "hidden," and let $w_\emptyset$ again represent the wage offer without noncompetes (where the absence of noncompete is known by the worker).

Proposition 2 (Undetected Noncompetes)**.** *If $\hat{p}_n = \bar{p}_n$, then $w_H > w_\emptyset$.*

Proposition 2 means that without clause detection, the employer's wage offer is higher than the wage offer where noncompetes can be detected but are not included. Undetectable noncompetes also generate the following feature.

Corollary 2 (No Premium)**.** *If $\hat{p}_n = \bar{p}_n$, there is no wage premium for contracts containing a noncompete clause.*

Because noncompetes cannot be detected, firms cannot condition a wage premium on signing a specific noncompete. In addition, we can show that the increase in average wages is not enough to eliminate *ex post* regret.

Corollary 3 (Undetected Regret)**.** *If $\hat{p}_n = \bar{p}_n$, then there exists d_i such that $\mathbb{E}[u_i(w_H, d_i)] \geq 0$, but $u_i(w_H, d_i) < 0$ if $p_n = 1$.*

Corollary 3 says that if workers cannot detect the noncompete, there will be some workers that will sign the noncompete (based on a positive expectation), but who are ex post worse off if they discover their contracts included a noncompete. This contrasts with Corollary 1, where full information eliminated regret.

Although average wages have been set for this loss in expectation, a finite realization leaves workers who signed noncompetes worse off. This is because they suffer penalty k, but do not receive sufficient *ex ante* compensation to cover it. These results about un-

detectable noncompetes show the importance of measuring whether workers read their contracts, as we do in our experimental section.

Mandated Visibility. Some policy analysts suggest mandating noncompetes to be more detectable (Meese, 2023). To model this, we assume that an exogenous intervention has set $\hat{p}_n = 1$. However, uncertainty about enforcement remains. Below, we show that a selected group of workers still signs the noncompete.

Proposition 3 (Worker Selection when Detected). *If $\hat{p}_n = p_n = 1$, then w_{NC} is accepted only if $\hat{p}_e$ is sufficiently low, or if d_i is sufficiently high.*

Proposition 3 models selection into a contract when the worker is aware of it. It shows that workers sign the noncompete only if they believe the enforcement probability $\hat{p}_e$ is sufficiently low, or if their taste for the job d_i is sufficiently high. In our empirical section, we study interventions aimed to increase awareness of the noncompete and the probability of enforcement. Proposition 3 helps interpret why some workers would still sign. Our next result is about how such workers would be paid.

Proposition 4 (Detected with Low Wage Premium). *If $\hat{p}_n = p_n = 1$, there exists an upper bound $\bar{\delta}$ such that $\forall \bar{\delta} \geq \epsilon > 0$, the noncompete wage premium $w_{NC} - w_{\varnothing} = \epsilon$.*

Proposition 4 shows that the wage premium for a detectable noncompete could be arbitrarily close to zero, depending on η and $\hat{p}_e$ enforcement beliefs. For intuition for Proposition 4, suppose that workers believe noncompetes are unlikely to be enforced. They will not demand a high premium because the risk of enforcement is low. Finally, we show that detectability does not eliminate *ex post* regret.

Proposition 5 (Detected Regret). *If $\hat{p}_n = p_n = 1$ and $\hat{p}_e \in (0, 1)$, then there exists d_i such that $\mathbb{E}[u_i(w_H, d_i)] \geq 0$, but $u_i(w_{NC}, d_i) < 0$ if $p_e = 1$.*

Proposition 5 shows that those who sign a detectable noncompete (due to positive expectation) can be *ex post* worse off than had they not signed it if enforcement is realized.

Non-Standard Beliefs and Preferences. Our results above assume that workers correctly know the base rate of noncompetes $\bar{p}_n$ and correctly weight the net present value of the noncompete penalty k. However, workers could be overoptimistic (Weinstein, 1980) about the base rate of noncompetes ($\hat{p}_n \leq \bar{p}_n$), and present-biased regarding k (Laibson, 1997). In Appendix A2, we show that these types of beliefs and preferences lead some workers to sign who are also *ex ante* worse off.

3 Experiment Overview, Setting, and Subjects

3.1 Subjects

The subjects of the experiment are contract HR recruiters in the finance industry (i.e., headhunters). We worked with two investment management firms to operationalize our experiment, which we call Firm A and Firm B. Both Firms A and B had previously used headhunters and other contract HR specialists for hiring. Throughout the broader U.S. economy, the employment of contracted HR recruiters has grown markedly in the last few decades (Cowgill and Perkowski, 2024).

Noncompetes are common in the finance industry, covering approximately one in four workers (Starr et al., 2021; Balasubramanian et al., 2024). The finance industry has fought to protect their use of noncompetes: For example, a proposed 2023 law in New York state banning noncompetes failed reportedly because of opposition from the finance industry (Ferr-Sadurn, 2023). Recruiters and HR staff are also frequently covered by noncompetes, particularly in sectors featuring high-skill labor. Based on their 2-digit occupation code, 20-30% of HR representatives sign noncompetes (Starr et al., 2021), above the national average of 18%. This includes recruiters and headhunters engaged on a short-term basis.

Oyer and Schaefer's 2011 discussion of "talent raids" explains why firms use noncompetes with recruiters. The efficacy of "talent raids" depends on (i) whether productivity is firm-specific, and (ii) how firms can learn the productivity of other competitors' work-

ers. On (i), productivity in finance is often *not* firm specific. Successful traders can take their investment ideas or strategy elsewhere. On (ii), worker quality is often privately observed by the current employers (Greenwald, 1986), including in our setting.

However, former HR staff can help competitors identify talent at their old firms. For these reasons, employers use noncompetes with HR staff to stop talent raids. Even knowing of a company's candidate flow (including rejected candidates) can be helpful for staffing competitors. Both Firms A and B used non-compete agreements and similar measures in the past, including for contract HR staff.

Contract HR recruiters are a useful benchmark population for several reasons. First, they are particularly well-informed about noncompetes. Because they are in HR, they are exposed to how noncompetes affect recruitment. They are also exposed to negotiation practices over employment contracts. Second, because they are *contract* recruiters, they should be especially averse to noncompete clauses, since their livelihood depends on mobility. Policymakers are especially concerned about noncompetes among freelancers, and have proposed specific laws to ban noncompetes for contract workers.[7] Finally, contract recruiters work offsite and do not interact with each other, which mitigates concerns about SUTVA violations. While contract HR recruiters are a unique population, we expect they provide an upper bound for workers' sensitivity to a noncompete.

Preview of Design. In Phase 1, Firm A makes randomized job offers to ∼14K freelance HR recruiters to review resumes submitted for a vacancy at Firm A. The hourly wage offered to the recruiter was randomized with the presence and salience of a noncompete.

In Phase 2, a competitor firm ("Firm B") sends job offers to the HR recruiters previously hired by Firm A, asking for help identifying promising candidates for an open vacancy. Any subject hired by Firm A could complete Firm B's task by sharing the resumes that Firm A gave them. In Phase 3, some workers are randomly released early from Firm A's noncompetes, and we track their applications to another job opening.

[7] See a New York City proposed law 'Regulating covenants not to compete for freelance workers' here.

What results should we *expect*? As a benchmark, we ran a survey on SocialSciencePrediction.org, a platform operated by U.C. Berkeley for collecting "expert forecasts of the effects of [...] social programs." Our survey described our design and asked the expert panel to forecast our results (Table C2 summarizes). We refer to these forecasts in a few cases for readers interested in this benchmark.

3.2 Sampling Frame, Weighting, and Balance

Firm A was interested in knowing more about how noncompete agreements affected their staff. We worked with them to identify baseline qualifications for the firm's recruiting tasks: having experience in either HR recruiting, finance, or both, and offering to perform freelance recruiting services. Using these criteria, we identified 29K such freelance recruiters on LinkedIn and other job platforms where recruiters post their availability.

These 29K contract HR recruiters form the population for this study. Based on their work as recruiters, they would fall in BLS occupational category code no. 13-1071 ("human resource specialists"). In columns (1) and (2) of Table 1, we present a set of descriptive statistics about these potential candidates based on their profiles.

Their average characteristics are similar to BLS category 13-1071: They are mostly female (66% in our sample, 75% in the BLS) and mostly white (72% in our sample, 76% in the BLS).[8] HR workers in the BLS earn about \$32/hour. Our workers' average asking rate is higher (\$51/hour), although the experiment shows that some recruiters are willing to work below their asking rate.

Stratification. We drew a stratified random sample of approximately 14K HR recruiters from the population of 29K. We derived the sample size of ∼14K based on power calculations for all three phases, and from the expected job acceptance rates of 10-20%.

Because the raw sample was stratified, it does not reflect the population. Accordingly,

[8]For statistics on BLS category 13-1071, see here.

Table 1: **Summary Statistics**

	(1)	(2)	(3)	(4)	(5)	(6)
	Population		Invited, Weighted		Invited, Unweighted	
Variable	Mean	SD	Mean	SD	Mean	SD
Recruiting Experience	0.730	0.444	0.730	0.444	0.759	0.428
Finance Experience	0.466	0.499	0.466	0.499	0.580	0.494
Female	0.661	0.473	0.661	0.473	0.550	0.497
Hourly Rate (Advertised)	51.5	49.8	51.3	50.4	54.9	51.7
Noncompete Enforcement State	0.697	0.459	0.697	0.459	0.570	0.495
White	0.725	0.446	0.724	0.447	0.743	0.437
Observations	29,005		13,896		13,896	

Notes: This table show summary statistics for the full population, the sample which were invited to participate in the experiment, and the weighted invited sample.

we use inverse probability weighting based on the representation of the strata in the population. Because the 14K customized job offers could not be sent all at once, they were sent in batches around the same time. Our specifications can include batch fixed effects (timing) as well as strata fixed effects (characteristics). Details of our stratification and batching procedure are in Appendix B1. The balance table from the unweighted invited sample is shown in Table C1. Table 1 columns (3)-(6) provide descriptive statistics on the unweighted and weighted sample.

4 Phase 1: Job Offers and Hiring

In the first Phase of the experiment, we work with Firm A to send 14K randomized job offers to recruiters in our sample. We measure their engagement with the offer, their browsing of the noncompete page, attempts to bargain, and (ultimately) whether they accept or reject the offer. We also study if the noncompete increased Firm A's wage bills and performance on the recruiters' task.

4.1 Experimental Design

Although we used several online platforms to identify qualified recruiters, the experiment used a single website for payroll and communication. Job offers were sent via the platform's email-like messaging service, which allowed recruiters to accept a job offer by clicking a link. Documents to help the recruiter perform work could be sent securely via the platform. The website provided some information about Firm A through its profile, including their ability to pay (i.e., that a credit card or bank information was on file).

Appendix B3 contains an example of a job offer to a recruiter. Recruiters are asked to review resumes provided by the firm and to select promising candidates for an associate position. They did not contact candidates, but screened applications and made recommendations for next steps. Although the task could probably be completed in about an hour, workers took about 1-2 weeks to complete the review (and were welcome to complete the job on this schedule). The initial outreach contains a proposed hourly rate, and a link to an eight-page work agreement.

All workers were required to sign this contract to begin work for Firm A. The contract included non-disclosure and confidentiality provisions for workers in all conditions that prohibit them from sharing confidential information given by Firm A. The contract also included a choice of law and forum clause indicating Florida law (where Firm A has offices). Florida's noncompete laws are viewed as the most pro-enforcement of all US states (Bishara, 2011).[9]

Reviewing and signing this agreement was performed online, and we tracked how many milliseconds each subject spent on each page. Some workers declined the job without opening the contract (i.e., did not click the link to the contract), or did not respond to the outreach. In all conditions, a "go to the end and sign" button allowed workers to skip

[9] Although Firm A's contracts designated Florida's pro-noncompete laws, some workers from outside of Florida could avoid enforcement depending on their own state's laws. However, prior research suggests that even unenforceable noncompetes can affect worker behavior. In Appendix E, we break down results by state-level enforcement status.

to the final page requiring a signature.

Randomized components. Each job offer contains two randomized components: First, the hourly rate, and second, the six-month noncompete. The randomized hourly rate was either \$25/hr or \$60/hr, which corresponded to the 25th and 75th percentile of the asking rate distribution in our population of 29K.[10] The three noncompete treatments were "no noncompete" (control), "hidden" noncompete, and "salient" noncompete.

- In the "**no noncompete**" condition, a noncompete clause did not appear anywhere in the eight-page contract.
- The "**hidden noncompete**" condition is identical to the "no noncompete" condition, except a noncompete clause appeared on page 7. This treatment reflects the typical experience, where the noncompete appears somewhere in a lengthy contract.
- The "**salient noncompete**" condition differs from the other two in three ways. The goal of these three features is to make the presence of the noncompete very clear to a worker in the salient condition (through several channels).

    1. First, the noncompete is mentioned in Firm A's initial outreach message offering a job.
    2. Second, the noncompete clause appeared on the first page of the work agreement (rather than the seventh).
    3. Third, the noncompete provision required a separate, additional signature on the first page of the work agreement. Workers cannot move past or skip this page without first signing the noncompete clause.

Figures B1 and B2 shows page one of the salient noncompete and hidden/no noncompete treatments, while Figures B3 and B4 show page 7. In the two noncompete conditions,

[10] According the BLS, the 25th percentile hourly wage is \$24.76/hour (very close to the \$25/hour randomized rate). The 75th percentile in the BLS was \$44.42, which is lower than our \$60/hour upper rate. https://www.bls.gov/oes/2023/may/oes131071.htm

the clause reads:

> *During the paid engagement with the Company and for a period of 6 months after the separation for any reason, Recipient will not directly or indirectly engage in any business that competes with the Company, including but not limited to, business engaged in finance, technology, healthcare, publishing, philanthropy, or sustainability/ESG activities. This covenant shall apply to the geographical area that includes anywhere in the United States.*

The noncompete is broad, covering the industries in which Firm A has investments, with a geographic coverage of the entire United States. It is also broad given the short-term nature of the work (i.e., a 6-month noncompete for a short task). Note, however, that the breadth of the noncompete is comparable to, e.g., Amazon's use of an 18-month noncompetes with low-wage packers hired for a few weeks over the holidays.[11]

Outcomes. For all 14K candidates we invited, we code whether they opened the employment contract, how many milliseconds they spent on each page of the employment contract (including page 7), and whether they bargained over the terms. We also measured if they accepted the job, completed the resume screens, the total amount of hours billed, the agreed-upon hourly rate ($25 or $60), and their total bill. Prior to beginning work, each worker's signature was confirmed to ensure it corresponded to their name. Figure 1 provides a schematic of the first phase.

4.2 Empirical Specification

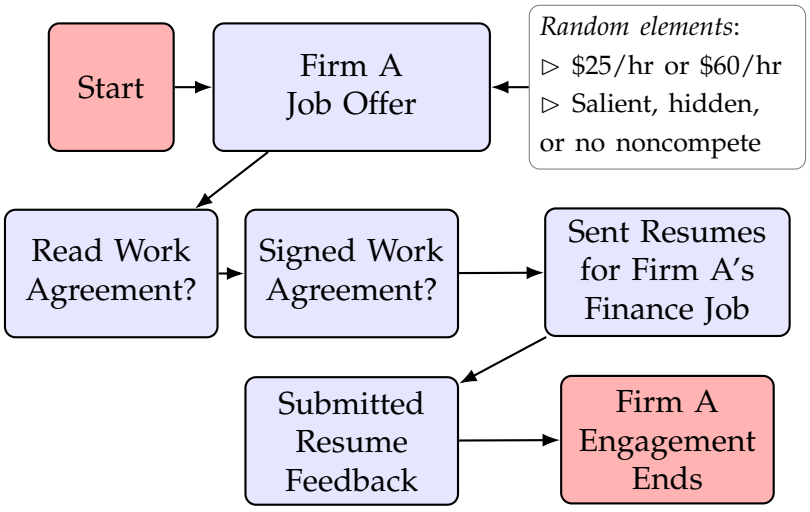
For our first stage outcomes, our estimating equation is

$$y_i = \beta_1 \mathbb{1}(Salient\ NC_i) + \beta_2 \mathbb{1}(Hidden\ NC_i) + \beta_3 w_i^A + \gamma_{s(i)} + \lambda_{t(i)} + \epsilon_i, \tag{3}$$

where "no noncompete" is the reference category, w_i^A is the wage offered by Firm A, $\gamma_{s(i)}$ are strata fixed effects where $s(i)$ is the strata for individual i, and $\lambda_{t(i)}$ are batch fixed effects where $t(i)$ is the batch for individual i. We cluster standard errors at the indi-

[11] For additional details about Amazon's noncompete, see here.

Figure 1: **Phase 1 Experimental Design Summary**



vidual level and use inverse probability weights (described earlier) given our stratified sample. Because noncompetes and wages are cross-randomized, β_1, β_2, and β_3 all represent unbiased estimates of an average causal effect of offering a salient noncompete, a hidden noncompete, and/or higher wages (respectively). In Appendix Table C6, we address possibility of contamination bias (Goldsmith-Pinkham et al., 2022), and show that our coefficients are broadly unchanged.

The estimates from Equation (3) can be scaled for interpretability. In particular, we estimate how much extra money (per hour) the firm would have to pay in the noncompete conditions to equalize the acceptance rates with the "no noncompete" condition. We call this the "wage equivalent." When y_i is an indicator for accepting Firm A's contract, the ratio $\frac{\beta_1}{\beta_3}$ is the wage equivalent for a salient noncompete, and $\frac{\beta_2}{\beta_3}$ is the wage equivalent for a hidden noncompete.[12]

[12]These ratios arise from the fact that β_3 estimates how much a $1 increase in hourly wages affects acceptance, while β_1 estimates how much a salient noncompete changes acceptance. The ratio thus scales the noncompete effect by the wage effect.

4.3 Phase 1 Results

Working for Firm A. Table 2 contains our results about opening the contract, signing it, and completing Firm A's task (and being paid). Our results suggest that the salient noncompete deterred workers from joining Firm A. Seeing the noncompete in their initial outreach, many workers declined to even open the contract. Contract opening rates in the salient condition fell by 34% (0.077/0.228) relative to control. The salient noncompete also reduced the rate at which workers sign the contract by 15% (0.023/0.153) and ultimately the rate at which workers complete Firm A's task by 15% (0.018/0.12) relative to the no noncompete group.

By contrast, we find no evidence that the hidden noncompete reduced opening rates. This a placebo test, since nothing before the 7th page of the contract indicated a difference with the control. We do find some evidence that workers in the "hidden" condition sign at slightly lower rates, but no evidence that they completed Firm A's task (and were paid) at higher or lower rates than the control group. Our standard errors allow us to rule out small effects. As we detail below, these results are broadly consistent with many workers not noticing the noncompete clause.

We estimate a wage equivalent for the salient noncompete of $12.95. That is, in order to have the same acceptance rates as the no noncompete condition, the firm offering a salient noncompete would have to raise wages by $12.95. The wage equivalent for the hidden noncompete is $6.90/hr, though we cannot reject zero difference. Because the workers were only offered one task (expected to take around an hour), this equivalence can be interpreted as the number of dollars required to increase acceptance rates to the level they are with no noncompete.

Expert Forecasts. Appendix Table C2 shows that experts over-estimated the deterrence effect of salient noncompetes. However, they correctly predicted the hidden noncompete contract signing rates (conditional on knowing the baseline in the control group).

Table 2: **Opened Contract, Signed Contract, Completed Firm A task**

	(1)	(2)	(3)
Dependent Variable	$\mathbb{1}\left(\text{Opened Contract}\right)$	$\mathbb{1}\left(\text{Signed Contract}\right)$	$\mathbb{1}\left(\text{Completed Task}\right)$
Offered Salient Noncompete	-.077***	-.023**	-.018**
	(.01)	(.0092)	(.0081)
Offered Hidden Noncompete	-.012	-.016*	-.0097
	(.011)	(.0092)	(.0083)
Firm A Wage Offer	.0018***	.0015***	.0014***
	(.00024)	(.00021)	(.00019)
Weighted	Y	Y	Y
Control Mean	.23	.15	.12
Salient - Hidden	-.07***	-.007	-.009
	(.01)	(.009)	(.008)
Salient Wage Equivalent	43.99***	15.03**	12.95**
	(8.4)	(6.4)	(6)
Hidden Wage Equivalent	6.85	10.62*	6.9
	(6.2)	(6.2)	(5.9)
Observations	13,896	13,896	13,896
R^2	.028	.021	.02

Notes: * p<0.1, ** p<0.05, *** p<0.01. All models include strata fixed effects and batch fixed effects. Standard errors reported in parentheses, clustered by individual.

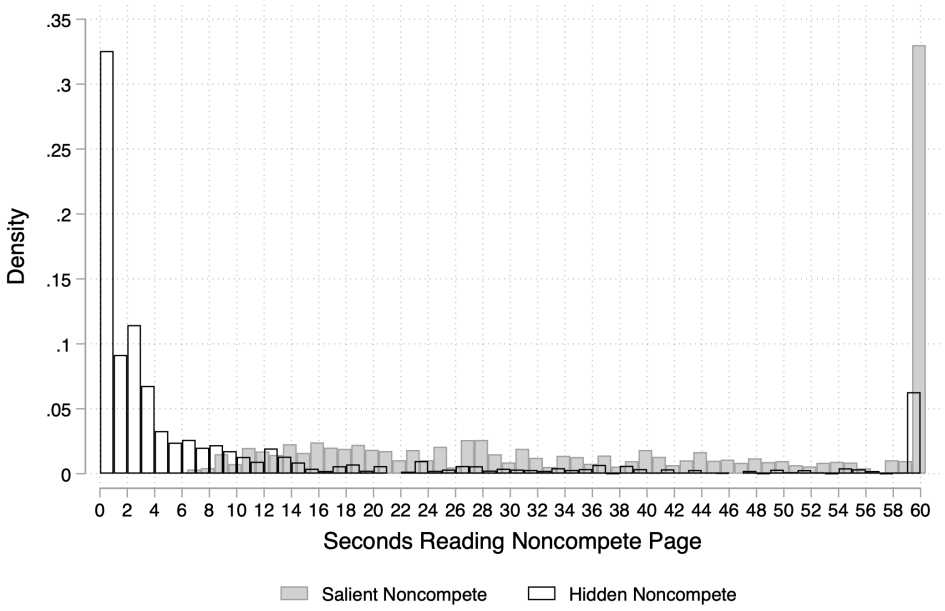
Contract Reading. Our theory suggests that the ability to detect noncompetes can affect worker outcomes. Our experimental data broadly shows that most workers in the hidden condition do not notice the noncompete, and those who do are less likely to sign. In the hidden and control conditions, workers spent similar amounts of time on page 7 (around 2 seconds), irrespective of whether a noncompete appeared.[13] For salient and hidden conditions, Figure 2 shows a histogram of the time spent reading the noncompete page (among those who got to it).

In the hidden noncompete condition, approximately 33% of workers skipped the noncompete entirely,[14] while 75% of workers spent less than 10.6 seconds on the noncompete

[13]The median worker with a hidden noncompete spent 2.5 seconds on the noncompete page, whereas the median worker in the no noncompete condition spent 2 seconds on page 7. Figure C2 shows a box-and-whisker plot of the time spent reading each page of the contract for workers all who opened the contract.

[14]Figure C3 shows the likelihood that a worker clicked to skip on a given page, and C4 shows the cumulative likelihood that an individual skipped by a given page.

Figure 2: **Histogram of Time Spent Reading Noncompete Page**



Notes: Results are weighted. Reading variables are winsorized at 60 seconds.

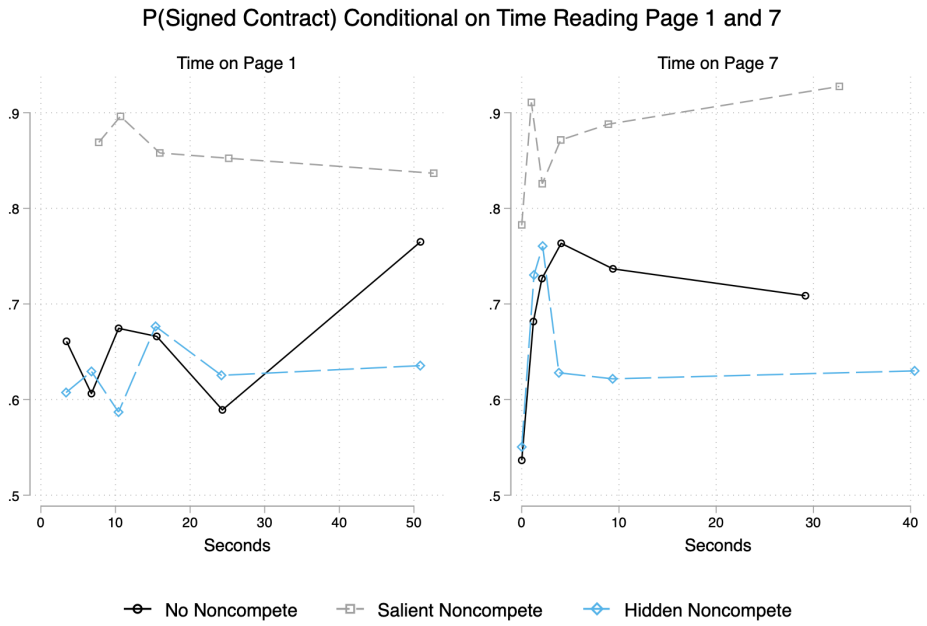
page (page 7). By comparison, in the no noncompete condition, 75% of workers spent less than 6.78 seconds on page 7 (now without a noncompete), with the bulk of the extra mass appearing in the right tail (see Figure C5). In contrast, in the salient condition (when the noncompete is on page 1), no recruiter spent less than 6 seconds reading the noncompete page. Approximately one in five workers spent at least one minute on it.

Workers who do spend time on the noncompete page when it is hidden are less likely to sign the contract, and some are less likely to complete Firm A's task. Figure 3 provides a binned scatterplot of how reading time relates to the probability of signing the contract.[15] The right panel examines reading page 7. Recruiters in the hidden condition who spend only a few seconds on page 7 are similarly likely to sign the contract as those with no noncompete. However, among workers who spend at least 5 seconds on page 7, those offered a hidden noncompete are unconditionally, about 10 percentage points less likely to work for Firm A. Regression tests of this difference are consistent and appear

[15]Figure C6 reports the same analysis for completing Firm A's task.

in Table C3.[16] These analyses suggest that some workers who discovered the "hidden" noncompete were deterred from working from Firm A. However, many did not notice it.

Figure 3: **Probability of Signing Contract Given Reading on Page 1 and 7**



Notes: Reading variables are winsorized at 60 seconds.

In contrast, workers offered a salient noncompete are, for the most part, highly likely to sign the contract (no matter how long they review the first page). Because salient workers were informed about the noncompete in their job offer email, they likely knew it contained a noncompete before they opened it. Accordingly, the salient workers were more likely to sign the contract than the other groups (conditional on opening the contract).

Heterogeneity. In Appendix E, we how noncompetes affect the probability of completing Firm A's task for different subpopulations. Four findings stand out. First, in no subsample do we detect that offering a hidden noncompete reduces the probability of completing Firm A's task to a statistically significant degree (relative to the no noncompete

[16] A table indicating similar results for completing Firm A's task is in C4. These results are noisier. At conventional levels, only those spending 2.6-10.6 seconds on page 7 show a drop in completing Firm A's task. We attribute these differences to some workers who signed the contract not finishing the task.

condition). In contrast comparing the salient and no noncompete conditions, we find that women are more sensitive to salient noncompetes than men. We also find strong differences based on the recruiters' asking rate, which proxy for the workers' outside option. Workers with high asking rates are particularly reluctant to work for Firm A under salient noncompetes, and workers with lower asking rates are hardly dissuaded on average. Finally, we do not detect that the salient noncompete effect differs across workers in states where noncompetes are enforceable (vs not).

Bargaining. Some workers responded to Firm A's offers by asking for a higher wage. Although our protocol did not grant these requests (all offers were take-it-or-leave-it), we kept data about each attempt to bargain. Overall, only about 1% of the subjects sought to negotiate over wages.

Table 3 examines bargaining rates using Equation (3). In column (1) we find that workers do negotiate more—when they randomly receive a lower wage offer. However, we find no evidence they bargain more when randomly assigned a salient or hidden noncompete. Thus, the HR recruiters do negotiate, just not necessarily over noncompetes. This is consistent with other research suggesting workers rarely bargain over noncompetes (Starr et al., 2021; Rothstein and Starr, 2022), again possibly because they do not notice them. Our results suggest this is true, even when the noncompete is made salient, and when other aspects of the job are held constant.

Columns (2)-(4) examine heterogeneity based on whether the individual opened the contract and how long they read page 7, which is relevant for the hidden treatment group. The results suggest that if anything, those in the salient noncompete condition were somewhat *less* likely to negotiate if they had opened the contract.[17] In contrast, among those who spent at least 15 seconds reading page 7, the hidden noncompete group were more likely to try to negotiate over wages. However, since so few workers spent more than 15

[17]These results are likely due to selection, since the salient group opened the contract already knowing about the noncompete.

Table 3: **Wage Bargaining and Reading Contract**

Dependent Variable:	(1)	(2)	(3)	(4)
	𝟙(Wage Bargaining)			
Sample:			Read p.7 for ___	
	Overall	Opened Contract	$>$ 0 seconds	$>$ 15 seconds
Offered Salient Noncompete	.002	-.0073*	-.0077	-.017
	(.0021)	(.0042)	(.0053)	(.021)
Offered Hidden Noncompete	.003	.0044	.0014	.039*
	(.0023)	(.0059)	(.0072)	(.021)
Firm A Wage Offer	-.00032***	-.00037**	-.00035*	-.00091
	(.000053)	(.00015)	(.00019)	(.00055)
Weighted	Y	Y	Y	Y
Control Mean	.01	.01	.02	0.004
Observations	13,896	2,855	1,877	369
R^2	.031	.074	.08	.41

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and fixed effects for the date of initial outreach. The dependent variable is an indicator for bargaining over wages. The four columns include the overall sample, those who opened the contract, and those who spent at least some time on page 7 or at least 15 seconds on page 7.

seconds on page 7, this heterogeneity is washed out in the overall estimate.

Job Performance. In Appendix D, we examine how randomly requiring noncompetes in a contract affects performance on Firm A's work. While not formally a part of our theoretical framework, noncompetes could affect the quantity or quality of work performed at Firm A. The noncompete could change the composition of workers willing to accept the job, and could also generate a treatment effect among selected workers. Our performance metrics are limited, and in general we find imprecise effects. The results, while imprecise, suggest that requiring a salient noncompete produces lower performance, and that at least some of this difference comes from a treatment effect (versus selection).

4.4 Firm A's Wage Bills: Compensating Differentials

Does Firm A need to pay higher wages to attract workers when it requires a noncompete? Our conceptual framework addresses this question.

- Proposition 1 showed that wages could rise if workers could detect noncompetes (or their absence) and correctly know enforcement.
- Corollary 2 showed that if the noncompetes were undetected, they would not cause wage premiums.
- Proposition 4 shows wage premiums could be small even noncompetes if are detectable.

In Table 4 we examine these wage premiums.[18] We find no evidence that workers given a higher wage ($60/hour) are more likely to accept a noncompete, compared to workers given a lower wage ($25/hour) in columns (1)-(3). Given this, our results in columns (4) and (5) are not surprising: When Firm A requires a salient or hidden noncompete, it does not increase the average hourly wages it pays, nor does it increase its average wage bill. Workers offered noncompetes are not paid higher.

This analysis suggests that workers are generally not trading off wages and noncompetes—at least within the confines our experiment. Rather, it seems that some workers will not agree to noncompetes, even when they come with relatively high wages. Firm A thus does not pay higher costs to get workers to sign noncompetes (either hidden or salient). However, Firm A may pay a cost on different margin when the noncompetes are salient: The firm may need more time to find workers willing to accept the job.

5 Phase 2: Poaching and Secret Sharing

We now study outcomes of Firm A's workers after their engagement with Firm A ended. In Phase 2, we worked with a second finance company, Firm B, to generate an offer to the $\approx 2K$ workers who signed the contract with Firm A and were sent work materials (i.e., resumes to review). Firm A was aware of this plan.[19] We are interested in how non-

[18] We also analyze earnings in Section 6.

[19] Firm A declined to enforce the noncompetes in this experiment, although they did send an exit message reminder of the noncompete that is customary for companies using noncompetes. In addition, we did not share any information with Firm A about which recruiters accepted Firm B's offer. Some of Firm A's workers chose to share about their experience with Firm B on their profiles.

Table 4: **Noncompetes and Firm A's Wage Bills**

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	𝟙(Completed Firm A Task)			Hourly wages	Total billed
Sample	Overall	Offered \$25/hr	Offered \$60/hr	Finished Firm A Task	
Offered Salient	-.0078	-.0079	-.029**	-1.2	-.88
	(.01)	(.01)	(.012)	(1.3)	(1.3)
Offered Hidden	-.0068	-.0069	-.013	-.19	.6
	(.01)	(.01)	(.013)	(1.3)	(1.3)
𝟙(Offered \$60/hr)	.058***				
	(.012)				
Salient× 𝟙(\$60/hr)	-.021				
	(.016)				
Hidden × 𝟙(\$60/hr)	-.0057				
	(.017)				
Weighted	Y	Y	Y	Y	Y
Control Mean	.12	.09	.14	46.73	44.92
Observations	13,896	6,945	6,951	1,597	1,597
R^2	.02	.025	.022	.081	.082

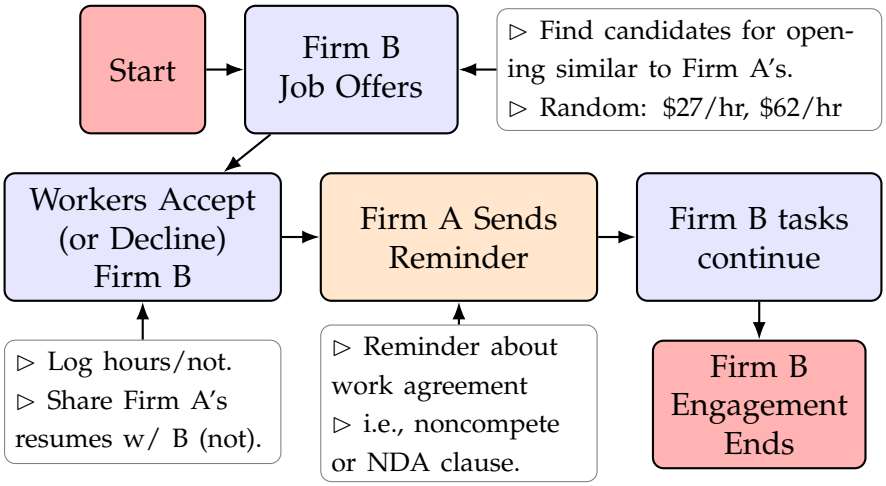
Notes: Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and fixed effects for the date of initial outreach. The dependent variable in columns (1)-(3) is an indicator for completing Firm A's task. The sample in column (1) is all workers, and in column (2) and (3) it is split out into workers offered \$25/hr and \$60/hr, respectively. In columns (4) and (5), the sample is limited to those who completed Firm A's task. The dependent variable in column (4) is average hourly wages, and the dependent variable in column (5) is the total amount billed by each worker, which can be different from their hourly wage because of how much time they spent working on the task.

competes affect worker willingness to take a job with a competitor, both before and after a reminder from Firm A. We also study whether noncompetes dissuaded these workers from leaking Firm A's confidential information, violating the terms of their NDA.

5.1 Experimental Design

Firm B is another investment firm, also with offices in Florida and that historically used contract recruiters. After working for Firm A, the online profiles of workers in Phase 1 often included a line about their work for Firm A. Firm B contacted all Firm A's former workers from Phase 1 with a message indicating that they saw the worker had related experience (Appendix B4 contains example text). Firm B expressed interest in hiring the recruiter to help identify candidates for a full-time finance associate position with 3-5

Figure 4: **Phase 2 Experimental Design Summary**



years of experience. All recruiters hired by Firm A could easily fulfill this task by sharing the resumes provided by Firm A, or by finding new resumes. Phase 2 also includes a randomized wage treatment. Hourly wages for Firm B's offer were randomized at $27 or $62 per hour. These rates are approximately the 25th and 75th percentile, but are slightly more than Phase 1.

Phase 2 consists of two sub-phases, separated by a reminder by Firm A about workers' contractual obligations. These reminders are often used by employers in exit interviews, or post-employment letters or emails. The language of the reminder can be found in Appendix B5. In Phase 2A, workers are initially approached by Firm B, and can choose to work for Firm B or not on their own volition. In Phase 2B (following the reminder by Firm A), the workers are given more opportunities to source leads for Firm B's job opening.

We observe which subjects work for Firm B, the number of resumes the workers share with Firm B, whether those resumes correspond to the resumes given to them by Firm A, the number of hours worked, and total payments. Figure 4 visualizes the second phase.

5.2 Empirical Specification

For second stage outcomes, we estimate five different versions of the following specification. The variations of this specification help us separate treatment and selection effects.

$$y_i = \beta_1 \mathbb{1}(Salient\ NC_i) + \beta_2 \mathbb{1}(Hidden\ NC_i) + \beta_3 w_i^B + \beta_4 w_i^A + \gamma_{s(i)} + \lambda_{t(i)} + \epsilon_i, \quad (4)$$

where w_i^B is Firm B's randomized wage offer.

The first version of Equation (4) examines all ∼14K invited workers. The resulting coefficients have a causal interpretation. They represent the effect Firm A adopting noncompetes (*requiring* them from workers). The effects of requiring noncompetes could come through a change in selection of workers who sign different noncompetes (caused by the requirement), or from treatment effects from signing the noncompete. Our first version of Equation (4) does not separate these.

Second, we estimate Equation (4) on the limited sample of workers who agreed to work for Firm A. This does not necessarily have a clear causal interpretation, because these differences could again arise from either selection or from treatment differences. Our next three versions of Equation (4) are designed to identify treatment effects of signing a noncompete specifically, i.e., by handling differences in selection or other confounds.

Treatment Effects for Signing a Noncompete. In our third version, we estimate Equation (4) with an instrumental variables approach, instrumenting for whether an individual signed a noncompete with whether they had been (randomly) offered one. This approach relies on the fact that we have no defiers, since it is not possible to have a noncompete with Firm A if you were not offered it. It also relies on the exclusion restriction—that being offered a noncompete does not affect e.g., working with Firm B except through whether the worker signed the noncompete. Since it is not possible to work with Firm B without signing Firm A's contract, we think this condition is met.[20]

[20]Note that in these models, we also include a control for the individual not being paid by Firm A. This ensures we are estimating causal effect of signing a noncompete with Firm A versus taking Firm A's job

In our fourth version, we study "skippers" in the hidden and no-noncompete conditions. By skippers, we mean workers who signed Firm A's contract without reading page 7 at all. Because these workers did not read page 7, they could not have selected into a noncompete. These workers are selected into signing the contract—but they could not have selected into the noncompete because they did not see it.

Skippers learn about their noncompete status only when Firm A sends the reminder. We thus limit our sample to workers in the no-noncompete or hidden conditions who *skipped page 7 entirely* and signed the contract. This approach estimates a skipper-specific treatment effect for learning they signed a noncompete. This approach works only for studying outcomes for that occur after Firm A's reminder. For outcomes before the reminder, it suggests a natural placebo test: Behavior should be indistinguishable.

In our fifth and final approach, we follow DiNardo et al. (2021) and use an instrument in a two-step Heckman approach (Heckman, 1979) to adjust for differential selection rates into working for Firm A. This allows us to recover an average treatment effect for a subset of the population. The instrument is the randomly assigned date that each worker was sent their job offer. Recruiters were particularly likely to complete Firm A's task when the invitation was sent on a Friday or Monday, and particularly unlikely to be completed when it was sent on Saturday (Figure C7 visualizes this in the unweighted sample).[21] Under a standard monotonicity requirement, this randomization gives us another strategy for estimating the treatment effects of signing a noncompete on Phase 2 outcomes.[22] The exclusion restriction requires that the day on which a worker was invited by Firm A does not affect how that worker engages with Firm B, aside from their decision to complete Firm A's task.

without a noncompete.

[21] Because the invite date is collinear with the batch fixed effects, when using this approach we omit the batch fixed effects.

[22] The monotonicity requirement is that being randomly assigned to a particular date affects all workers' choices to join Firm A weakly in the same direction.

5.3 Phase 2 Results: Working for Firm B

Table 5 contains our results on pre- and post-reminder job mobility. Across all results, we find a consistent pattern: Pre-reminder, we find no evidence that noncompetes dissuade workers from joining Firm B. However, following the reminder, workers who are offered and/or signed noncompetes are less likely to accept Firm B's job.

In Columns 1 and 6 of Table 5, we study the effects of Firm A requiring a noncompete. These indicate that Firm A using noncompetes lowers the probability of working for Firm B. However, this could be true for several reasons. For example, noncompetes could select workers who were uninterested in a future job (e.g., because of vacation plans during the six-month noncompete period).

For this reason, we turn to our approaches for measuring the treatment effect of signing. Our results in the subsequent columns are all highly consistent with each other: They show a negative treatment effect of signing a noncompete on post-reminder mobility. Throughout our post-reminder results, we also find much larger effects for signers of *hidden* noncompetes than for signers of salient ones.[23] This distinction is consistent across all demographic subgroups we analyzed (Figure E3). We also find no differences based on the enforceability in the recruiter's home jurisdiction (see Appendix E).

Why are signers of the salient noncompete actually *more mobile* than signers of the hidden noncompete? Proposition 3 suggests a natural answer: When noncompetes are easily detectable, the only people willing to sign are those who do not believe the noncompete will be enforced.

Skippers. Columns 4 and 8 of Table 5 contains treatment effects for the 'skippers.' Column 4 contains our placebo test: among skippers, we find no differences in working for

[23] For example, in the sample of workers paid by Firm A (column 8), workers with a salient noncompete are 8.8 percentage points less likely to join Firm B, post-reminder, a decline of 24% from the no noncompete mean of 36%. In contrast, those in the hidden noncompete condition are 19 percentage points less likely to join Firm B post-reminder, a decline of 52%.

Table 5: **Joining Firm B**

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	𝟙(Accepted Firm B)									
	Before the Reminder					After the Reminder				
Offered Salient	.0079					-.012***				
	(.0053)					(.0043)				
Offered Hidden	.0064					-.023***				
	(.0051)					(.0042)				
Signed Salient		.076*	.044		.029		-.097**	-.088***		-.071*
		(.042)	(.038)		(.039)		(.038)	(.034)		(.039)
Signed Hidden		.054	.016	.054	.0088		-.2***	-.19***	-.16***	-.19***
		(.04)	(.034)	(.065)	(.035)		(.035)	(.032)	(.058)	(.034)
Firm B Wage Offer	.012***	.0055***	.0044***	.0058***	.0042***	.0052***	.0032***	.0045***	.0057***	.0043***
	(.0003)	(.0005)	(.00084)	(.0018)	(.00085)	(.00026)	(.00038)	(.00074)	(.0016)	(.00078)
Inverse Mills Ratio					.26					-.43*
					(.22)					(.22)
Estimator	OLS	2SLS	OLS	OLS	Heckman	OLS	2SLS	OLS	OLS	Heckman
Sample	All	All	Paid by Firm A	Skippers	Paid by Firm A	All	All	Paid by Firm A	Skippers	Paid by Firm A
Weighted	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	.08	.64	.64	.66	.64	.04	.36	.36	.39	.36
KP RK Wald F		347.1					347.1			
Salient - Hidden	.002	.02	.03		.02	.01***	.1***	.1***		.12***
	(.005)	(.04)	(.04)		(.04)	(.004)	(.03)	(.03)		(.03)
Salient Wage Equiv.	-.68	-13.73*	-10.13		-6.87	2.31***	30.7**	19.68**		16.27*
	(.45)	(7.6)	(8.9)		(9.3)	(.84)	(12)	(8.1)		(9.3)
Hidden Wage Equiv.	-.55	-9.67	-3.66	-9.39	-2.1	4.43***	62.56***	42.37***	27.49**	42.97***
	(.44)	(7.3)	(7.9)	(12)	(8.4)	(.81)	(13)	(9.9)	(12)	(11)
Observations	13,896	13,896	1,597	351	1,597	13,896	13,896	1,597	351	1,597
R^2	.52	.59	.13	.36	.074	.24	.27	.16	.43	.11

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and batch fixed effects, with the exception of columns (5) and (10), which do not include the batch fixed effects because they are collinear with the date of invitation instrument. The dependent variable is whether the individual accepted Firm B's job offer. Columns (1)-(5) look before the reminder. Columns (6)-(10) look after the reminder. Columns (1)-(2) and (5)-(6) consider the full sample. The remaining consider the sample of individuals who were paid by Firm A. Columns (4) and (9) consider the hidden or no-noncompete individuals who were paid by Firm A but who did not read page 7 of their contract. Columns (2) and (6) include a control for whether the individual did not complete Firm A's task to ensure the appropriate comparison. Columns (5) and (10) include the inverse mills ratio, which uses the date of the initial job offer as an instrument for completing Firm A's task.

Firm B *before* Firm A's reminder. For these individuals, post-reminder those with a hidden noncompete are 16 percentage points less likely to join Firm B, a 41% decline from the no noncompete baseline of 39%. These effects are purely treatment effects, given that these individuals could not have selected into noncompetes.

Wage Equivalents. Like Firm A, Firm B also randomized wages. As such, we can scale our effects in dollars to provide context for their size (as we did for Firm A, described at the end of Section 4.2). The 8.8 percentage point reduction in the likelihood of joining Firm B due to the salient noncompete is the equivalent of paying $19.68 more per hour. For the hidden noncompete, the number is $42.37/hr. As with Firm A's wage equivalence, this comparison does not mean that Firm B must pay workers more to violate their noncompetes—it is simply a benchmarking equivalence.[24]

Taken with the wage equivalence to get workers to accept noncompetes and complete Firm A's task (in column (3) of Table 2), these results imply that it is easy to get workers into a hidden noncompete, but difficult to get them out of it. In particular, our results suggest it costs approximately six times as much to get a worker to break a *hidden* noncompete post-reminder as it does to get them into it ($42.4/$6.9). For *salient* noncompetes, the differential is just approximately 1.5 times ($19.7/12.95). Broadly, these patterns suggest that hidden noncompete clauses, plus reminders, can most effectively prevent workers from taking jobs with competitors, and that salient noncompetes are less effective, likely due to adverse selection.

5.4 Phase 2 Results: Information Spillovers

All recruiters at Firm A received resumes for promising candidates. As part of our experiment, we made slight textual alterations to these resumes before sending them to

[24] In Table C5 we investigate if Firm A's noncompete raised Firm B's wage bills, and we find no evidence that it did. This suggests that the harm to Firm B comes from making it more difficult to hire workers generally, not necessarily that Firm B has to pay workers more to break their noncompete.

Table 6: **Sharing Firm A Resumes with Firm B**

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	𝟙(Shared Firm A Resumes with Firm B)									
	Before the Reminder					After the Reminder				
Offered Salient	.00029					.00095				
	(.0021)					(.00073)				
Offered Hidden	.0014					.0013				
	(.0021)					(.00091)				
Signed Salient		.0023	.0011		.0023		.0084	.0094		.0045
		(.019)	(.017)		(.02)		(.0067)	(.0075)		(.006)
Signed Hidden		.012	.014	.046	.011		.011	.013	.002	.0086
		(.018)	(.018)	(.029)	(.019)		(.008)	(.0079)	(.0081)	(.0075)
Firm B Wage Offer	.00088***	.00051***	.0012***	.0019*	.0012***	.00015***	8.0e-06	.000064	-4.4e-07	-.000017
	(.00014)	(.00015)	(.00038)	(.001)	(.00043)	(.000057)	(.000062)	(.00018)	(.00016)	(.00019)
Inverse Mills Ratio					-.0062					.059
					(.11)					(.061)
Estimator	OLS	2SLS	OLS	OLS	Heckman	OLS	2SLS	OLS	OLS	Heckman
Sample	All	All	Paid by Firm A	Skippers	Paid by Firm A	All	All	Paid by Firm A	Skippers	Paid by Firm A
Weighted	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	0	.04	.04	.03	.04	0	0	0	0	0
KP RK Wald F		347.1					347.1			
Salient - Hidden	-.001	-.01	-.01		-.01	-.0003	-.002	-.003		-.004
	(.002)	(.02)	(.02)		(.02)	(.001)	(.01)	(.008)		(.01)
Salient Wage Equiv.	-.33	-4.59	-.95		-1.85	-6.32	-1045.93	-147.04		260.79
	(2.3)	(37)	(14)		(16)	(4.6)	(8,495)	(452)		(2,654)
Hidden Wage Equiv.	-1.59	-23.84	-12.25	-24.16	-9.11	-8.37**	-1358.35	-198.63	4576.2	504.33
	(2.4)	(37)	(16)	(21)	(16)	(3.9)	(9,920)	(515)	(1,601,388)	(5,759)
Observations	13,896	13,896	1,597	351	1,597	13,896	13,896	1,597	351	1,597
R^2	.053	.046	.12	.35	.056	.017	.011	.097	.28	.043

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and batch fixed effects, with the exception of columns (5) and (10), which do not include the batch fixed effects because they are highly collinear with the date of invitation instrument. The dependent variable is whether the individual accepted Firm B's job offer. Columns (1)-(5) look before the reminder. Columns (6)-(10) look after the reminder. Columns (1)-(2) and (5)-(6) consider the full sample. The remaining consider the sample of individuals who were paid by Firm A. Columns (4) and (9) consider the hidden or no-noncompete individuals who were paid by Firm A but who did not read page 7 of their contract. Columns (5) and (10) include the inverse mills ratio, which uses the date of the initial job offer as an instrument for completing Firm A's task.

recruiter subjects. These alterations ensured that if a recruiter discovered a candidate independently (e.g., through an online search or personal acquaintance), we could distinguish that source from our modified Firm A resume.

In Table 6, we use the same approaches of Equation (4) to study knowledge spillovers (sharing Firm A's resumes). On average, 4% of workers shared Firm A's resumes with Firm B at any point. Conditional on sharing a candidate from Firm A, 100% shared the modified version, i.e., none appeared to have discovered the same candidate independently. Across specifications in Table 6, we find consistent evidence that when Firm B

randomly pays higher wages, recruiters are more willing to share Firm A's documents with Firm B (in violation of their NDA with Firm A). These results validate our measure of information leakage, by showing that leakage does happen, and that our measure of leakage is responsive to randomized treatments (higher/lower wages). However, we find no detectable effects of noncompetes in Table 6, and can rule out small effects. This is true both pre-and post-reminder, and including in any subgroup (see Appendix E).

Expert Forecasts. Appendix Table C2 shows that experts forecast different results. Experts predicted that salient noncompetes would deter workers from joining Firm B pre-reminder. They also forecast that workers with salient noncompetes would be the least likely to join Firm B (post-reminder). In contrast, we find no mobility reductions pre-reminder. After the reminder, workers with salient noncompetes are *more* likely to violate them (compared to workers with hidden noncompetes). Experts also predicted that noncompetes would reduce secret sharing, but we find no evidence noncompetes for this (either pre- or post-reminder).

5.5 Sentiment Analysis and Early Release

Post-Reminder Sentiment Analysis. The reminder of the noncompete—which likely shocks subjective beliefs about the probability of enforcement—appears to play a key role in driving any compliance with the noncompete. To better understand the role of reminders, Appendix F analyzes the text of subjects' responses to the reminder using a sentiment analysis tool called DistilBERT (Sanh et al., 2019). Our results suggest that, regardless of whether workers had a hidden or salient noncompete, they did not respond positively to being reminded of that fact. Full details of our analysis are in Appendix F.

Job Search after Release of Noncompete. As a robustness check on our mobility results from Phase 2, in phase 3 Firm A randomly released workers early from their noncompetes. We measure their job application activity for another job opening within the

finance industry. Workers released early from their noncompete are about +20% more likely to apply, compared to not-yet-released individuals. Appendix G contains the full details of this final phase of our experiment. The results provide additional evidence that signing a noncompete causes workers to be less likely to apply for jobs at competitors.

6 Earnings

Our final question is how noncompetes affect workers' earnings. To study the causal effect of requiring and signing a noncompete, we estimate variants of Equation (3) with a Poisson model, given that individuals who do not work for Firm A or Firm B have zero earnings from those companies (Chen and Roth, 2022). We present our findings in Table 7. These models estimate the causal effect of Firm A requiring a noncompete in the first column of each panel. In the second and third columns, we show the causal effect of signing one (again using offers as an instrument for signing and studying the skipper LATE). We study earnings cumulatively as well as separately from Firm A and B.

We find that the firm requiring a noncompete reduces total earnings from Firm A and B by 12.2% when the noncompete is hidden, and 16.5% when it is salient. In general, these earnings differences could reflect (a) the fact that individuals select out of Firm A when offered a job with a noncompete (and thus do not get paid by Firm A), (b) any compensating differentials at Firm A or B arising from the noncompete, and (c) because noncompetes might deter an individual from working for (and thus being paid by) Firm B. Given that, as shown above, we find that our workers did not receive compensating differentials for noncompetes, we turn to the sorting effects (based on noncompetes) as potential explanations for earnings differences. Columns (4) and (7) reveal that the salient effect is driven by the fact that those offered a salient noncompete are more likely to turn down Firm A. In contrast, earnings losses from requiring hidden noncompete come from the fact that those offered a hidden noncompete are more likely to turn down Firm B.

Table 7: **Noncompetes and Earnings**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable	Earnings From Firms A & B			Earnings From Firm A			Earnings From Firm B		
Offered Salient	-.18**			-.17**			-.23		
	(.079)			(.077)			(.19)		
Offered Hidden	-.13*			-.069			-.58***		
	(.076)			(.075)			(.19)		
Signed Salient		-.0027			.011			-.09	
		(.028)			(.014)			(.19)	
Signed Hidden		-.043*	-.078**		.016	-.024		-.53***	-.51*
		(.022)	(.035)		(.011)	(.017)		(.19)	(.27)
Firm A Wage Offer	.034***	.022***	.02***	.038***	.025***	.025***	.011**	-.00096	-.018**
	(.0019)	(.00085)	(.0012)	(.0018)	(.00031)	(.00051)	(.0047)	(.0045)	(.0076)
Estimator	Poisson	IVPoisson	Poisson	Poisson	IVPoisson	Poisson	Poisson	IVPoisson	Poisson
Sample	All	All	Skippers	All	All	Skippers	All	All	Skippers
Weighted	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	6.29	52.1	53.92	5.4	44.92	45.53	.9	7.18	12.32
Salient - Hidden	-.05	.04		-.11	-.005		.35	.44*	
	(.08)	(.03)		(.08)	(.01)		(.22)	(.23)	
Observations	13,896	13,896	351	13,896	13,896	351	13,664	13,896	227

Notes: Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and fixed effects for the date of initial outreach, with the exception of the IV Poisson models. Due to convergence issues, the IV Poisson models include controls for gender, noncompete enforcement, asking rates, and race. Those models also include a control for the individual not completing Firm A's task, to ensure the comparison group for noncompete signers is to those who completed Firm A's task in the control group.

The results in columns (2) and (3) estimate the causal effect of signing a noncompete on total earnings from Firm A and B. We estimate that signing a hidden noncompete reduces earnings by 4.3% overall, and 7.5% for those who skipped page 7. The mechanism underlying these earnings losses is, as expected from the mobility analysis above, that these individuals make less money from Firm B (columns 8 and 9). In contrast, our estimates on the causal effect of signing a salient noncompete are small and imprecise. Because those signing salient noncompetes are more likely to work for Firm B (than hidden noncompetes), we fail to reject the hypothesis that signing a salient noncompete reduces earnings at Firm B (though again the estimates are imprecise).

Appendix Figures E6 to E8 examine heterogeneity in the causal effects of signing a noncompete across various subsamples. We find no subsample for which signing a noncompete (salient or hidden) increases earnings at standard levels of statistical significance. Signing the *hidden* noncompete reduces earnings across nearly all subsamples, while the estimates for signing a *salient* noncompete are noisier. The primary heterogeneity we

detect across both salient and hidden noncompetes is that noncompetes causes earnings losses for workers with high asking rates. Workers with low asking rates appear have positive but imprecisely estimated effects.

7 Discussion and Conclusion

In our final section, we discuss the generalizability of these results, and how they fit into the existing literature and policy conversations.

7.1 Generalizability

Contract Recruiters. It is important to consider how the results here might generalize to other settings. Contract HR professionals in finance are a unique population, but they differ in ways that could reasonably make them more averse to noncompete agreements than the average worker. HR professionals are likely aware of noncompetes, either from signing them themselves, or by understanding their effect on employee mobility.

As freelancers, these HR professionals should also be particularly wary of restrictions that could limit their ability to work in their chosen field. Finally, HR professionals are likely comfortable with negotiations over employment-related terms. Thus, the aversion to noncompetes that we document here, particularly in the salient condition, could be an overestimate of aversion to noncompetes in the general population. In this sense, it is likely easier to get a worker in a more typical employment situation to sign a noncompete, especially a hidden noncompete, than the HR professionals we study.

Inattention. Many results are driven by workers signing contracts without reading them. It is an empirical question whether workers in at-will-employment relationships read their contracts. Suggestive evidence on the extent to which at will workers read their noncompetes and negotiate over them in the US labor force from Starr et al. (2021) indicate that inattention is not specific to our sample. Using data from their paper, Figure C8

shows broadly that approximately 1/3 of workers report reading their noncompete either not at all or quickly, and that only approximately 10% of workers report negotiating over their noncompete. These patterns are not specific to any occupation, even if reading seems more common in some higher-skilled occupations. These results are consistent with inattention as a broader phenomenon.

Trade Secrets. As it relates to information sharing and knowledge spillovers, our results are specific to sharing resumes. Thus, our results are perhaps most likely to generalize to other contexts where client lists are a valuable source of information. They may be less generalizable to other types of trade secrets. It is unclear the extent to which this particular result will generalize to other workers who may be less accustomed to dealing with client lists and confidential information.

Contract Terms, Enforcement, and Firm-Driven Recruitment. Our results are specific to the particular language and terms of Firm A's noncompete. One could imagine changing the terms of the noncompete, including the duration and the scope. In addition, per our experimental protocol, we could not file a lawsuit to enforce the noncompete. If workers respond differently when they are served with a lawsuit, then our results are likely to underestimate the effects of noncompetes on worker mobility. Finally, in our field experiment Firm B actively solicited all workers hired by Firm A, even if they had a noncompete. Thus, our estimated reductions in mobility from noncompetes are likely too small, relative to what they would have been if Firm B had been dissuaded from making job offers to those with noncompetes ex ante.

7.2 Limitations and Additional Robustness Checks

Because we have mutually exclusive treatments (salient or hidden), our estimates are potentially subject to contamination bias (Goldsmith-Pinkham et al., 2022), despite randomization. Table C6 shows that our results are robust to accounting for this.

Another potential concern is that the sample weights we use as a default are skewed if some strata are largely underrepresented in the sample (and therefore have large sample weights). To assess how important weighting is in driving the overall estimates, in Appendix H we show the unweighted results from the main text are substantively similar.

We also ensure our results are robust to different approaches to calculating standard errors and multiple hypothesis testing. Table C7 shows that the first and second stage results are robust when accounting for testing multiple hypotheses, and Table H11 shows the results are robust to calculating p-values using randomization inference techniques.

Finally, we could only reliably gather earnings data from Firm A and Firm B. Subsequent studies can improve upon this by gathering more complete earnings data.

7.3 Contributions

The primary result in this study is that noncompetes reduce earnings and mobility, without adding any additional protection for trade secrets relative to a baseline contract with an NDA. These results push against positive noncompete-earnings correlations in the literature. They facially align with more plausibly causal studies of state noncompete policies (Marx et al., 2009; Johnson et al., 2023a). However, the results also suggest that studies of state policies do not fully estimate the effect of noncompete clauses because (a) noncompetes are still used regularly in states that won't enforce them (Prescott et al., 2016; Rothstein and Starr, 2022) and (b) we find that noncompetes cause reductions in mobility and earnings regardless of their actual enforceability in court. Finally, the results also contradict claims that noncompetes are necessary to protect trade secrets.

In addition, researchers and policymakers have advocated for clearer disclosure requirements for noncompetes so that workers can make more informed choices. Our results suggest that making noncompetes salient selects workers more likely to break the noncompetes *ex post*, and does not result in workers being paid more for accepting the noncompete. These results are consistent with our theory that such workers are selected

on their beliefs about the likely enforcement of the noncompete.

More broadly, our theoretical approach introducing inattention and uncertainty about the presence and enforcement of noncompetes could apply to other contract terms, including nondisclosure agreements (Sockin et al., 2021; Hoffman and Lampmann, 2019), nonsolicitation agreements (Graves, 2022), and training repayment agreements (Hoffman and Burks, 2017; Harris, 2020). It is conceivable that inattention similarly inhibits wage premiums for these other restrictive contracts, and likely cause *ex post* regret. Our experimental design may also offer an avenue for studying these contract terms empirically.

The causal effects of noncompetes on earnings, mobility, and information flows are important for the economics of labor, innovation and public policy. In this paper, we hoped to take a small step towards better understanding through a field experiment.

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Appendix: For Online Publication Only

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A Theoretical Appendix

A1 Proofs for Results in Main Paper

Lemma A1. *Consider two contracts, one that induces a distaste parameter of* Θ *and* $\eta = 0$*, and a second contract inducing a distaste parameter of* $\Theta' = \Theta - \delta$*, where* $\eta > \delta > 0$*.*

Wages for the first contract are $\phi_\Theta^{-1}(v)$, where

$$\phi_\Theta(w) = -\left[w - \frac{1 - F_\Theta(-w)}{f_\Theta(-w)}.\right]$$

Wages for the second contract are $\phi_\Theta^{-1}(v + \eta - \delta) + \delta$.

Proof. We begin by modeling the wages in the first contract. The derivative of the firm's utility (Equation 2) with respect to the wage is $u_f' = -f_\Theta(-w)\left(\phi_\Theta(w) - v\right)$, where

$$\phi_\Theta(w) = -\left[w - \frac{1 - F_\Theta(-w)}{f_\Theta(-w)}\right]$$

is the equivalent of Myerson (1981)'s "virtual value" in our setting.[25] Because of the regularity of F_Θ, $\phi_\Theta(w)$ is increasing (as is its inverse).

Riley and Zeckhauser (1983) show that within this setup, the optimal take-it-or-leave-it wage is where marginal revenue is zero, i.e., $w_{*\Theta} = \phi_\Theta^{-1}(v)$. For the second contract, the same approach yields the virtual value

$$\phi_{\Theta'}(w) = -\left[w - \frac{1 - F_{\Theta'}(-w)}{f_{\Theta'}(-w)}\right],$$

and the optimal wage $w_{*\Theta'} = \phi_{\Theta'}^{-1}(v + \eta)$. Because of the regularity of F_Θ, $F_{\Theta'}$ is also regular, and $\phi_{\Theta'}(w)$ is increasing (as is its inverse).

We will now write $\phi_{\Theta'}(w)$ as a function of $\phi_\Theta(w)$. Because Θ' is a left-shifted version of Θ by $\delta > 0$, we can transform the CDF and PDFs as follows:

$$F_{\Theta'}(x) = F_\Theta(x + \delta), \quad f_{\Theta'}(x) = f_\Theta(x + \delta).$$

Using the shift relationships for $F_{\Theta'}$ and $f_{\Theta'}$, we can substitute. The expression for $\phi_{\Theta'}(w)$ (the second contract) becomes:

$$\phi_{\Theta'}(w) = -\left[w - \frac{1 - F_\Theta(-w + \delta)}{f_\Theta(-w + \delta)}\right]. \tag{5}$$

Notice that if we evaluate $\phi_\Theta(w)$ at $w - \delta$, we get:

$$\phi_\Theta(w - \delta) = -\left[(w - \delta) - \frac{1 - F_\Theta(-w + \delta)}{f_\Theta(-w + \delta)}\right] \tag{6}$$

$$= \underbrace{-\left[w - \frac{1 - F_\Theta(-w + \delta)}{f_\Theta(-w + \delta)}\right]}_{\phi_{\Theta'}(w), \text{ Equation 5}} + \delta \tag{7}$$

$$= \phi_{\Theta'}(w) + \delta \tag{8}$$

Rearranging, this implies that $\phi_{\Theta'}(w) = \phi_\Theta(w - \delta) - \delta$. We now derive wages in the two contracts in two steps.

[25] The virtual value is negated here because agents gain disutility and are paid for work, rather than gaining utility and paying a price (as in the original paper's setting).

Step 1: Write $\phi_{\Theta'}^{-1}(y)$ in terms of ϕ_{Θ}^{-1}. Let $y = \phi_{\Theta'}(w)$. From our previous result,

$$y = \phi_{\Theta}(w - \delta) + \delta. \tag{9}$$

Solving 9 for w gives us the inverse of $\phi_{\Theta'}$. We proceed by subtracting δ from both sides:

$$y - \delta = \phi_{\Theta}(w - \delta). \tag{10}$$

Since ϕ_{Θ} is increasing, it is invertible, and its inverse ϕ_{Θ}^{-1} is also increasing. Apply ϕ_{Θ}^{-1} to both sides of Equation 10:

$$\phi_{\Theta}^{-1}(y - \delta) = w - \delta.$$

Solve for w:

$$w = \phi_{\Theta}^{-1}(y - \delta) + \delta.$$

As such, the inverse function $\phi_{\Theta'}^{-1}(y) = \phi_{\Theta}^{-1}(y - \delta) + \delta$.

Step 2: Derive $w_{*\Theta'}$ and $w_{*\Theta}$. Using the definitions derived above,

1. Deriving $w_{*\Theta'}$:

$$\begin{aligned} w_{*\Theta'} &= \phi_{\Theta'}^{-1}(v + \eta) \\ &= \phi_{\Theta}^{-1}(v + \eta - \delta) + \delta. \end{aligned} \tag{11}$$

2. For $w_{*\Theta}$,

$$w_{*\Theta} = \phi_{\Theta}^{-1}(v). \tag{12}$$

□

Lemma A2. *Consider two contracts, one that induces a distaste parameter of Θ and $\eta = 0$, and a second contract inducing a distaste parameter of $\Theta' = \Theta - \delta$, where $\eta > \delta > 0$. Wages in the second contract are higher.*

Proof. We begin by using the results of Lemma A1, showing that wages for the first contract are $\phi_{\Theta}^{-1}(v)$, and wages for the second contract are $\phi_{\Theta}^{-1}(v + \eta - \delta) + \delta$. We can now show the difference between the second and first are positive in two steps.

Step 1: Calculating the Difference $w_{*\Theta'} - w_{*\Theta}$. Subtract Equation 12 from Equation 11:

$$w_{*\Theta'} - w_{*\Theta} = \left[\phi_{\Theta}^{-1}(v + \eta - \delta) + \delta\right] - \phi_{\Theta}^{-1}(v) \tag{13}$$

$$= \underbrace{\left[\phi_{\Theta}^{-1}(v + \eta - \delta) - \phi_{\Theta}^{-1}(v)\right]}_{\Delta w} + \delta \tag{14}$$

$$= \Delta w + \delta. \tag{15}$$

Since ϕ_{Θ}^{-1} is an increasing function, the sign of Δw depends on the sign of the difference in the arguments. The difference in the arguments is:

$$(v + \eta - \delta) - v = \eta - \delta.$$

We have been given that $\eta > \delta > 0$, which implies $\eta - \delta > 0$. Therefore $v + \eta - \delta > v$. Since ϕ_{Θ}^{-1} is increasing:

$$\phi_{\Theta}^{-1}(v + \eta - \delta) > \phi_{\Theta}^{-1}(v).$$

Thus:

$$\Delta w = \phi_{\Theta}^{-1}(v + \eta - \delta) - \phi_{\Theta}^{-1}(v) > 0. \tag{16}$$

Step 2: Determine the Sign of $w_{*\Theta'} - w_{*\Theta}$. Above we showed that:

$$w_{*\Theta'} - w_{*\Theta} = \Delta w + \delta.$$

We know that $\Delta w > 0$ from Equation (16), and were given that $\delta > 0$ Therefore:

$$w_{*\Theta'} - w_{*\Theta} = (\Delta w) + \delta > 0 + \delta = \delta > 0. \tag{17}$$

So:

$$w_{*\Theta'} - w_{*\Theta} > \delta > 0.$$

This shows 1) wages are higher under the second contract Θ'. □

A1.1 Proof for Proposition 1

Proposition 1 (Full Information). *If $\hat{p}_n = p_n$ and $\hat{p}_e = p_e$, then $w_{NC} > w_{\emptyset}$.*

Proof. This is an application of Lemma A2. Lemma A2 sets up a comparison of two contracts.

One induces a distaste parameter of Θ and $\eta = 0$ (corresponding to no-noncompete), and a second contract inducing a distaste parameter of $\Theta' = \Theta - \delta$ (shifted to the left), where $\eta > \delta > 0$. In this application, the δ of Lemma A2 is equal to $\hat{p}_n \hat{p}_n k$ or simply k (because this setting of transparent noncompetes features $\hat{p}_n = \hat{p}_n = 1$. Lemma A2 shows that in this setup, wages for the transparent noncompete are higher than no noncompete. □

A1.2 Proof for Corollary 1

Corollary 1 (No Regret). *If $\hat{p}_n = p_n$ and $\hat{p}_e = p_e$, then $u_i(w_{NC}) \geq 0$ for all workers who sign a noncompete.*

Proof. By not signing the noncompete, the worker could consume zero. The workers only sign if the payoff $w + d_i + k \geq 0$, because there are no ex-post realizations of unknown variables. As such, all workers who signed are better off than had they not signed. □

A1.3 Proof for Proposition 2

Proposition 2 (Undetected Noncompetes). *If $\hat{p}_n = \bar{p}_n$, then $w_H > w_\emptyset$.*

Proof. This is an application of Lemma A2. Lemma A2 sets up a comparison of two contracts. One induces a distaste parameter of Θ and $\eta = 0$ (corresponding to no-noncompete), and a second contract inducing a distaste parameter of $\Theta' = \Theta - \delta$ (shifted to the left), where $\eta > \delta > 0$. In our application to Proposition 2, the δ of Lemma A2 is equal to $\hat{p}_n \hat{p}_n k$. Lemma A2 shows that in this setup, wages for the undetectable noncompete are higher than no noncompete. □

A1.4 Proof for Corollary 3

Corollary 3 (Undetected Regret). *If $\hat{p}_n = \bar{p}_n$, then there exists d_i such that $\mathbb{E}[u_i(w_H, d_i)] \geq 0$, but $u_i(w_H, d_i) < 0$ if $p_n = 1$.*

Proof. By not singing the noncompete, the worker could consume zero. Lemma A1 shows that wages for the hidden noncompete is $\phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) + \hat{p}_n \hat{p}_e k$. All workers signed for whom

$$\begin{aligned} d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) &\geq 0 \\ \implies d_i &\geq -\phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) \end{aligned}$$

Let $\underline{d}_i$ refer to the lowest type (highest distaste for work) that still signs the contract.

The $\bar{p}$ proportion of workers who signed the contract had it enforced experienced a payoff of:

$$d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) - k$$

These workers are better off if

$$\begin{aligned} d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) - k &\geq 0 \\ \implies d_i &\geq -\phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) + k \end{aligned}$$

Let $\underline{d}'_i$ refer to the lowest type (highest distaste for work) that is better off signing the contract, even when it is enforced. We know that $\underline{d}'_i > \underline{d}_i$ because:

$$-\phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) + k \geq -\phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k)$$

and $k > 0$. Together, this means that all workers whose Θ distaste parameter lies between $[\underline{d}_i, \underline{d}'_i]$ have ex-post payoffs below their outside offer when they sign a contract containing a hidden noncompete. □

A1.5 Proof for Proposition 3

Proposition 3 (Worker Selection when Detected). *If $\hat{p}_n = p_n = 1$, then w_{NC} is accepted only if $\hat{p}_e$ is sufficiently low, or if d_i is sufficiently high.*

Proof. Lemma A1 shows that wages in the noncompete are equal to $\phi_\Theta^{-1}(v+\eta-\hat{p}_e k)+\hat{p}_e k$. Note that the worker signs the noncompete only if

$$d_i + \phi_\Theta^{-1}(v+\eta-\hat{p}_e k) \geq 0 \tag{18}$$

Note that having a higher d_i makes the inequality hold. Because $\vec{\phi}_\Theta^{-1}(\cdot)$ is increasing in its argument, lower $\hat{p}_e$ will increase $\phi_\Theta^{-1}(v+\eta-\hat{p}_e k)$ and make the inequality hold. □

A1.6 Proof for Proposition 4

Proposition 4 (Detected with Low Wage Premium). *If $\hat{p}_n = p_n = 1$, there exists an upper bound $\bar{\delta}$ such that $\forall \bar{\delta} \geq \epsilon > 0$, the noncompete wage premium $w_{NC} - w_\emptyset = \epsilon$.*

Proof. Lemma A1 shows that wages for the detectable noncompete is $\phi_\Theta^{-1}(v+\eta-\hat{p}_e k)+\hat{p}_e k$, and wages no noncompete (detectably) are simply $\phi_\Theta^{-1}(v)$. The wage premium is thus the difference:

$$\Delta(\eta, \hat{p}_e) = \phi_\Theta^{-1}(v+\eta-\hat{p}_e k)+\hat{p}_e k - \phi_\Theta^{-1}(v) \tag{19}$$

Consider Δ over the two-dimensional intervals $\hat{p}_e \in [0,1]$ and $\eta \in [0,\bar{\eta}]$. On these intervals,

- Δ reaches a lower bound $\underline{\delta}$. Note that the lower bound is less than zero ($\underline{\delta} \leq 0$) because $\Delta(\cdot,\cdot)$ equals zero if $\eta = 0$ and $\hat{p}_e = 0$.
- We also know that $\Delta(\cdot,\cdot)$ reaches a maximum $\bar{\delta}$. We know that $\bar{\delta} \geq 0$, because $\Delta(\cdot,\cdot) > 0$ if $\eta > 0$ and $\hat{p}_e = 0$.

Let $\epsilon > 0$ represent an arbitrarily small wage difference ($0 < \epsilon \leq \bar{\delta}$). For some value of $\hat{p}_e$ and η, we know that

$$\Delta(\eta, \hat{p}_e) = \epsilon. \tag{20}$$

This is because Δ is continuous and reaches a max and min over an interval. As such, from the multidimensional intermediate value theorem (i.e., the Poincaré-Miranda theorem), Δ reaches ϵ at some point over the interval. □

A1.7 Proof for Proposition 5

Proposition 5 (Detected Regret). *If $\hat{p}_n = p_n = 1$ and $\hat{p}_e \in (0,1)$, then there exists d_i such that $\mathbb{E}[u_i(w_H, d_i)] \geq 0$, but $u_i(w_{NC}, d_i) < 0$ if $p_e = 1$.*

Proof. This proof parallels the proof for Corollary 3. By not singing the noncompete, the worker could consume zero. Assume that $\hat{p}_n = 1$. From Lemma A1, the optimal wage for the hidden noncompete was $\phi_\Theta^{-1}(v+\eta-\hat{p}_e k)+\hat{p}_e k$. All workers signed for whom

$$\begin{aligned} d_i + \phi_\Theta^{-1}(v+\eta-\hat{p}_e k) &\geq 0 \\ \implies d_i &\geq -\phi_\Theta^{-1}(v+\eta-\hat{p}_e k) \end{aligned}$$

Let $\underline{d}_i$ refer to the lowest type (highest distaste for work) that still signs the contract. The $\bar{p}$ proportion of workers who signed the contract had it enforced experienced a payoff of:

$$d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_e k) - k$$

These workers are better off if

$$\begin{aligned} d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_e k) - k \geq 0 \\ \implies d_i \geq -\phi_\Theta^{-1}(v + \eta - \hat{p}_e k) + k \end{aligned}$$

Let $\underline{d}'_i$ refer to the lowest type (highest distaste for work) that is better off signing the contract, even when it is enforced. We know that $\underline{d}'_i > \underline{d}_i$ because:

$$-\phi_\Theta^{-1}(v + \eta - \hat{p}_e k) + k \geq -\phi_\Theta^{-1}(v + \eta - \hat{p}_e k)$$

and $k > 0$. Together, this means that all workers whose Θ distaste parameter lies between $[\underline{d}_i, \underline{d}'_i]$ have ex-post payoffs below their outside offer when they sign a contract containing a visible noncompete. □

A2 Supplementary Results and Proofs

Proposition A1. *Suppose that workers cannot detect noncompetes, and have an overoptimistic belief about the base-rate of noncompetes ($\hat{p}_n \leq \bar{p}_n$). Some workers who sign the noncompete are ex ante worse off.*

Proof. Lemma A1 shows that wages for this noncompete is $\phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) + \hat{p}_n \hat{p}_e k$. Given the true rate of noncompetes $\bar{p}_n$, a worker will be *ex ante* better off signing the contract if:

$$d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) + \hat{p}_n \hat{p}_e k - \underline{p}_n \hat{p}_e k \geq 0 \tag{21}$$

$$\implies d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) + \hat{p}_e k(\hat{p}_n - \underline{p}_n) \geq 0 \tag{22}$$

$$\implies d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) - \hat{p}_e k(\underline{p}_n - \hat{p}_n) \geq 0 \tag{23}$$

$$\implies -\phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) + \hat{p}_e k(\underline{p}_n - \hat{p}_n) \leq d_i \tag{24}$$

Let $\underline{d}_i$ represent the lowest type that is still ex ante better off. We now calculate the lowest type that signs the contract.

$$d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) \geq 0 \tag{25}$$

$$\implies d_i \geq -\phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) \tag{26}$$

Let $\underline{d}'_i$ represent the lowest type that signs the contract. We can now show that $\underline{d}'_i \leq \underline{d}_i$.

$$\underline{d}'_i \leq \underline{d}_i \tag{27}$$

$$-\phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) \leq -\phi_\Theta^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) + \hat{p}_e k(\underline{p}_n - \hat{p}_n) \tag{28}$$

$$0 \leq \hat{p}_e k(\underline{p}_n - \hat{p}_n) \tag{29}$$

The final inequality holds because $\hat{p}_e \geq 0$, $k \geq 0$, and because $\bar{p}_n \geq \hat{p}_n$ (because of the workers' overoptimism about the rate of noncompetes).

This means there are workers whose distaste d_i lies in $[\underline{d}_i', \underline{d}_i]$ who sign the contract even though they are ex ante worse off (because of their overoptimism about the presence of a noncompete). □

Proposition A2. *Suppose that workers exhibit hyperbolic discounting (Laibson, 1997). Although the net present value of lost wages is k, the worker discounts this further to βk, where $\beta \in (0,1)$. They do not discount the up-front wage, which is paid immediately. Under this form of present bias, some workers who sign the noncompete are ex ante worse off.*

Proof. Lemma A1 shows that wages for this noncompete is $\phi_\Theta^{-1}(v + \eta - \hat{p}_n\hat{p}_e\beta k) + \hat{p}_n\hat{p}_e\beta k$. Given the true penalty of $k > \beta k$, a worker will be *ex ante* better off signing the contract if:

$$d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_n\hat{p}_e\beta k) + \hat{p}_n\hat{p}_e\beta k - \hat{p}_n\hat{p}_e k \geq 0 \tag{30}$$
$$\implies d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_n\hat{p}_e\beta k) + \hat{p}_n\hat{p}_e k(\beta - 1) \geq 0 \tag{31}$$
$$\implies d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_n\hat{p}_e\beta k) - \hat{p}_n\hat{p}_e k(1 - \beta) \geq 0 \tag{32}$$
$$\implies -\phi_\Theta^{-1}(v + \eta - \hat{p}_n\hat{p}_e\beta k) + \hat{p}_n\hat{p}_e k(1 - \beta) \leq d_i \tag{33}$$

Let $\underline{d}_i$ represent the lowest type that is still ex ante better off. We now calculate the lowest type that signs the contract.

$$d_i + \phi_\Theta^{-1}(v + \eta - \hat{p}_n\hat{p}_e\beta k) \geq 0 \tag{34}$$
$$\implies d_i \geq -\phi_\Theta^{-1}(v + \eta - \hat{p}_n\hat{p}_e\beta k) \tag{35}$$

Let $\underline{d}_i'$ represent the lowest type that signs the contract. We can now show that $\underline{d}_i' \leq \underline{d}_i$.

$$\underline{d}_i' \leq \underline{d}_i \tag{36}$$
$$-\phi_\Theta^{-1}(v + \eta - \hat{p}_n\hat{p}_e\beta k) + \hat{p}_n\hat{p}_e k(1 - \beta) \leq -\phi_\Theta^{-1}(v + \eta - \hat{p}_n\hat{p}_e\beta k) + \hat{p}_n\hat{p}_e k(1 - \beta) \tag{37}$$
$$0 \leq \hat{p}_n\hat{p}_e k(1 - \beta) \tag{38}$$

The final inequality holds because $\hat{p}_e \geq 0$, $k \geq 0$, and because $\beta - 1$ (because of the workers' present bias).

This means there are workers whose distaste d_i lies in $[\underline{d}_i', \underline{d}_i]$ who sign the contract even though they are ex ante worse off (because of their present bias and discount of future wage penalties). □

B Experimental Details

In this section we describe several of the details underlying the experiment.

B1 Stratification and Batching

We used a stratified random sample of about 14K recruiters. The strata in the sample were based on i) whether the recruiter's jurisdiction allowed noncompete agreements to be enforced,[26] ii) the hourly rate on the recruiter's profile (quartiles), iii) gender, and iv) degree of prior experience in finance, HR, and/or freelancing on this platform. This produced 64 strata.

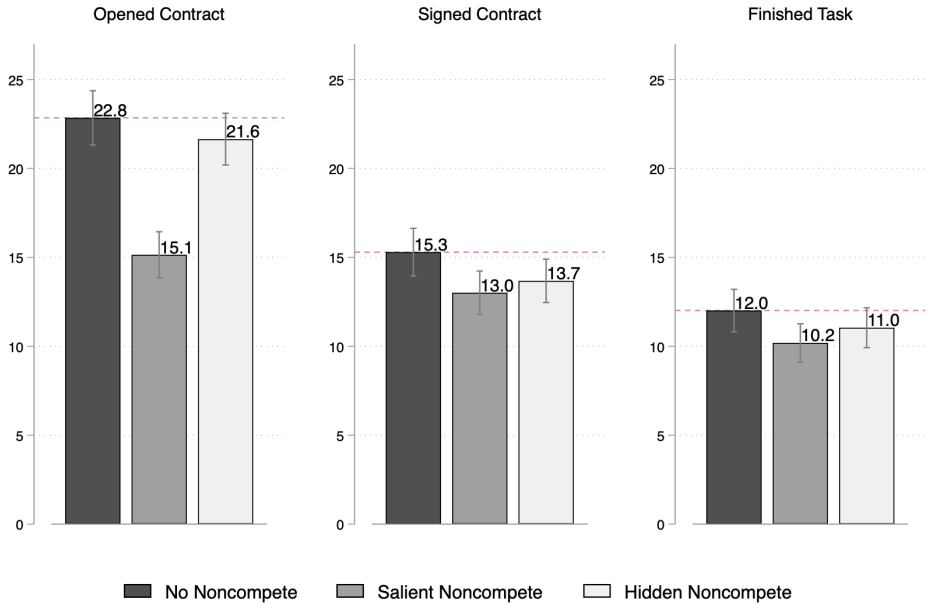
To incorporate the randomized components into our stratified sample and determine our final sample, we randomized treatment assignments within strata, with equal proportions assigned across the six treatment groups, and then checked for balance on a set of observables for our randomization in the unweighted sample—and re-randomized if there were any differences in observables across the treatment assignments detectable at $p \leq 0.2$. Note that the variables used to construct strata were balanced by construction. The other observables we used to check balance were i) deciles of total freelancing revenue on the platform, ii) the average amount of activity on the platform, and iii) whether the subject was predicted to be white based on a name classifier. The balance table from the unweighted invited sample is shown in Table C1.

Finally, we randomly selected one recruiter from each stratum and placed them into 478 batches. Job offers were sent in these batches. Each batch was sent out within the same short period of time (a 2-6-hour window). This permits us to include batch fixed effects (timing) as well as strata fixed effects (characteristics) in our specifications. Strata sizes varied. For example, there were a high number of subjects with finance experience in the highest quartiles of asking price, and a lower number without finance experience in these quartiles. Batches were constructed by selecting one subject from each stratum, and thus small strata would run out of subjects before larger strata. As a result, the batch sizes also varied. Batches were sent out in the order of the batch size. This guaranteed that we had some coverage for each stratum.

[26] We based this classification on the coding of state noncompete laws described here.

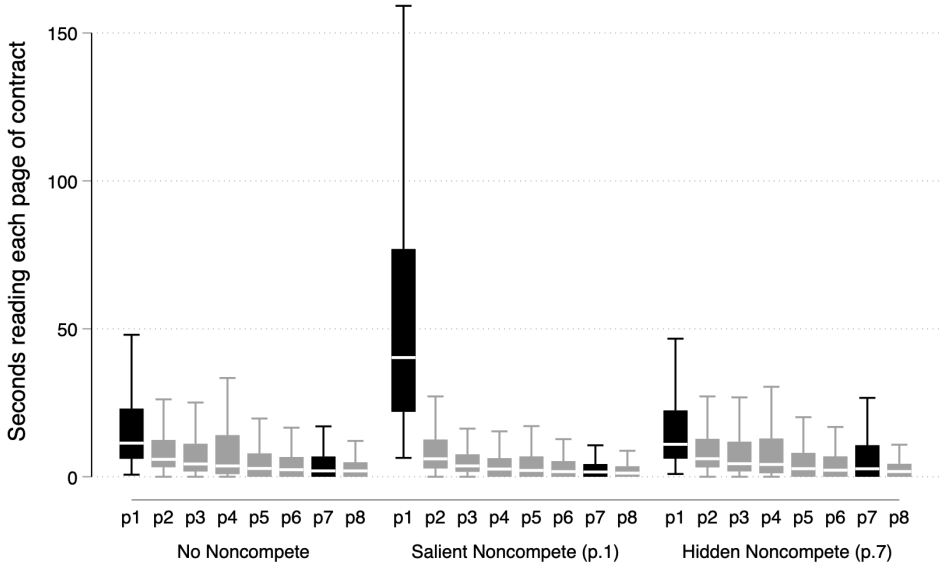
C Supplemental Tables and Figures

Figure C1: **Among those invited, what percentage opened the contract, signed it, and completed the task?**



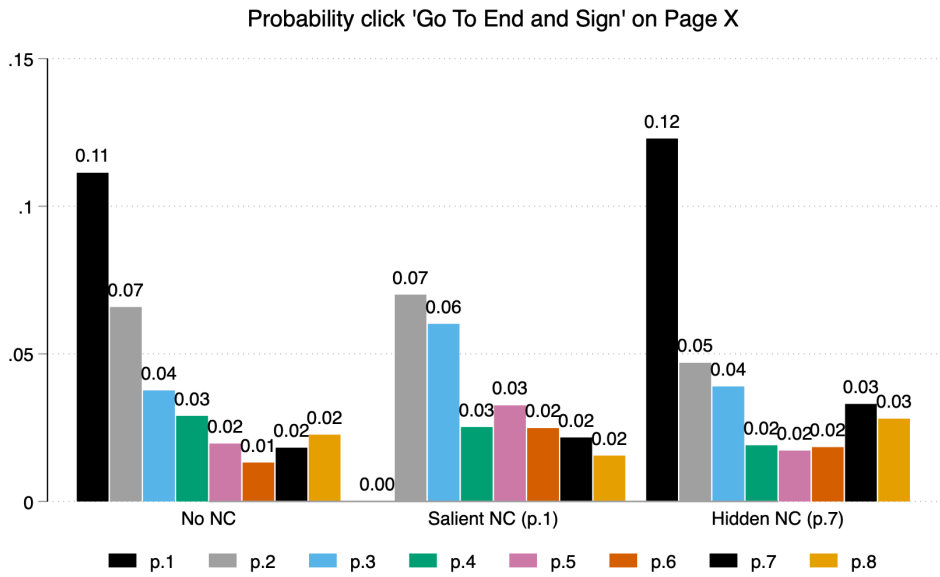
Notes: This figure shows the weighted probabilities in the invited sample of opening the contract, signing the contract, and completing Firm A's task, according to the noncompete treatment condition.

Figure C2: **Box and Whisker Plot of Time Spent Reading Each Page of Contract**



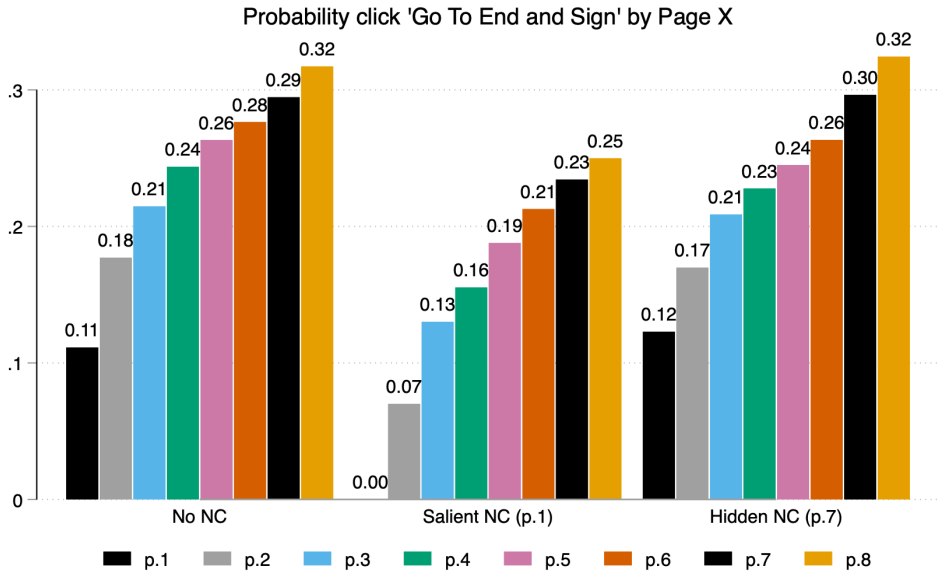
Notes: Page 1 of the Salient noncompete condition is the noncompete page. Page 7 of the Hidden noncompete condition is the noncompete page. Results are weighted. The sample is all those who opened the contract.

Figure C3: **Probability of Skipping on a Given Page**



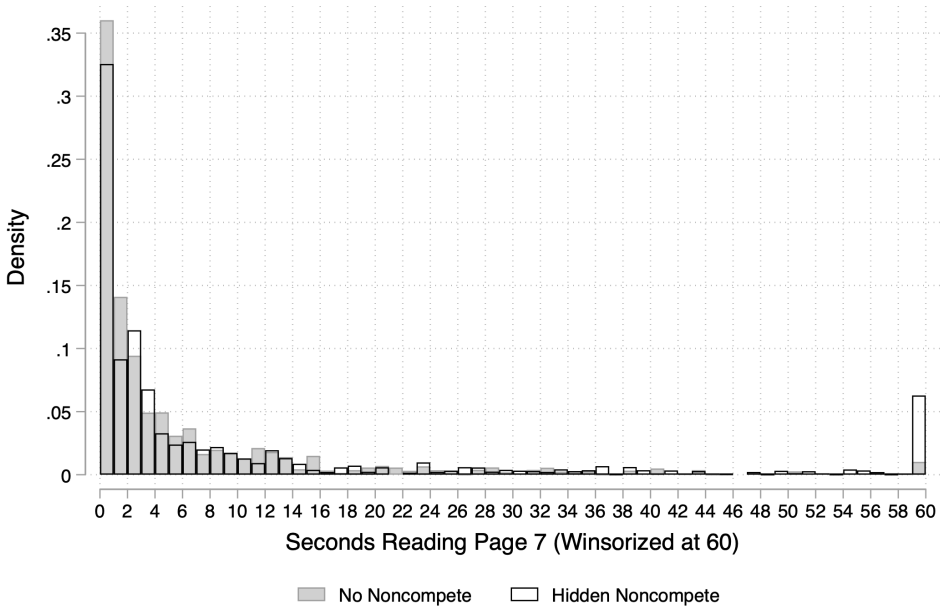
Notes: This figure shows the probability that somebody who opened the contract hit skip on each page. The sample includes those who opened the contract.

Figure C4: **Probability of Skipping On or Before a Given Page**



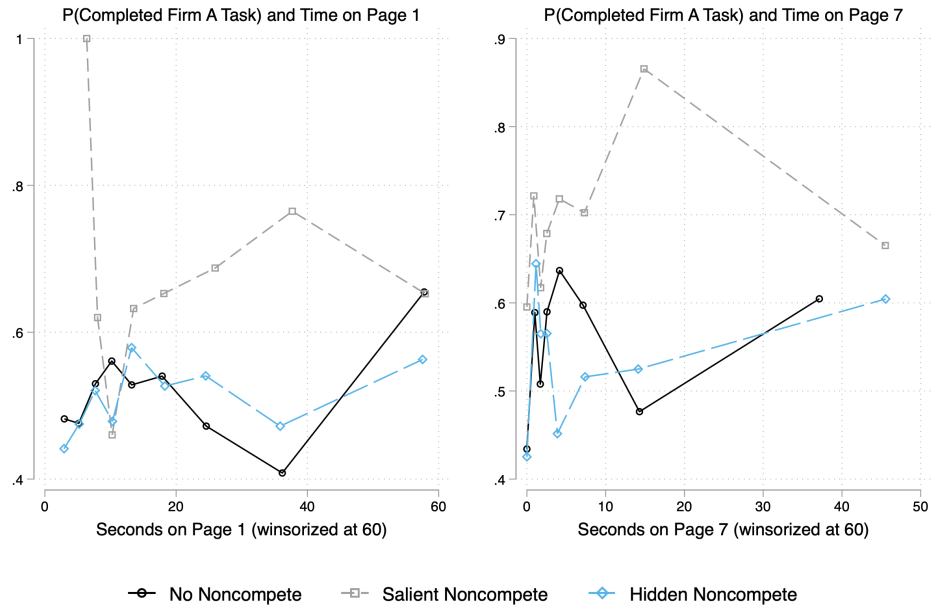
Notes: This figure shows the probability that somebody who opened the contract hit skip on or before each page. The sample includes those who opened the contract.

Figure C5: **Histogram of Time Spent Reading Page 7**



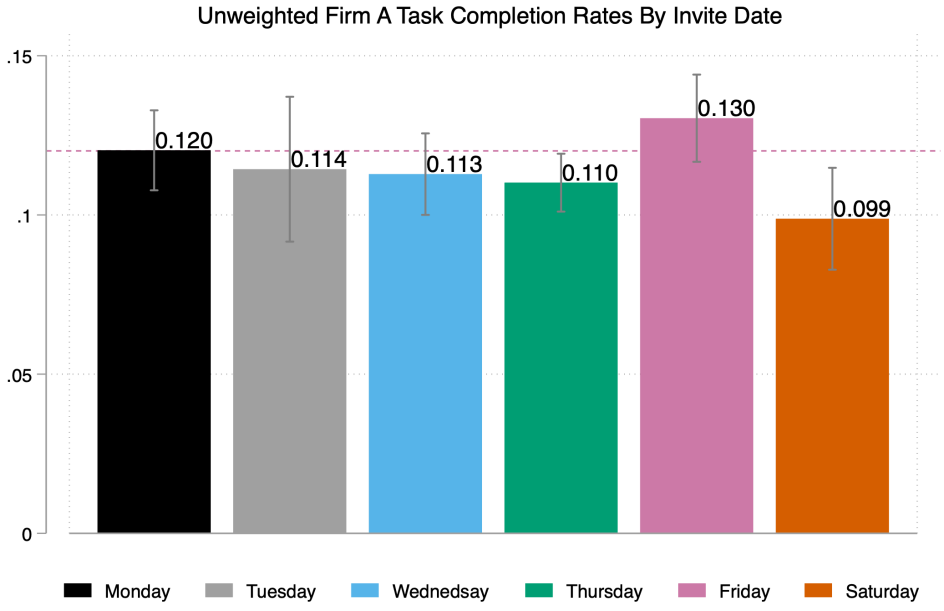
Notes: Results are weighted. Reading variables are winsorized at 60 seconds. The sample are all those who opened the contract.

Figure C6: **Probability of Completing Firm A Task Given Reading on Page 1 and 7**



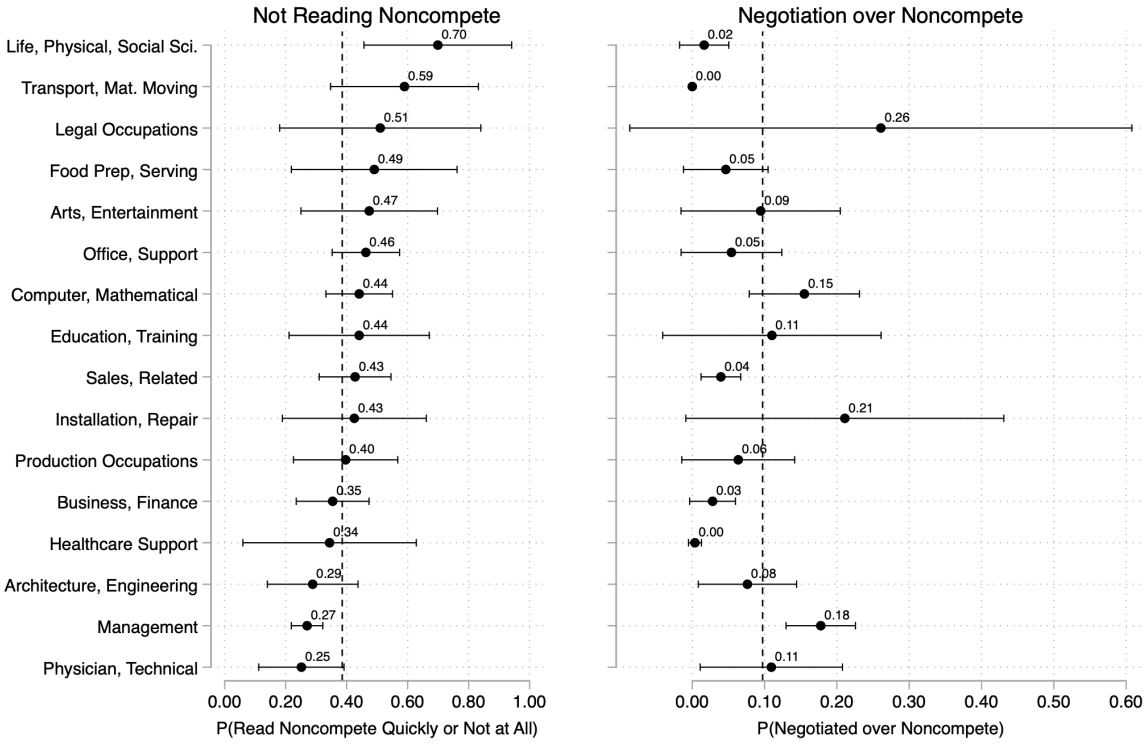
Notes: Reading variables are winsorized at 60 seconds.The sample includes those who opened the contract.

Figure C7: **Variation in Instrumental Variable for Heckman Selection Correction**



Notes: This figure shows how Firm A task completion rates vary by the day of the week the individual was invited. The instrument is the randomized invite date.

Figure C8: **Reading and Negotiating Over Noncompetes in the US Labor Force**



Notes: This figure uses the sample of noncompete signers from the nationally representative survey in Starr et al. (2021). It shows the occupation-specific (weighted) probability of not reading or reading quickly the noncompete, and the probability of negotiating over the noncompete or for other benefits in exchange for signing. Occupations with less than 15 observations are dropped.

Table C1: **Balance Table among the Invited Sample**

Treatment	Recruit Experience	Finance Experience	Female	Hourly Rate	Enforcement State	Predicted White
Hidden Noncompete, $25/hr	0.75	0.59	0.55	54.73	0.57	0.74
Hidden Noncompete, $60/hr	0.75	0.59	0.55	54.93	0.57	0.74
No Noncompete, $25/hr	0.76	0.58	0.55	54.84	0.57	0.75
No Noncompete, $60/hr	0.77	0.57	0.56	55.55	0.57	0.75
Salient Noncompete, $25/hr	0.77	0.57	0.54	54.23	0.57	0.74
Salient Noncompete, $60/hr	0.76	0.58	0.55	54.88	0.57	0.74

Notes: This table shows the average level of each variable in the unweighted, invited sample across the six treatment conditions.

Table C2: **Expert Forecasts**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Predictions from Experts					
	Actual	US Sample			Full Sample		
	Estimate	Mean	Median	N	Mean	Median	N
% Signing Contract							
No Noncompete	15.3						
Salient noncompete	13.0	10.7	10	34	16.5	11.2	74
Hidden noncompete	13.7	15.8	14	34	22.1	14	74
% Joining Firm B Pre-Reminder							
No Noncompete	64.2						
Salient noncompete	69.1	35.5	36.5	33	37.8	40	72
Hidden noncompete	66.1	48.4	50	33	47.6	50	73
% Joining Firm B Post Reminder							
No Noncompete	36.8						
Salient noncompete	28.1	17.8	18	33	19.9	20	72
Hidden noncompete	17.4	21.4	20	33	23.3	20	72
% Secret Sharing Pre-Reminder							
No noncompete	4.2	37.9	35	33	40.1	45	71
Salient noncompete	4.6	27.3	25	33	30.7	25	71
Hidden noncompete	5.4	37.3	35	33	40	40	71
Secret Sharing Post-Reminder							
No noncompete	0.4	25.3	20	33	32	28	71
Salient noncompete	1.2	21.4	15	33	25.7	20	71
Hidden noncompete	1.5	21.5	15	33	29.3	25	71

Notes: The expert forecasts come from the social science prediction platform. Column (1) shows the actual percentages from the study, while columns (2)-(4) report statistics for the US sample of experts and (5)-(7) include the full sample. Note that in the questions related to signing the contract and joining firm B, the respondents were given the base rate found in the study from the control group. See Starr, Evan, Cowgill, Bo, and Brandon Freiberg. 2024. "Noncompetes Field Experiment." Social Science Prediction Platform. April 3.
https://socialscienceprediction.org/s/xfy0c6.

Table C3: **Reading Page 7 and Signing Contract**

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	𝟙(Signed contract)				
Sample:	Those who spent ___ seconds on p.7				
	0	> 0	> 15	> 30	> 45
Salient	.25***	.15***	.21**	.055	.42
	(.049)	(.026)	(.08)	(.18)	(.33)
Hidden	.022	-.059**	-.11	-.22*	-.49*
	(.045)	(.029)	(.069)	(.13)	(.25)
Firm A Wage Offer	.0021*	.0013*	-.0016	-.0057**	-.00039
	(.0011)	(.00069)	(.0018)	(.0027)	(.0046)
Weighted	Y	Y	Y	Y	Y
Control Mean	.53	.72	.71	.68	.87
Observations	975	1,877	369	171	87
R^2	.17	.12	.36	.57	.84

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and fixed effects for the date of initial outreach. The dependent variable is an indicator for signing the contract. The five columns are limited to the sample who spent at least the amount indicated in the column heading reading page 7 of the contract.

Table C4: **Reading Page 7 and Completing Firm A Task**

	(1)	(2)	(3)	(4)
Dependent Variable:	𝟙(Completed Firm A Task)			
Sample:	Quartile of Seconds on p.7			
	1st	2nd	3rd	4th
Salient	.18***	.14**	.098*	.25***
	(.05)	(.059)	(.056)	(.073)
Hidden	-.021	.071	-.11**	.00024
	(.045)	(.06)	(.053)	(.06)
Firm A Wage Offer	.002*	.0025	.0039***	-.0023
	(.0011)	(.0016)	(.0014)	(.0016)
Weighted	Y	Y	Y	Y
Control Mean	.44	.53	.6	.53
Min Seconds on p.7	0	.747	2.699	10.67
Max Seconds on p.7	0.745	2.698	10.666	3939.09
Observations	1,019	641	678	507
R^2	.15	.21	.24	.28

Notes: * p<0.1, ** p<0.05, *** p<0.01. The sample is conditioned on reaching page 7. Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and fixed effects for the date of initial outreach. The dependent variable is an indicator for signing the contract. The four columns are limited to the quantiles of the reading time on page 7 of the hidden noncompete group. The range of reading times for each quantile is given by the Min and Max rows.

Table C5: **Noncompetes and Firm B's Wage Bills**

	(1) Overall	(2) Paid \$25/hr	(3) Paid \$60/hr	(4) Avg. \$/hr	(5) Avg. \$ Billed
Salient	.0079	.026	-.18***	-3.6*	.76
	(.045)	(.044)	(.052)	(2.2)	(2.9)
Hidden	-.11***	-.096***	-.27***	-.17	4.7
	(.037)	(.036)	(.051)	(2.5)	(2.9)
$\mathbb{1}$(Firm B Offered \$62/hr)	.26***				
	(.047)				
Salient $\times$ $\mathbb{1}$(Offered \$62)	-.19***				
	(.069)				
Hidden $\times$ $\mathbb{1}$(Offered \$62)	-.15**				
	(.061)				
Firm A Wage Offer	.00027	.0009	.000095	.005	-.083
	(.00078)	(.00097)	(.0012)	(.057)	(.078)
Weighted	Y	Y	Y	Y	Y
Control Mean	.36	.24	.49	50.41	19.41
Observations	1,597	792	803	452	452
R^2	.16	.24	.21	.34	.32

Notes: Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and fixed effects for the date of initial outreach. The dependent variable in columns (1)-(3) is an indicator for completing Firm B's task. The sample in column (1) is all workers paid by Firm A, and in column (2) and (3) it is split out into workers offered \$27/hr and \$62/hr, respectively. In columns (4) and (5), the sample is limited to those who completed Firm A's task. The dependent variable in column (4) is average hourly wages, and the dependent variable in column (5) is the total amount billed by each worker, which can be different from their hourly wage because of how much time they spent working on the task.

Table C6: **Adjusting for Contamination Bias**

Panel A: Phase 1 Outcomes

	(1)	(2)	(3)	(4)
Dependent Variable	𝟙(Opened Contract)	𝟙(Signed Contract)	𝟙(Completed Task)	𝟙(Bargained)
Salient	-.077***	-.023**	-.018**	.002
	(.01)	(.0091)	(.0081)	(.0021)
Hidden	-.012	-.016*	-.0097	.003
	(.011)	(.0091)	(.0082)	(.0022)
Weighted	Y	Y	Y	Y
Control Mean	.22	.15	.12	0
Observations	13,896	13,896	13,896	13,896

Panel B: Phase 2 Outcomes

	(1)	(2)	(3)	(4)
Dependent Variable	𝟙(Accepted Firm B)		𝟙(Shared Firm A Resumes)	
	Pre-Reminder	Post-Reminder	Pre-Reminder	Post-Reminder
Salient	.046	-.092***	-.0019	.0067
	(.036)	(.032)	(.015)	(.0071)
Hidden	.016	-.19***	.015	.012
	(.032)	(.03)	(.017)	(.0072)
Weighted	Y	Y	Y	Y
Control Mean	.64	.36	.04	0
Observations	1,598	1,598	1,598	1,598

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. This table shows results from the procedure developed in Goldsmith-Pinkham et al. (2022) to account for contamination bias. The results shown are from the EW approach using their 'multe' command in Stata. In Panel A and B we estimate equations (3) and (4), respectively.

Table C7: **Adjusting for Multiple Hypothesis Testing**

Panel A: Phase 1 Outcomes

	(1)	(2)	(3)	(4)
Dependent Variable	𝟙(Opened Contract)	𝟙(Signed Contract)	𝟙(Completed Task)	𝟙(Bargained)
Salient	-.077***	-.023**	-.018**	.002
P-value	(0.000)	(0.013)	(0.025)	(0.355)
Q-sharpened	[0.001]	[0.048]	[0.065]	[0.338]
Romano-Wolf	{0.0006}	{0.107}	{0.151}	{0.648}
Hidden	-.012	-.016*	-.0097	.003
P-value	(0.262)	(0.079)	(0.241)	(0.188)
Q-sharpened	[0.28]	[0.169]	[0.28]	[0.28]
Romano-Wolf	{0.606}	{0.324}	{0.606}	{0.575}
Observations	13896	13896	13896	13896

Panel B: Phase 2 Outcomes

	(1)	(2)	(3)	(4)
Dependent Variable	𝟙(Accepted Firm B)		𝟙(Shared Firm A Resumes)	
	Pre-Reminder	Post-Reminder	Pre-Reminder	Post-Reminder
Salient	.045	-.088***	.0013	.0094
P-value	(0.235)	(0.010)	(0.939)	(0.210)
Q-sharpened	[0.28]	[0.048]	[0.537]	[0.28]
Romano-Wolf	{0.606}	{0.094}	{0.931}	{0.606}
Hidden	.017	-.19***	.015	.013
P-value	(0.622)	(0.000)	(0.409)	(0.110)
Q-sharpened	[0.537]	[0.001]	[0.357]	[0.21]
Romano-Wolf	{0.782}	{0.002}	{0.648}	{0.421}
Observations	1597	1597	1597	1597

Notes: * p<0.1, ** p<0.05, *** p<0.01. In Panel A and B we estimate equations (3) and (4), respectively. For each coefficient we report the p-value from the main text in parentheses, the Q-sharpened p-values from Benjamini et al. (2006) in brackets, and the Romano Wolf adjusted p-values from Clarke et al. (2020) with 10,000 bootstrapped replications in curly braces.

D Job Performance

In this section, we examine how randomly requiring noncompetes in a contract affects performance on Firm A's work. While not formally a part of our theoretical framework, noncompetes could affect the quantity or quality of work by selecting of the workers willing to accept the job. There may also be a treatment effect from signing a noncompete on performance.

We study two main performance metrics. First, Firm A asked the workers to justify their various choices among the candidates they reviewed. We measured how many characters they used to explain their decisions, which is plausibly a measure of worker effort. Second, we count the total number of possible mistakes that workers made in their submission. Mistakes include incomplete feedback, mixing up the maximum salary offer with the minimum salary offer, and others. These include entering the wrong name of the candidate, not evaluating all eight candidates, evaluating the same candidate more than once, submitting salary offers that are either way too low, or mixing up the maximum salary offer with the minimum salary offer. Workers who did not complete Firm A's job were coded as a zero for these variables.

We estimate three versions of Equation (3) using Poisson regressions (given that the dependent variables are counts), described in detail in 5.2. Briefly, the first version estimates the causal effect of the firm requiring a noncompete. The second uses the random offer of a noncompete to instrument for signing one. The third is not causal, but looks at how noncompetes relate to performance among those who completed Firm A's task.

The results, shown in Table D1, suggest little impact of noncompetes on task performance, with the exception that requiring a salient noncompete reduces the length of the notes by 19.7%. This effect is natural, however, given that salient noncompetes reduce completion rates (and those who did not complete provide no notes). There is some evidence that signing a hidden noncompete causes better performance than signing a salient one. However, given the lack of evidence against the control condition, it is not clear how to interpret these differentials.

Table D1: **Task Performance**

	(1)	(2)	(3)	(4)	(5)	(6)
	Number of Characters in Notes			Mistakes		
Offered Salient	-.22*			-.083		
	(.12)			(.16)		
Offered Hidden	.061			-.24		
	(.11)			(.15)		
Signed Salient		-.055	-.039		.06	.13
		(.099)	(.095)		(.15)	(.13)
Signed Hidden		.14	.14		-.16	-.13
		(.093)	(.083)		(.13)	(.12)
Firm A Wage Offer	.018***	.0055**	.0047**	.0086**	-.0035	-.0037
	(.0029)	(.0023)	(.0022)	(.0037)	(.0033)	(.0029)
Sample	All	All	Paid by Firm A	All	All	Paid by Firm A
Weighted	Y	Y	Y	Y	Y	Y
Estimator	Poisson	IVPoisson	Poisson	Poisson	IVPoisson	Poisson
Control Mean	126.98	1025.42	1025.42	.49	4.03	4.03
Salient - Hidden	-.28**	-.2*	-.17*	.15	.22*	.25**
	(.12)	(.1)	(.09)	(.14)	(.12)	(.12)
Salient Wage Equivalent	12.15*	9.99	8.36	9.56	17.42	34.46
	(6.5)	(19)	(20)	(18)	(43)	(44)
Hidden Wage Equivalent	-3.33	-26.42	-29.12	27.35	-45.73	-33.73
	(6.1)	(22)	(23)	(20)	(61)	(41)
Observations	13,881	13,896	1,597	13,896	13,896	1,597

Notes: * p<0.1, ** p<0.05, *** p<0.01. All models include strata fixed effects and batch fixed effects. Standard errors reported in parentheses, clustered by individual. The dependent variable in columns (1)-(3) is a count of the number of characters in the notes that subjects used to explain their choices. The dependent variable in column (4)-(6) is a count of mistakes the subject made in submitting their documentation to Firm A. All models include strata fixed effects and fixed effects for the date of initial outreach, with the exception of the IV Poisson regression in column (2) and (5). Due to convergence issues, column (2) and (5) models include controls for gender, noncompete enforcement, asking rates, and race. To ensure the comparison group for noncompete signers is to those who completed Firm A's task in the control group, column (2) and column (5) also includes a control for the individual not completing Firm A's task.

E Heterogeneity

In this section we consider which subgroups are driving the main results regarding selection into noncompetes, mobility to Firm B, secret sharing, and earnings.

We focus first on completing Firm A's task, corresponding to column (3) of Table 2. Prior research has found, for example, that banning noncompetes was especially beneficial for women and racial minorities (Lipsitz and Starr, 2022; Johnson et al., 2023), suggesting that

we might expect that such groups may be particularly averse to getting into them *ex ante*. Other research has questioned how workers might see noncompetes when their states do not enforce noncompetes (Starr et al., 2020). To examine these ideas, we limit our sample based on various characteristics and then re-estimate Equation (3).

Figure E1 shows heterogeneity in the relationship between noncompetes and completing Firm A's task for various subgroups. In no subgroup do we see any deterrent effect of the hidden noncompete. Focusing on the salient noncompete, while the effects are similarly negative for many subgroups, two sources of heterogeneity that seem relevant are the gender and asking rate differentials. Men are only 0.3 percentage points less likely to sign a contract with a salient noncompete vs. a contract with no noncompete, while women are 2.4 percentage points less likely to sign a salient noncompete.

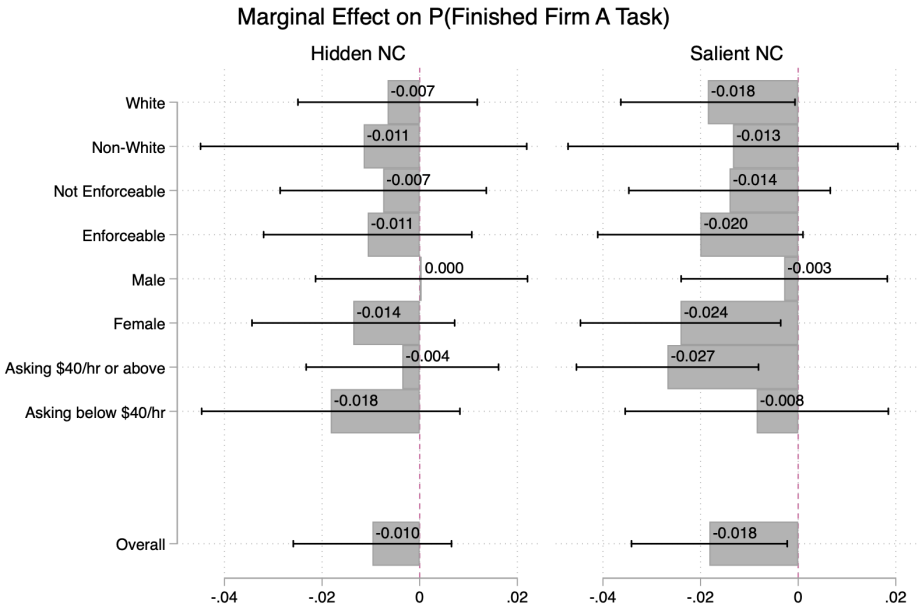
Turning to Phase 2, we next consider heterogeneity in the causal effect of signing a noncompete on mobility to Firm B, both pre- and post-reminder. Figure E2 shows the 2SLS model corresponding to column (2) of Table 5. Similarly, Figure E3 examines the 2SLS model in column (7) of the same table for post-reminder mobility.

The results in Figure E2 suggest some interesting heterogeneity. Pre-reminder, we see no evidence that a signing a noncompete reduces mobility. In fact, we find that the causal effect of signing a salient noncompete increases mobility in states where the noncompete is per se unenforceable. In contrast, in Figure E3, we find that all groups were much less likely to join Firm B as a result of signing a hidden noncompete, while all of the marginal effects for the salient noncompete are approximately half as large.

Figures E4 and E5 examine heterogeneity in the causal effect of Firm A requiring noncompetes on subsequent secret sharing. In no subsample, before or after the reminder, do we find evidence that requiring noncompetes, whether salient or hidden, reduced secret sharing.

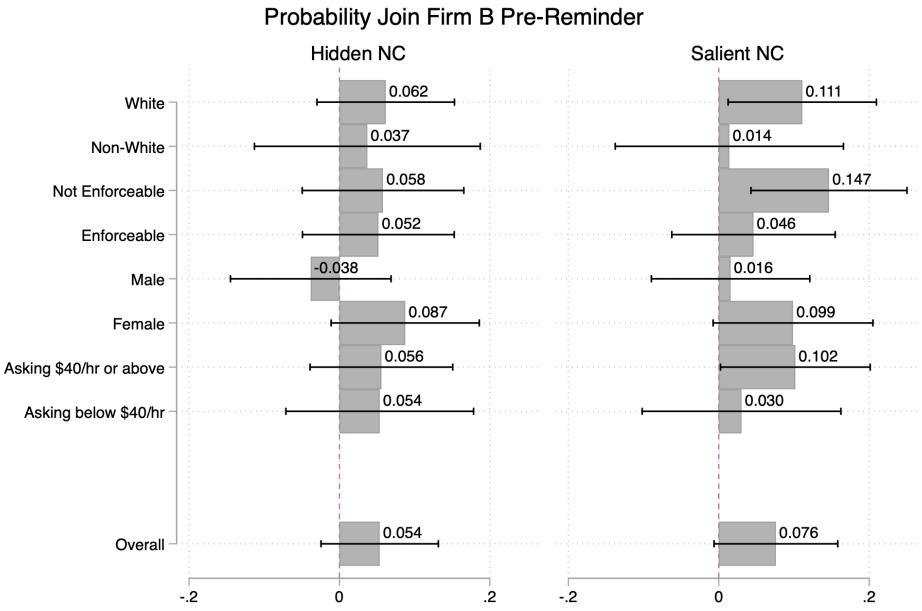
Finally, Figures E6 to E8 show subsample analyses corresponding to the causal effect of signing a noncompete on earnings, both cumulative from Firm A and B and separately. For no subsample do we find that noncompetes, either salient or hidden are positively and statistically significantly cause higher earnings. With one exception, the cumulative results for hidden noncompetes suggest that signing a hidden noncompetes reduces earnings similarly across groups. The one exception is that those with low-asking rates do not appear to have suffered earnings losses on average, though the estimates are imprecise. The results from signing a salient noncompete are in general noisier, though as in the hidden case, signing a salient noncompete for those with high asking rates reduces earnings.

Figure E1: **Heterogeneity in Causal Effect of Noncompete on Completing Firm A's Task**



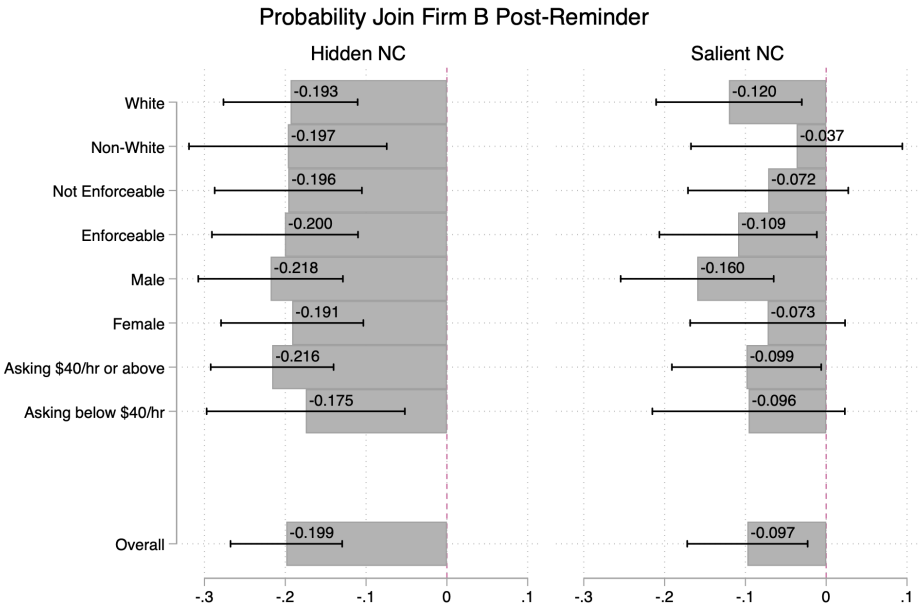
Notes: This figure shows heterogeneity in the coefficients on hidden and salient noncompetes related to completing Firm A's Task. The sample is limited to each subsample listed on the y axis, and then Equation (3) is estimated on that subsample and the coefficients and 95% confidence intervals on the hidden and salient noncompete variables are shown.

Figure E2: **Heterogeneity in Causal Effect of Signing Noncompete on Joining Firm B Pre-Reminder, 2SLS Model**



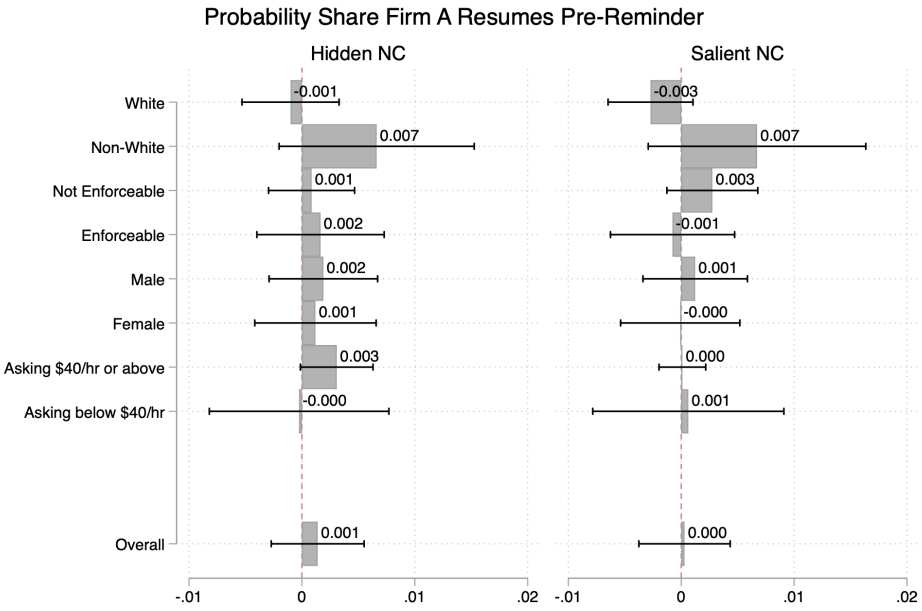
Notes: This figure shows subsample results from column (2) of Table 5.

Figure E3: **Heterogeneity in Causal Effect of Signing Noncompete on Joining Firm B Post-Reminder, 2SLS Model**



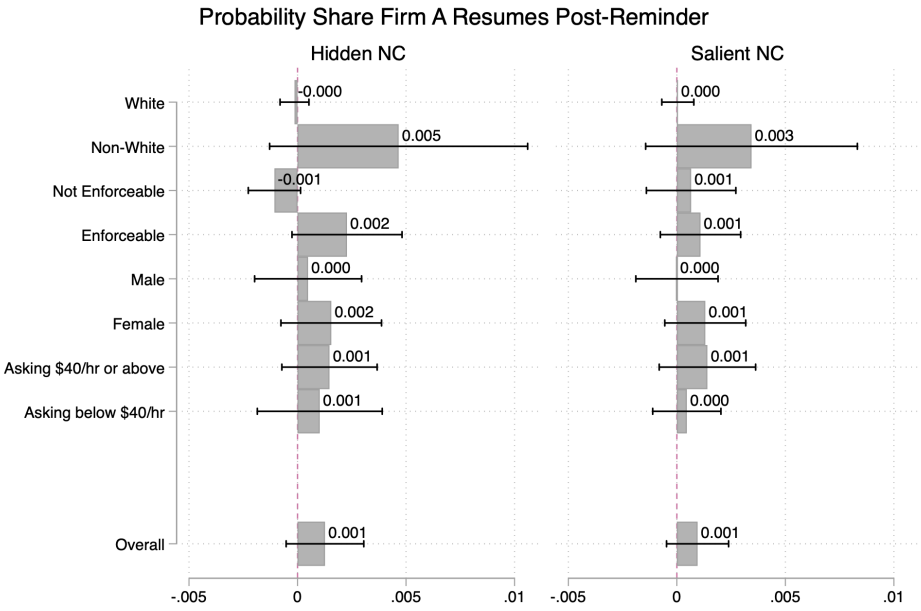
Notes: This figure shows subsample results from column (6) of Table 5.

Figure E4: **Heterogeneity in Causal Effect of Firm A Requiring Noncompetes on Sharing Firm A's Resumes Pre-Reminder**



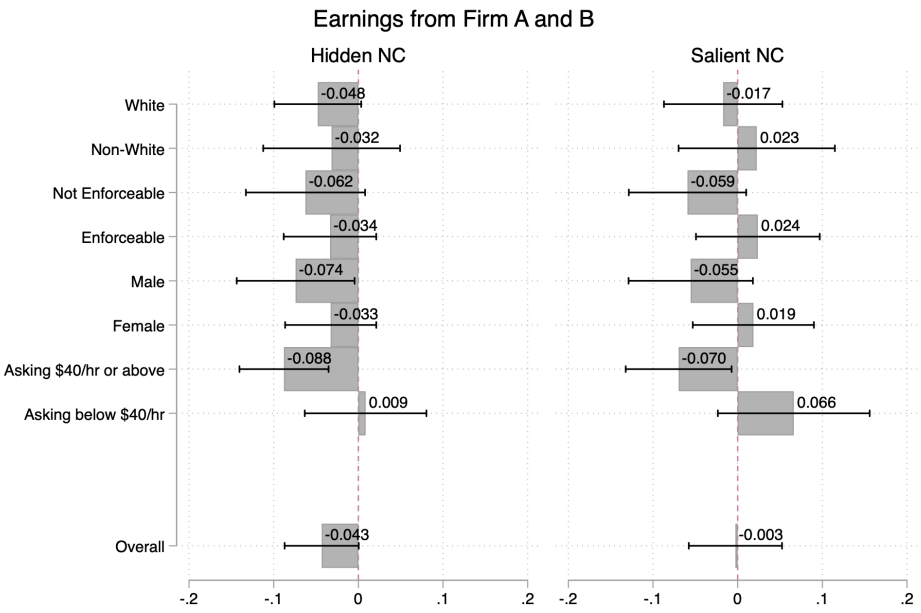
Notes: This figure shows subsample results from column (1) of Table 6.

Figure E5: **Heterogeneity in Causal Effect of Firm A Requiring Noncompetes on Sharing Firm A's Resumes Post-Reminder**



Notes: This figure shows subsample results from column (6) of Table 6.

Figure E6: **Heterogeneity in Causal Effect of Signing a Noncompete on Firm A and B Earnings, 2SLS model**



Notes: This figure shows subsample analyses of column (2) of Table 7.

Figure E7: **Heterogeneity in Causal Effect of Signing a Noncompete on Firm A Earnings, 2SLS model**



Notes: This figure shows subsample analyses of column (5) of Table 7.

Figure E8: **Heterogeneity in Causal Effect of Signing a Noncompete on Firm B Earnings, 2SLS model**



Notes: This figure shows subsample analyses of column (8) of Table 7.

F Post-Reminder Sentiment Analysis

The reminder of contractual obligations—which likely shocks subjective beliefs about the probability of enforcement—appears to play a key role in driving any compliance with the noncompete. Without it, the evidence above suggests that noncompetes may have little effect. To better understand the role of reminders, we turn to a textual analysis of the post-reminder responses of the subjects. The most common responses to the reminder were of the form 'Thank you for letting me know.' Others indicated fear of breaking their contract ("The risk is too high, hopefully in the future we may cross paths again").

To systematically analyze the sentiment of the post-reminder interactions, we deployed a sentiment analysis tool called DistilBERT, which classified the post-reminder messages as having positive sentiment or negative sentiment (or no response). In the control group, approximately 13% of responses were coded as negative, while 46% were coded as positive. Table F1 reports regression results from Equation (4) of the salient and hidden noncompete groups who finished Firm A's task (columns 1 and 2) or who are in the hidden or no noncompete group and skipped page 7 entirely (columns 3 and 4). Column (1) documents that those bound by noncompetes were 6 percentage points more likely to respond negatively to the reminder, while column (2) indicates that noncompete signers were 15-18 percentage points less likely to respond positively. We observe similar patterns for skippers. This analysis suggests that, regardless of whether workers had a hidden or salient noncompete, they did not respond positively to being reminded of that fact.

Table F1: **Post-Reminder Sentiment**

	(1)	(2)	(3)	(4)
Sample:	Finished Firm A's Task		... And Skipped p.7	
Dependent Variable: Sentiment is	𝟙(Negative)	𝟙(Positive)	𝟙(Negative)	𝟙(Positive)
Salient	.064**	-.15***		
	(.029)	(.037)		
Hidden	.067**	-.18***	.067	-.27***
	(.028)	(.035)	(.046)	(.063)
Firm B Wage Offer	.00031	-.00013	.0018	.002
	(.00066)	(.00085)	(.0015)	(.0018)
Weighted	Y	Y	Y	Y
Control Mean	.13	.46	.13	.58
Salient - Hidden	-.003	.03		
	(.03)	(.03)		
Observations	1,597	1,597	351	351
R^2	.07	.11	.37	.46

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and fixed effects for the date of initial outreach and Firm A's offered wage. The dependent variable is an indicator for whether the sentiment of the response was positive or negative. The sample is limited to those who performed Firm A's task. Columns (3) and (4) are those who skipped page 7.

G Phase 3: Job Search after (Randomized) Release of Noncompete

As a robustness check on the mobility results from Phase 2, the third phase of the experiment involves randomly releasing the workers from their noncompetes, and measuring their applications to another job. Figure G1 illustrates the experiment. As workers are approximately 3-4 months into their 6-month noncompete, Firm B posted a new job opening for a recruiter onto the platform (using a separate account). This job would later be advertised to workers at randomly selected times.

A few weeks after the job is posted, two cross-randomized treatment schedules begin. On each day, a few workers are randomly shown the job ad. Meanwhile, another set of randomly selected workers are sent a message from Firm A. By this point in the experiment, all workers who signed the noncompete had been reminded (in Phase 2). The new message from Firm A reminds workers of their nondisclosure obligations, and releases them from their noncompete (if they had one).[27] The message can be found in Appendix B6.

This process continued until all workers were both (i) sent the remind/release message, and (ii) shown the job ad. Crucially, the *sequence* of these two events were randomized. Some were randomly released before seeing the ad, and some were randomly released after seeing it. We track who applies for the job posting every day. We thus created a daily panel over a 36-day period. The panel starts with posting of the ad, and concludes two-weeks after the final set of workers were messaged.

G1 Empirical Approach

Were workers more likely to respond to the job ad if they had been released from their noncompete? We estimate this using the following triple differences specification:

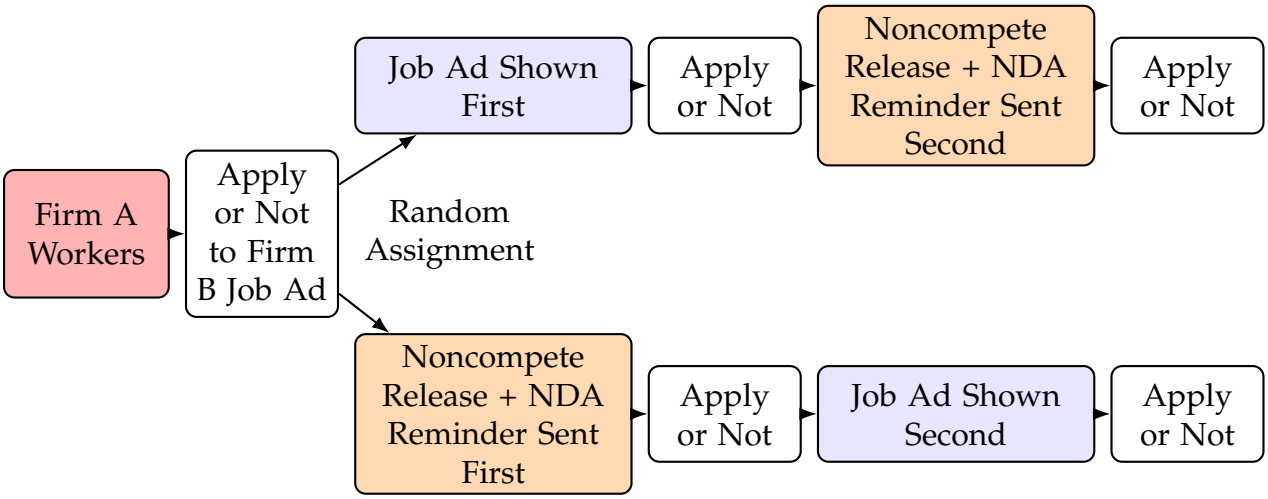
$$\begin{aligned} y_{it} = \beta_1 \mathbb{1}(Noncompete_i) + \beta_2 \mathbb{1}(SentJobAd_{it}) + \beta_3 \mathbb{1}(Released_{it}) \\ + \beta_4 \mathbb{1}(Noncompete_i)\mathbb{1}(SentJobAd_{it}) + \beta_5 \mathbb{1}(Noncompete_i)\mathbb{1}(Released_{it}) \\ + \beta_6 \mathbb{1}(SentJobAd_{it})\mathbb{1}(Released_{it}) + \beta_7 \mathbb{1}(Released_{it})\mathbb{1}(SentJobAd_{it})\mathbb{1}(Noncompete_i) \\ + \beta_8 w_i^B + \beta_9 w_i^A + \gamma_{s(i)} + \lambda_{t(i)} + \alpha_t + \epsilon_i, \quad (39) \end{aligned}$$

where α_t refers to date fixed effects. The triple differences estimate β_7 compares whether workers applied for Firm B's job, based on (a) whether they had signed a noncompete (first difference), (b) whether they had been sent the job ad by time t (second difference), and (c) whether they had been released by time t (third difference). In contrast, β_4 reflects differences in job application rates among those who have yet to be released/reminded by Firm A, based on whether the worker has a noncompete or not (first difference) and whether they have been invited to Firm B's job (second difference).

An attractive element of the Phase 3 experiment is that the coefficient of interest (β_7) can be interpreted as the average treatment effect of signing a noncompete. The random release did not induce changes in selection into noncompetes: Although workers may have

[27] Workers who did not have a noncompete still received a message reminding them about their nondisclosure agreement.

Figure G1: **Phase 3 Experimental Design Summary**



Notes: This figure describes Phase 3 of our experiment. Prior to this part of our experiment, a job opening was posted on a job search platform. We randomized when workers were alerted to the job ad, as well as the release of a noncompete. The release of the noncompete came with a reminder of the ongoing NDA obligations. Workers in the control group who did not sign a noncompete were only sent a reminder of the ongoing NDA obligation. We then measured whether Firm A's former workers applied to the job or not.

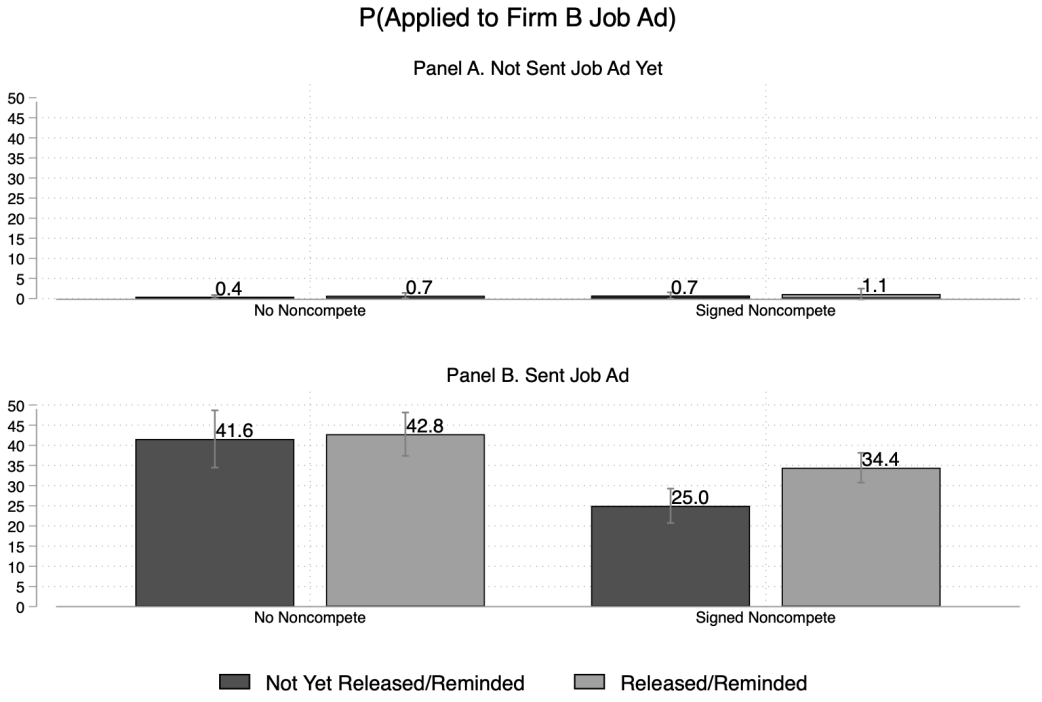
selected into having a noncompete (or not), the randomized release happens conditional on this selection. This approach allows workers to select into noncompetes and estimates the treatment effect of release. Because Equation (39) conditions on having signed a noncompete, any differences in selection differences are controlled for.

G2 Phase 3 Results

Figure G2 visualizes the results from the random release analysis. The top two panels reveal that, without being directly sent Firm B's job ad, few workers apply. In the bottom panel, after workers were sent the job ad, approximately 42% of those without a noncompete applied for Firm B's job. This number was roughly the same, regardless of whether they yet been sent the reminder message. However, among those who had a noncompete and had received the job ad, those who had not yet been released from the noncompete applied to Firm B at a rate of 25%, compared to 34% among those who had been released.

Table G1 formally tests these differences using Equation (39). Based on column (3), these models suggest that being released from a noncompete increased application rates to Firm B's job posting by 20% (0.068/0.3). They also suggest that not being released from a noncompete reduces willingness to apply by 43% (0.13/0.3). Table G2 reports similar effects with the Heckman correction approach, and Tables G4 and G3 report similar results for those with hidden and salient noncompetes.

Figure G2: **Applying to Firm B's Job Ad**



Notes: This figure shows the probability that the workers applied to Firm B's job ad in Phase 3. It cuts the data based on whether the worker had been invited to apply yet (top row vs. bottom row), whether they had a noncompete (left two columns vs. right two columns) and sent the reminder/release yet (left bar vs. right bar within each group).

Table G1: **Phase 3 Release and Applications**

	(1)	(2)	(3)
Dependent Variable	𝟙(Applied for Firm B Job)		
Sample	Noncompete	No Noncompete	Full Sample
Released/Reminded	.0016	-.0033	.018
	(.014)	(.025)	(.014)
Sent Job Ad	.25***	.36***	.38***
	(.022)	(.034)	(.035)
Released/Reminded × Sent Job Ad	.067***	.016	.0068
	(.018)	(.027)	(.028)
Noncompete			-.0027
			(.012)
Released/Reminded × Noncompete			-.015
			(.01)
Sent Job Ad × Noncompete			-.13***
			(.04)
Released × Sent Job Ad × Noncompete			.068**
			(.033)
Weighted	Y	Y	Y
Control Mean	.24	.3	.3
Observations	35,770	19,460	55,230
R^2	.22	.31	.21

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. All models include strata fixed effects, fixed effects for the date of initial outreach, Firm A's offered wage in Phase 1, Firm B's offered wage in Phase 2, and day fixed effects. The dependent variable is an indicator for whether the individual applied for Firm B's job posting. The sample are only those who completed Firm A's task and who were available to be reached by the reminder/release message. Column 1 is the noncompete sample, column 2 is the no noncompete sample, and column 3 is the full sample. The mean of the DV in column 3 is the no noncompete group mean.

Table G2: **Phase 3 Release and Applications, With Heckman Correction**

	(1)	(2)	(3)
Dependent Variable	𝟙(Applied for Firm B Job)		
Sample	Noncompete	No Noncompete	Full Sample
Released/Reminded	.002	-.0037	.018
	(.014)	(.025)	(.014)
Sent Job Ad	.25***	.36***	.38***
	(.022)	(.034)	(.035)
Released/Reminded × Sent Job Ad	.066***	.017	.0068
	(.018)	(.027)	(.028)
Noncompete			-.0031
			(.022)
Released/Reminded × Noncompete			-.015
			(.01)
Sent Job Ad × Noncompete			-.13***
			(.04)
Released × Sent Ad × Noncompete			.068**
			(.033)
Inverse Mills Ratio	.25	-2.9**	.011
	(.55)	(1.1)	(.53)
Weighted	Y	Y	Y
Control Mean	.24	.3	.3
Observations	35,770	19,460	55,230
R^2	.22	.31	.21

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. All models include strata fixed effects, fixed effects for the date of initial outreach, Firm A's offered wage in Phase 1, Firm B's offered wage in Phase 2, and day fixed effects. The dependent variable is an indicator for whether the individual applied for Firm B's job posting. The sample are only those who completed Firm A's task and who were available to be reached by the reminder/release message. Column 1 is the noncompete sample, column 2 is the no noncompete sample, and column 3 is the full sample. The mean of the DV in column 3 is the no noncompete group mean. The inverse mills ratio comes from the first-stage Heckman selection model using invite date as an instrument.

Table G3: **Phase 3 Release and Applications by Hidden and Salient**

	(1) Hidden	(2) Salient	(3) No Noncompete	(4) Full Sample
Released/Reminded	.0025	-.0037	-.0033	.019
	(.022)	(.024)	(.025)	(.014)
Sent Job Ad	.26***	.27***	.36***	.38***
	(.027)	(.033)	(.034)	(.035)
Released/Reminded × Sent Job Ad	.047**	.086***	.016	.0062
	(.024)	(.023)	(.027)	(.028)
Hidden				-.0072
				(.012)
Salient				.0031
				(.016)
Released/Reminded × Hidden				-.017
				(.012)
Released/Reminded × Salient				-.013
				(.013)
Sent Job Ad × Hidden				-.12***
				(.045)
Sent Job Ad × Salient				-.14***
				(.045)
Released × Sent Job Ad × Hidden				.049
				(.037)
Released × Sent Job Ad × Salient				.089**
				(.038)
Weighted	Y	Y	Y	Y
Control Mean	.22	.25	.3	.3
Observations	18,445	17,325	19,460	55,230
R^2	.3	.31	.31	.21

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. All models include strata fixed effects, fixed effects for the date of initial outreach, Firm A's offered wage in Phase 1, Firm B's offered wage in Phase 2, and day fixed effects. The dependent variable is an indicator for whether the individual applied for Firm B's job posting. The sample are only those who completed Firm A's task and who were available to be reached by the reminder/release message. Column 1 is the noncompete sample, column 2 is the no noncompete sample, and column 3 is the full sample. The mean of the DV in column 3 is the no noncompete group mean.

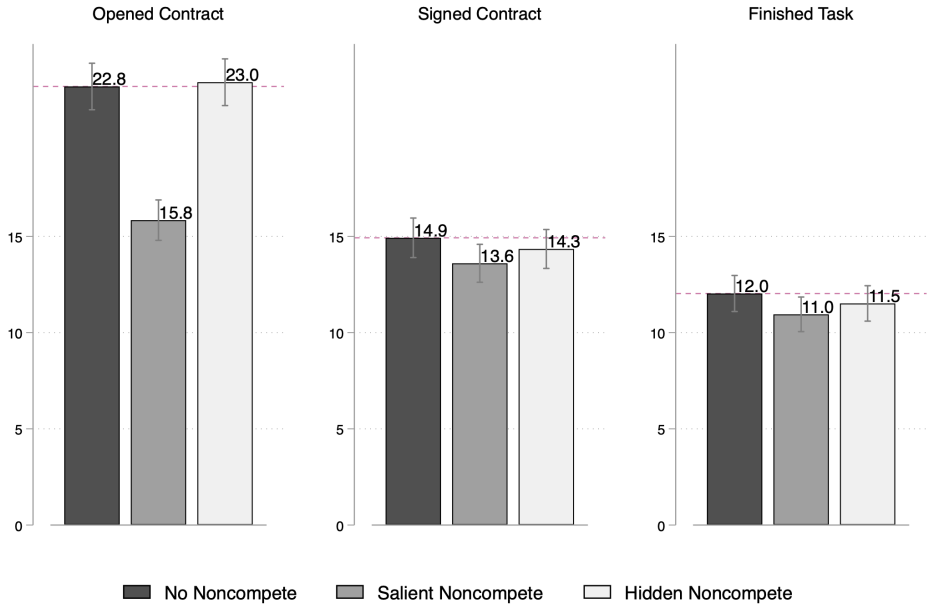
Table G4: **Phase 3 Release and Applications Hidden and Salient, With Heckman Correction**

	(1) Hidden	(2) Salient	(3) No Noncompete	(4) Full Sample
Released/Reminded	.0028	-.006	-.0037	.018
	(.022)	(.024)	(.025)	(.014)
Sent Job Ad	.26***	.27***	.36***	.38***
	(.027)	(.033)	(.034)	(.035)
Released/Reminded × Sent Job Ad	.047*	.091***	.017	.0064
	(.024)	(.024)	(.027)	(.028)
	(.00096)	(.00099)	(.001)	(.00061)
Hidden				.012
				(.018)
Salient				.042
				(.032)
Released/Reminded × Hidden				-.017
				(.012)
Released/Reminded × Salient				-.013
				(.013)
Sent Job Ad × Hidden				-.12***
				(.045)
Sent Job Ad × Salient				-.14***
				(.045)
Released × Sent Job Ad × Hidden				.051
				(.037)
Released × Sent Job Ad × Salient				.089**
				(.038)
Inverse Mills Ratio	.11	-3.5**	-2.9**	-.79
	(.58)	(1.7)	(1.1)	(.54)
Weighted	Y	Y	Y	Y
Control Mean	.22	.25	.3	.3
Observations	18,445	17,325	19,460	55,230
R^2	.3	.31	.31	.21

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. All models include strata fixed effects, fixed effects for the date of initial outreach, Firm A's offered wage in Phase 1, Firm B's offered wage in Phase 2, and day fixed effects. The dependent variable is an indicator for whether the individual applied for Firm B's job posting. The sample are only those who completed Firm A's task and who were available to be reached by the reminder/release message. Column 1 is the noncompete sample, column 2 is the no noncompete sample, and column 3 is the full sample. The mean of the DV in column 3 is the no noncompete group mean. The inverse mills ratio comes from the first-stage Heckman selection model using invite date as an instrument.

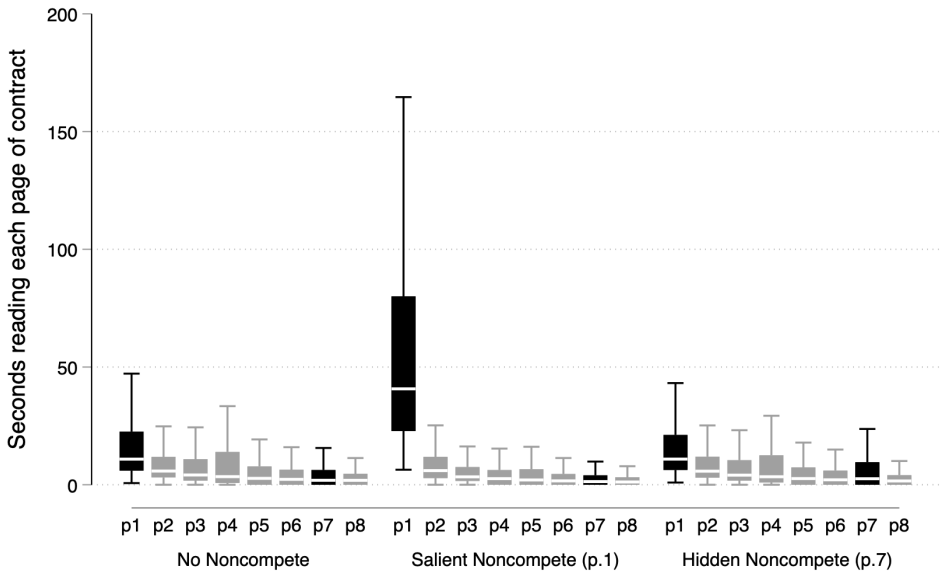
H Unweighted Results

Figure H1: **What unweighted percentage opened the contract, signed it, and completed the task?**



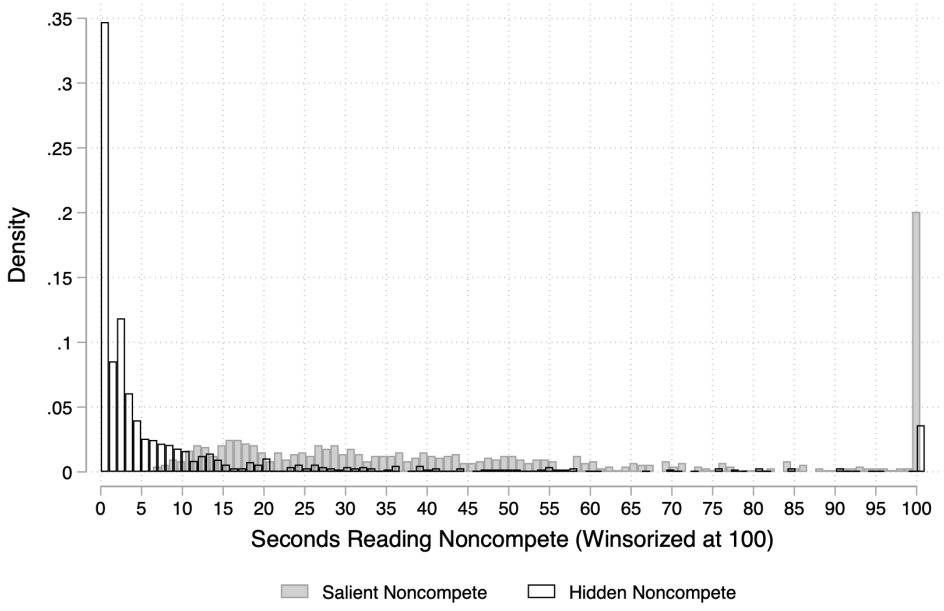
Notes: This figure shows the unweighted probabilities of opening the contract, signing the contract, and completing Firm A's task, according to the noncompete treatment condition.

Figure H2: **Unweighted Box and Whisker Plot of Time Spent Reading Each Page of Contract**



Notes: Page 1 of the Salient noncompete condition is the noncompete page. Page 7 of the Hidden noncompete condition is the noncompete page. Results are unweighted.

Figure H3: **Unweighted Histogram of Time Spent Reading Noncompete Page**



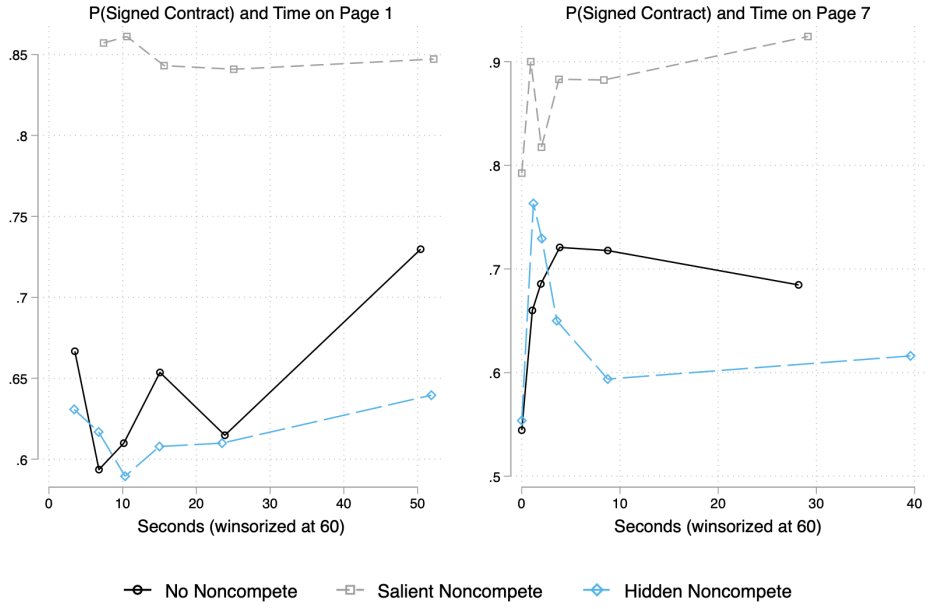
Notes: Results are unweighted. Reading variables are winsorized at 100 seconds.

Table H1: **Unweighted Opened Contract, Signed Contract, Completed Firm A task**

	(1) 𝟙(Opened Contract)	(2) 𝟙(Signed Contract)	(3) 𝟙(Completed Task)
Salient	-.07***	-.013*	-.011*
	(.0081)	(.0072)	(.0066)
Hidden	.0022	-.0058	-.0053
	(.0087)	(.0073)	(.0067)
Firm A Wage Offer	.002***	.0017***	.0015***
	(.00019)	(.00017)	(.00015)
Weighted	N	N	N
Control Mean	.22	.14	.12
Salient - Hidden	-.07***	-.008	-.006
	(.008)	(.007)	(.007)
Salient Wage Equivalent	35.13***	7.77*	7.23
	(5.3)	(4.3)	(4.4)
Hidden Wage Equivalent	-1.09	3.4	3.48
	(4.4)	(4.3)	(4.4)
Observations	13,896	13,896	13,896
R^2	.029	.021	.019

Notes: * p<0.1, ** p<0.05, *** p<0.01. All models include strata fixed effects and batch fixed effects. Standard errors reported in parentheses, clustered by individual.

Figure H4: **Unweighted Probability of Signing Contract Given Reading on Page 1 and 7**



Notes: Reading variables are winsorized at 60 seconds.

Table H2: **Unweighted Reading Page 7 and Completing Firm A Task**

	(1)	(2)	(3)	(4)
Dependent Variable:	𝟙(Completed Firm A Task)			
Sample:	Quartile of Seconds on p.7			
	1st	2nd	3rd	4th
Salient	.16***	.21***	.1**	.24***
	(.041)	(.05)	(.052)	(.069)
Hidden	-.034	.088*	-.12***	-.049
	(.038)	(.052)	(.046)	(.054)
Firm A Wage Offer	.0017*	.0025*	.0039***	-.0011
	(.00094)	(.0013)	(.0012)	(.0014)
Weighted	N	N	N	N
Control Mean	.44	.5	.59	.57
Min Seconds on p.7	0	.747	2.699	10.67
Max Seconds on p.7	0.745	2.698	10.666	3939.09
Observations	1,019	641	678	507
R^2	.15	.21	.24	.28

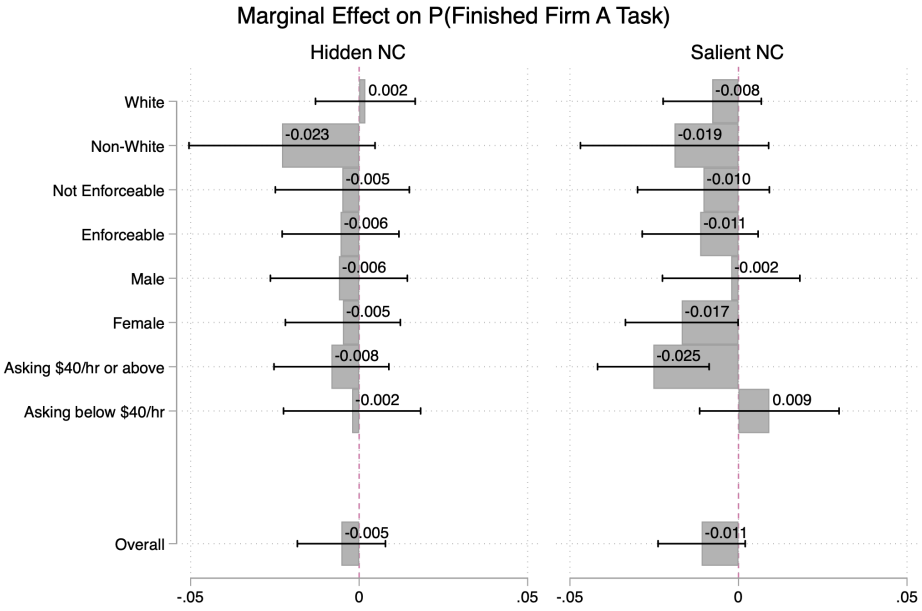
Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and fixed effects for the date of initial outreach. The dependent variable is an indicator for signing the contract. The four columns are limited to the quantiles of the reading time on page 7 of the hidden noncompete group. The range of reading times for each quantile is given by the Min and Max rows.

Table H3: **Unweighted Wage Bargaining and Reading Contract**

	(1)	(2)	(3)	(4)
Dependent Variable:		𝟙(Wage Bargaining)		
Sample:			Read p.7 for ___	
	Overall	Opened Contract	>0 seconds	>15 seconds
Salient	.0025	-.0087*	-.01	-.024
	(.0024)	(.005)	(.0065)	(.024)
Hidden	.0018	.0041	.0029	.032
	(.0023)	(.006)	(.0078)	(.022)
Firm A Wage Offer	-.00036***	-.00041***	-.0004**	-.00055
	(.000055)	(.00015)	(.00019)	(.0005)
Weighted	N	N	N	N
Control Mean	.01	.01	.02	0
Observations	13,896	2,855	1,877	369
R^2	.035	.081	.088	.35

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and fixed effects for the date of initial outreach. The dependent variable is an indicator for bargaining over wages. The four columns include the overall sample, those who opened the contract, and those who spent at least some time on page 7 or at least 15 seconds on page 7.

Figure H5: **Unweighted Heterogeneity in Noncompete-Completing Firm A Task**



Notes: This figure shows heterogeneity in the coefficients on hidden and salient noncompetes related to completing Firm A's task the contract. The sample is limited to each subsample, and then Equation (3) is estimated on that subsample, unweighted, and the coefficients and 95% confidence intervals on the hidden and salient noncompete variables are shown.

Table H4: **Unweighted Noncompetes and Firm A's Wage Bills**

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	𝟙(Completed Firm A Task)			Hourly wages	Total billed
Sample	Overall	Paid \$25/hr	Paid \$60/hr	Finished Firm A Task	
Salient	-.0042	-.0043	-.018*	-.81	-.8
	(.0084)	(.0084)	(.01)	(1.1)	(1.1)
Hidden	-.0041	-.0041	-.0065	.12	.79
	(.0084)	(.0083)	(.01)	(1.1)	(1.1)
𝟙(Offered \$60/hr)	.058***				
	(.0095)				
Salient$\times$ 𝟙(\$60/hr)	-.013				
	(.013)				
Hidden $\times$ 𝟙(\$60/hr)0	-.0023				
	(.013)				
Constant	.091***	.091***	.15***	47***	45***
	(.006)	(.006)	(.0074)	(.74)	(.77)
Weighted	N	N	N	N	N
Control Mean	.12	.09	.14	46.74	45.22
Observations	13,896	6,945	6,951	1,597	1,597
R^2	.019	.024	.019	.071	.071

Notes: Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and fixed effects for the date of initial outreach. The dependent variable in columns (1)-(3) is an indicator for completing Firm A's task. The sample in column (1) is all workers, and in column (2) and (3) it is split out into workers offered \$5/hr and \$60/hr, respectively. In columns (4) and (5), the sample is limited to those who completed Firm A's task. The dependent variable in column (4) is average hourly wages, and the dependent variable in column (5) is the total amount billed by each worker, which can be different from their hourly wage because of how much time they spent working on the task.

Table H5: **Unweighted Task Performance**

	(1)	(2)	(3)	(4)	(5)	(6)
	Number of Characters in Notes			Mistakes		
Offered Salient	-.23**			.018		
	(.1)			(.11)		
Offered Hidden	-.024			-.091		
	(.1)			(.11)		
Signed Salient		-.13	-.13		.11	.12
		(.089)	(.085)		(.099)	(.099)
Signed Hidden		.031	.0012		-.046	-.057
		(.088)	(.077)		(.097)	(.093)
Firm A Wage Offer	.018***	.0047**	.0038*	.011***	-.0016	-.0014
	(.0025)	(.0021)	(.002)	(.0027)	(.0023)	(.0022)
Sample	All	All	Paid by Firm A	All	All	Paid by Firm A
Weighted	N	N	N	N	N	N
Estimator	Poisson	IVPoisson	Poisson	Poisson	IVPoisson	Poisson
Control Mean	141.29	1148.11	1148.11	.46	3.73	3.73
Salient - Hidden	-.21**	-.16*	-.13*	.11	.15	.18*
	(.1)	(.09)	(.08)	(.11)	(.1)	(.09)
Salient Wage Equivalent	12.75**	26.79	34.94	-1.6	68.64	89.41
	(5.8)	(22)	(29)	(10)	(116)	(159)
Hidden Wage Equivalent	1.29	-6.49	-.33	8.12	-29.42	-40.93
	(5.5)	(19)	(20)	(9.9)	(75)	(90)
Observations	13,881	13,896	1,597	13,896	13,896	1,597

Notes: * p<0.1, ** p<0.05, *** p<0.01. All models include strata fixed effects and batch fixed effects. Standard errors reported in parentheses, clustered by individual. The dependent variable in columns (1)-(3) is a count of the number of characters in the notes that subjects used to explain their choices. The dependent variable in column (4)-(6) is a count of mistakes the subject made in submitting their documentation to Firm A. All models include strata fixed effects and fixed effects for the date of initial outreach, with the exception of the IV Poisson regression in column (2) and (5). Due to convergence issues, column (2) and (5) models include controls for gender, noncompete enforcement, asking rates, and race. To ensure the comparison group for noncompete signers is to those who completed Firm A's task in the control group, column (2) and column (5) also includes a control for the individual not completing Firm A's task.

Table H6: **Unweighted Joining Firm B**

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1(Accepted Firm B)									
	Before the Reminder					After the Reminder				
Offered Salient	.0063					-.015***				
	(.004)					(.0036)				
Offered Hidden	.0039					-.025***				
	(.004)					(.0034)				
Signed Salient		.062*	.037		.02		-.13***	-.12***		-.11***
		(.032)	(.029)		(.03)		(.03)	(.029)		(.03)
Signed Hidden		.038	.00021	-.0003	-.0013		-.21***	-.2***	-.13**	-.2***
		(.032)	(.029)	(.062)	(.029)		(.028)	(.027)	(.058)	(.026)
Firm B Wage Offer	.012***	.0063***	.005***	.0066***	.0049***	.0055***	.0036***	.0041***	.0066***	.004***
	(.00023)	(.00041)	(.00069)	(.0016)	(.00067)	(.00021)	(.00034)	(.00063)	(.0016)	(.00062)
Inverse Mills Ratio					.18					-.2
					(.18)					(.17)
Estimator	OLS	2SLS	OLS	OLS	Heckman	OLS	2SLS	OLS	OLS	Heckman
Sample	All	All	Paid by Firm A	Skippers	Paid by Firm A	All	All	Paid by Firm A	Skippers	Paid by Firm A
Weighted	N	N	N	N	N	N	N	N	N	N
Control Mean	.08	.64	.64	.65	.64	.05	.38	.38	.38	.38
KP RK Wald F		570.86					570.86			
Salient - Hidden	.002	.02	.04		.02	.01***	.09***	.09***		.09***
	(.004)	(.03)	(.03)		(.03)	(.003)	(.03)	(.03)		(.03)
Salient Wage Equivalent	-.53	-9.94*	-7.37		-4.13	2.78***	34.89***	28.4***		26.8***
	(.33)	(5.1)	(6)		(6.1)	(.66)	(9)	(8.2)		(8.4)
Hidden Wage Equivalent	-.32	-6.03	-.04	.05	.27	4.58***	58.76***	49.53***	19.6*	49.46***
	(.33)	(5.1)	(5.8)	(9.4)	(5.8)	(.63)	(9.7)	(10)	(10)	(10)
Observations	13,896	13,896	1,597	351	1,597	13,896	13,896	1,597	351	1,597
R^2	.54	.6	.12	.33	.09	.25	.28	.14	.35	.12

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and batch fixed effects, with the exception of columns (5) and (10), which do not include the batch fixed effects because they are highly collinear with the date of invitation instrument. The dependent variable is whether the individual accepted Firm B's job offer. Columns (1)-(5) look before the reminder. Columns (6)-(10) look after the reminder. Columns (1)-(2) and (5)-(6) consider the full sample. The remaining consider the sample of individuals who were paid by Firm A. Columns (4) and (9) consider the hidden or no-noncompete individuals who were paid by Firm A but who did not read page 7 of their contract. Columns (5) and (10) include the inverse mills ratio, which uses the date of the initial job offer as an instrument for completing Firm A's task.

Table H7: **Unweighted Sharing Firm A Resumes with Firm B**

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	𝟙(Shared Firm A Resumes with Firm B)									
	Before the Reminder					After the Reminder				
Offered Salient	.0011					.0005				
	(.0014)					(.00068)				
Offered Hidden	.0019					.00024				
	(.0015)					(.00065)				
Signed Salient		.0093	.0089		.0038		.0046	.0016		.002
		(.012)	(.012)		(.013)		(.006)	(.006)		(.0058)
Signed Hidden		.017	.02	.048	.015		.0022	.0021	-.0012	.0014
		(.012)	(.012)	(.031)	(.012)		(.0055)	(.0055)	(.011)	(.0054)
Firm B Wage Offer	.00082***	.0005***	.00097***	.0028***	.001***	.00014***	-9.7e-06	-.000024	-.000022	-.000047
	(.0001)	(.00012)	(.00029)	(.00087)	(.00028)	(.00004)	(.00005)	(.00014)	(.00021)	(.00014)
Inverse Mills Ratio					.11					.0088
					(.082)					(.04)
Estimator	OLS	2SLS	OLS	OLS	Heckman	OLS	2SLS	OLS	OLS	Heckman
Sample	All	All	Paid by Firm A	Skippers	Paid by Firm A	All	All	Paid by Firm A	Skippers	Paid by Firm A
Weighted	N	N	N	N	N	N	N	N	N	N
Control Mean	0	.03	.03	.03	.03	0	0	0	0	0
KP RK Wald F		570.86					570.86			
Salient - Hidden	-.0009	-.007	-.01		-.01	.0003	.002	-.0005		.0006
	(.002)	(.01)	(.01)		(.01)	(.0007)	(.006)	(.006)		(.007)
Salient Wage Equiv.	-1.3	-18.85	-9.19		-3.64	-3.55	472.56	66.81		42.98
	(1.7)	(26)	(13)		(12)	(4.8)	(2,423)	(448)		(144)
Hidden Wage Equiv.	-2.35	-33.7	-20.59	-17.28	-13.93	-1.73	224.16	87.74	-55.58	30.62
	(1.8)	(26)	(14)	(11)	(12)	(4.6)	(1,332)	(576)	(848)	(139)
Observations	13,896	13,896	1,597	351	1,597	13,896	13,896	1,597	351	1,597
R^2	.046	.042	.091	.35	.062	.014	.0087	.072	.27	.045

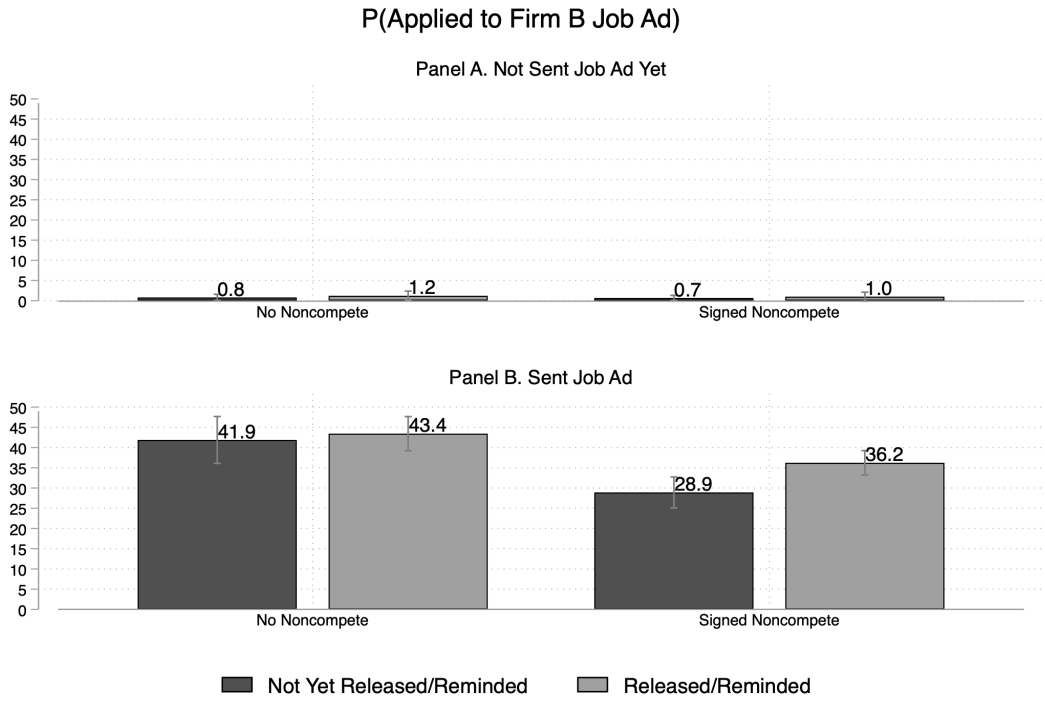
Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and batch fixed effects, with the exception of columns (5) and (10), which do not include the batch fixed effects because they are highly collinear with the date of invitation instrument. The dependent variable is whether the individual accepted Firm B's job offer. Columns (1)-(5) look before the reminder. Columns (6)-(10) look after the reminder. Columns (1)-(2) and (5)-(6) consider the full sample. The remaining consider the sample of individuals who were paid by Firm A. Columns (4) and (9) consider the hidden or no-noncompete individuals who were paid by Firm A but who did not read page 7 of their contract. Columns (5) and (10) include the inverse mills ratio, which uses the date of the initial job offer as an instrument for completing Firm A's task.

Table H8: **Unweighted Post-Reminder Sentiment**

	(1)	(2)	(3)	(4)
Sample:	Signed Firm A's Contract		Skipped p.7	
Dependent Variable: Sentiment is	𝟙(Negative)	𝟙(Positive)	𝟙(Negative)	𝟙(Positive)
Salient	.083***	-.15***		
	(.025)	(.031)		
Hidden	.07***	-.16***	.11**	-.27***
	(.024)	(.03)	(.045)	(.06)
Firm B Wage Offer	-.00021	.0012*	.00077	.0038**
	(.00057)	(.0007)	(.0014)	(.0017)
Weighted	N	N	N	N
Control Mean	.14	.46	.12	.53
Salient - Hidden	.01	.01		
	(.03)	(.03)		
Observations	1,597	1,597	351	351
R^2	.069	.1	.34	.37

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and fixed effects for the date of initial outreach and Firm A's offered wage. The dependent variable is an indicator for whether the sentiment of the response was positive or negative. The four columns include the sample who signed Firm A's resumes, and those who skipped page 6.

Figure H6: **Unweighted Applying to Firm B's Job Ad**



Notes: This figure shows the probability that the workers applied to Firm B's job ad in Phase 3. It cuts the data based on whether they worker had been invited to apply yet (top row vs. bottom row), whether they had a noncompete (left column vs. right column) and sent the reminder/release yet (left bar vs. right bar within each group).

Table H9: **Unweighted Phase 3 Release and Applications**

	(1)	(2)	(3)
Dependent Variable	𝟙(Applied for Firm B Job)		
Sample	Noncompete	No Noncompete	Full Sample
Released/Reminded	.0086	.025	.023*
	(.013)	(.02)	(.012)
Sent Job Ad	.29***	.39***	.39***
	(.02)	(.029)	(.029)
Released/Reminded × Sent Job Ad	.063***	.0053	.013
	(.016)	(.02)	(.021)
Noncompete			-.018*
			(.011)
Released/Reminded × Noncompete			-.0081
			(.0092)
Sent Job Ad × Noncompete			-.099***
			(.035)
Released × Sent Ad × Noncompete			.048*
			(.026)
Constant	-.014	-.063	-.025
	(.045)	(.063)	(.037)
Weighted	N	N	N
Control Mean	.25	.31	.31
Observations	35,735	19,390	55,125
R^2	.23	.31	.21

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. All models include strata fixed effects, fixed effects for the date of initial outreach, Firm A's offered wage in Phase 1, Firm B's offered wage in Phase 2, and day fixed effects. The dependent variable is an indicator for whether the individual applied for Firm B's job posting. The sample are only those who agreed to Firm A's contract and who were available to be reached by the reminder/release message. Column 1 is the noncompete sample, column 2 is the no noncompete sample, and column 3 is the full sample. The mean of the DV in column 3 is the no noncompete group mean.

Table H10: **Unweighted Noncompetes and Earnings**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable	Earnings From Firms A & B			Earnings From Firm A			Earnings From Firm B		
Offered Salient	-.14**			-.11*			-.34**		
	(.062)			(.061)			(.15)		
Offered Hidden	-.1*			-.029			-.63***		
	(.061)			(.06)			(.16)		
Signed Salient		-.037			.0017			-.28*	
		(.023)			(.011)			(.15)	
Signed Hidden		-.059***	-.069*		.016	-.015		-.63***	-.65**
		(.02)	(.039)		(.01)	(.017)		(.17)	(.28)
Firm A Wage Offer	.034***	.021***	.019***	.039***	.025***	.025***	.0082**	-.0041	-.022***
	(.0015)	(.00071)	(.0015)	(.0014)	(.00029)	(.00043)	(.0037)	(.0035)	(.0074)
Estimator	Poisson	IVPoisson	Poisson	Poisson	IVPoisson	Poisson	Poisson	IVPoisson	Poisson
Sample	All	All	Skippers	All	All	Skippers	All	All	Skippers
Weighted	N	N	N	N	N	N	N	N	N
Control Mean	6.48	53.45	53.76	5.45	45.22	44.77	1.04	8.23	13
Salient - Hidden	-.04	.02		-.08	-.01		.29*	.35*	
	(.06)	(.02)		(.06)	(.01)		(.18)	(.19)	
Observations	13,896	13,896	351	13,896	13,896	351	13,664	13,896	227

Notes: Standard errors in parentheses, clustered at the individual level. The benchmark is the no noncompete condition. All models include strata fixed effects and fixed effects for the date of initial outreach, with the exception of the IV Poisson models. Due to convergence issues, the IV Poisson models include controls for gender, noncompete enforcement, asking rates, and race. Those models also include a control for the individual not completing Firm A's task, to ensure the comparison group for noncompete signers is to those who completed Firm A's task in the control group.

Table H11: **Randomization Inference P-values**

Panel A: Phase 1 Outcomes

	(1)	(2)	(3)	(4)
Dependent Variable	𝟙(Opened Contract)	𝟙(Signed Contract)	𝟙(Completed Task)	𝟙(Bargained)
Salient	-.07***	-.013*	-.011*	.0025
	(.0081)	(.0072)	(.0066)	(.0024)
Hidden	.0022	-.0058	-.0053	.0018
	(.0087)	(.0073)	(.0067)	(.0023)
Salient RI P-Value	0	.04	.053	.226
Hidden RI P-value	.763	.357	.359	.367
Weighted	N	N	N	N
Observations	13,896	13,896	13,896	13,896

Panel B: Phase 2 Outcomes

	(1)	(2)	(3)	(4)
Dependent Variable	𝟙(Accepted Firm B)		𝟙(Shared Firm A Resumes)	
	Pre-Reminder	Post-Reminder	Pre-Reminder	Post-Reminder
Salient	.038	-.12***	.0091	.0016
	(.029)	(.029)	(.012)	(.006)
Hidden	.0011	-.2***	.02	.0021
	(.029)	(.027)	(.012)	(.0055)
Salient RI P-Value	.154	0	.445	.797
Hidden RI P-Value	.971	0	.063	.723
Weighted	N	N	N	N
Observations	1,597	1,597	1,597	1,597

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses, clustered at the individual level. Below each regression we report the randomization inference p-values from 1000 replications, following the 'ritest' Stata command described in Heß (2017). In Panel A and B we estimate equations (3) and (4), respectively.

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