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

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

Abstract

We study worker noncompete clauses in a large field experiment with two finance firms. Across ~14,000 job offers to freelance recruiters on short-term contracts, we randomize wages and the presence, salience, and duration of noncompetes (all contracts also included a nondisclosure agreement). Removing a noncompete increases mobility between competing employers by 36–52% and raises workers' total earnings from the two firms by 12–17%. We find no evidence—rejecting even small effects—that removing noncompetes generates secret leakage. We also find no evidence that workers choose noncompete jobs for higher pay. Many workers appear unaware of noncompetes before firms' post-employment communication. The results align with a model of inattention and uncertainty about 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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I Introduction

Noncompete clauses (“noncompetes”) have been debated since at least the 15th century (Blake, 1960). By prohibiting workers from joining competitors, they create a tension between freedom to contract and freedom to move. Two persistent claims are that noncompetes benefit workers because they can refuse them or demand more pay, and that they are needed to protect firm secrets and thus preserve incentives to innovate.

This paper assesses these claims theoretically and empirically through a large field experiment with HR recruiters on short-term contracts in the finance industry, where noncompetes are often the default. In phase one, we work with a company (“Firm A”) that previously used noncompetes, and make approximately 14,000 offers to contract HR recruiters to enter hourly recruiting engagements with Firm A. Recruiters’ initial task was to screen résumés for a Firm A vacancy, with pay capped at one billable hour.¹ The task gave recruiters access to competitively sensitive information about Firm A’s recruiting and compensation practices, including confidential materials about Firm A’s candidates.

We randomly removed Firm A’s noncompete entirely, made it more salient, or left it in the contract on page seven. Contracts in all conditions included a standard non-disclosure agreement. We also cross-randomized Firm A’s wage offer. In phase two, we work with a competitor (“Firm B”) to study workers’ willingness to work for a competitor and disclose Firm A’s private information. In phase three, Firm A randomly releases workers several months early from their noncompetes. This design allows us to identify both the causal effect of firms’ adopting or removing noncompetes and the effect of a worker signing one. As a condition of the experiment, subjects faced no actual legal risk from any decision.²

To guide the analysis, we develop a theoretical framework of workers’ decisions to sign noncompetes. The key is uncertainty: if reading is costly, workers may not know whether

¹The contract governed hourly engagements and remained open unless one side closed it. While Firm A did not provide additional assignments during the experimental period to preserve equal treatment, Firm A and the recruiters could work together after the experimental period, as is common following initial tasks.

²In particular, we required that the noncompetes and nondisclosure agreements never be enforced.

an offer contains a noncompete. Even when they notice one, they may be uncertain about enforcement. The model shows how this uncertainty suppresses compensating differentials and generates *ex post* harm despite voluntary contracting. Behavioral factors (e.g., present bias, base-rate neglect, [DellaVigna, 2009](#)) can amplify this harm.

To study these mechanisms, the experiment includes two manipulations of noticing and perceived enforcement risk:

1. Among workers randomly assigned a noncompete, some were shown their noncompete “saliently” (in the initial outreach, on the first page of the contract, with an extra signature) rather than “hidden” on the seventh page (the typical placement). We measure how long each subject spent on each contract page.
2. Although workers faced no actual legal repercussions, we study how Firm A’s exit-message *reminder* of workers’ post-employment obligations changed behavior.

Workers randomly offered salient noncompetes are 15% less likely to accept Firm A’s job than workers offered no noncompete, equivalent to a \$12/hr wage reduction. By contrast, we detect no average acceptance differential between hidden and no-noncompete offers, likely because workers overlook the clause: in the hidden condition, 50% of workers who opened the contract spent 2.6 seconds or less on the noncompete page.

We find no evidence that workers are more likely to agree to noncompetes when offered a higher wage, relative to the no-noncompete treatment. As such, workers who work for Firm A under a noncompete are not paid more for signing one. We can also rule out small effects on bargaining attempts. Because wages are randomized, this evidence speaks to one channel through which compensating differentials could arise (worker-side sorting). Outside of our experiment, compensating differentials could still arise through firms’ wage-setting, recruiting intensity, or other job attributes.

In Phase 2, we find that removing Firm A’s noncompetes increases mobility to Firm B (a labor market competitor), for both hidden and salient clauses (including in states where they are unenforceable) without increasing secret leakage. Firm A’s exit-message reminder appears central to the mobility effects. Before the reminder we detect no effects

of the noncompete on mobility (even when salient), while after the reminder mobility falls 36% for those with a salient noncompete and 52% for those with a hidden noncompete. These results suggest adverse selection among salient signers relative to hidden signers. Although removing noncompetes increased post-reminder mobility to Firm B, we can rule out even small effects on secret sharing.

In phase three, we randomly release workers early from their noncompetes at the end of the experiment. Workers released from their noncompete are 20% more likely to apply to a competitor's job opening than not-yet-released workers.

Lastly, we study how noncompetes affect worker earnings. Removing hidden noncompetes from a job offer increases earnings from Firms A and B by 12%, while removing salient noncompetes increases earnings by 17%. For *hidden* noncompete signers, the earnings losses are driven largely by turning down Firm B: signing a hidden noncompete reduces earnings by 6.1% on average from Firms A and B, and by 14% for workers who skipped the clause entirely. By contrast, workers offered a *salient* noncompete experience earnings losses mainly because they are less likely to take Firm A's initial job offer. Using data from a payroll platform, we find little evidence that workers offset these losses by substituting work effort to other firms.

This study makes three main contributions. First, we document causal effects of firms requiring and workers signing noncompetes. The direction and magnitude of our results are inconsistent with 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 otherwise seek competing work (e.g., workers near retirement may be more willing to sign a noncompete). To the contrary, workers who agree to a salient noncompete are more willing to do future work with a competitor (relative to a hidden noncompete). One interpretation is that the salient noncompetes disproportionately select for workers who believe they will not be caught.

Second, we propose an alternative theory consistent with these results: understanding

contracts and their likely enforcement is costly. Some workers therefore remain uninformed and accept contracts that expose them to risk. Even if average wage levels adjust to the presence of noncompetes and expected enforcement, workers whose noncompetes are enforced may not be sufficiently compensated *ex ante* and are therefore harmed *ex post*. This framework may apply to other employment terms such as NDAs, training repayment agreements, and non-solicitation agreements.

Third, we find no evidence that removing noncompetes (while keeping NDAs) increases secret sharing, and we can rule out small effects. These results are important because courts enforce noncompetes on the basis that they are necessary for protecting firm secrets, despite alternatives like NDAs. Yet, enforcing a noncompete prevents not only those secrets from being shared, but also all of the worker’s other skills, knowledge, and human capital. In our setting, removing noncompetes increases mobility and earnings without detectable effects on the spread of firm secrets.

In the remainder of this section, we review related literature. Section II develops a simple model of wages, inattention, and noncompete adoption. Section III presents our empirical setting. Sections IV through VII describe the experiment and results, and Section VIII concludes. The study was preregistered on the AEA RCT Registry; Appendix A compares the preregistration with the analyses reported in the paper.

I.A Related Literature

This paper connects broadly to research on employment contracting, monopsony, and knowledge diffusion. It also contributes to a growing empirical and theoretical literature on how restrictive covenants affect labor markets and firm behavior.

Contracting, Monopsony, and Earnings. Classic theories of contracting view noncompetes as mutually beneficial agreements: they protect firm investments and generate surplus to be shared with workers (Klein, Crawford, and Alchian, 1978; Rubin and Shedd,

1981). In this framework, workers freely choose whether to sign such clauses and demand compensating differentials for reduced mobility (Friedman, 1991; Posner, Triantis, and Triantis, 2004). Empirically, several papers find that workers with noncompetes earn more or receive greater investment from firms (Starr, Ganco, and Campbell, 2018; Lavetti, Simon, and White, 2020; Starr, Prescott, and Bishara, 2021; Rothstein and Starr, 2022; Jeffers, 2024).

A contrasting perspective emphasizes monopsony. Noncompetes can restrict job-to-job mobility, limiting competition and depressing wages below marginal products (Boal and Ransom, 1997; U.S. Council of Economic Advisers, 2016; Manning, 2021; Caldwell, Dube, and Naidu, 2024). By adding employer-specific search frictions (Burdett and Mortensen, 1998), such clauses may misallocate labor (Shi, 2023; Mueller, 2023). Natural experiments exploiting state-level changes in enforceability find that limiting noncompetes raises wages and mobility (Lipsitz and Starr, 2022; Balasubramanian et al., 2022; Johnson, Lavetti, and Lipsitz, 2023). Other empirical evidence finds that noncompetes shape worker behavior even when unenforceable (Starr, Prescott, and Bishara, 2020; Prescott and Starr, 2024).

Our work complements both literatures by formalizing a tractable theory for why workers may fail to be compensated *ex ante* for the costs imposed by noncompetes. Building on behavioral models and evidence on limited attention and costly information acquisition (Schwartz and Wilde, 1983; DellaVigna, 2009; Bakos, Marotta-Wurgler, and Trossen, 2014),³ we show that uncertainty about contract content or enforceability can suppress compensating differentials and generate *ex post* harm even under voluntary contracting.

Empirical Strategies. Two empirical approaches dominate prior work. One relies on surveys of workers and firms, correlating self-reported noncompete use with wages and investments (Lavetti, Simon, and White, 2020; Starr, Prescott, and Bishara, 2021; Rothstein and Starr, 2022; Balasubramanian, Starr, and Yamaguchi, 2024). These studies suggest posi-

³A related literature studies rational inattention and costly information processing (e.g., Sims, 2003; Matějka and McKay, 2012, 2015; Martin, 2017; Ravid, 2020; Cusumano, Fabbri, and Pieroth, 2024). We do not explicitly model rational inattention in the canonical entropy-cost framework, but our paper speaks to a related mechanism: costly attention to contract terms and uncertainty about their enforcement.

tive associations but face endogeneity concerns, as noncompetes are adopted nonrandomly and often bundled with other restrictions. The second strategy exploits state-level policy shocks to identify effects of enforceability ([Lipsitz and Starr, 2022](#); [Balasubramanian et al., 2022](#); [Johnson, Lavetti, and Lipsitz, 2023](#)). These studies capture regulatory impacts but not firms’ decisions to include noncompetes, nor workers’ responses when such clauses are formally unenforceable.

[McAdams \(2019\)](#) concludes that causal evidence on noncompetes themselves remains limited. We contribute a large-scale field experiment that estimates the effects of the firm requiring—and the worker signing—noncompetes on relevant outcomes (e.g., acceptance, compliance, earnings, secret sharing), in states that do and do not enforce them, while identifying key mechanisms (e.g., contract salience, enforcement cues).

Knowledge Diffusion and Innovation. Noncompetes have implications for knowledge flows broadly, even if their enforcement is intended to protect a narrow subset of that knowledge (e.g., firms’ trade secrets). A rich literature explores this theme, e.g., California’s ban on noncompetes and the rise of Silicon Valley ([Saxenian, 1996](#); [Gilson, 1999](#); [Hyde, 2003](#); [Fallick, Fleischman, and Rebitzer, 2006](#); [Lobel, 2013](#)). Other work finds that noncompete enforceability resulted in lower mobility, fewer startups, and slower idea diffusion ([Franco and Filson, 2006](#); [Marx, Strumsky, and Fleming, 2009](#); [Starr, Balasubramanian, and Sakakibara, 2018](#); [Johnson, Lipsitz, and Pei, 2023](#); [Reinmuth and Rockall, 2023](#)).

This literature does not measure secret sharing directly—even though secret protection is central to the case for noncompete enforcement. Instead, it proxies for knowledge flows using patent citations or spinout formation ([Alcacer and Gittelman, 2006](#); [Fadeev, 2023](#)). Our contribution is to measure secret leakage directly: in our design, workers can share identifiable firm materials, permitting direct observation of such leakage. We then compare noncompetes with NDAs, which protect secrets without restricting mobility. We find no measurable increase in secret sharing when noncompetes are removed, suggesting that

NDAs alone can prevent confidential spillovers.

Policy and Disclosure. Noncompetes have become a central focus of labor-market regulation. By 2024, roughly a dozen U.S. states had banned them for some or all workers, and the Federal Trade Commission had adopted a rule that would have imposed a national ban but was challenged and ultimately vacated. Similar debates are active in the United Kingdom, Canada, Italy, and Australia ([Young, 2021](#); [Boeri, Garnero, and Luisetto, 2023](#); [Ross, 2024](#); [Torgnes and Terjesen, 2024](#)).

Theoretical work offers mixed guidance. Some models favor bans on efficiency grounds ([Baslandze, 2022](#); [Shi, 2023](#)), while others highlight turnover costs or contractual externalities that may make partial restrictions preferable ([Aghion and Bolton, 1987](#); [Gottfries and Jarosch, 2023](#)). Recent proposals instead emphasize disclosure and salience requirements—ensuring that workers understand noncompetes at the time of contracting ([Meese, 2023](#)). Early-notice statutes in several U.S. states aim to achieve this.

Our experimental evidence speaks directly to these debates. Making noncompetes salient reduces acceptance and earnings, yet also selects workers more likely to disregard the restriction *ex post*. This tradeoff implies that greater transparency may alter selection rather than simply improving welfare.

II Conceptual Framework

We develop a wage-setting model with asymmetric information (based on [Myerson, 1981](#); [Riley and Zeckhauser, 1983](#); [Bulow and Roberts, 1989](#)) as a tool for interpreting the empirical results. In Period 1, a mass of workers decides whether to accept an employer’s offer under uncertainty about whether the contract contains a noncompete and, if so, whether the restriction will be effective at a later mobility opportunity. In Period 2, the worker faces that mobility opportunity: any previously hidden clause is revealed, and whether the restriction will be effective is clarified if the worker attempts to move. The

model highlights two frictions that matter for our empirics: workers may not know whether a contract contains a noncompete in Period 1, and they may be uncertain whether a signed noncompete will be enforced in Period 2. Proofs are in Appendix B.

II.A Setup

In Period 1, workers have outside option zero and privately known distaste for the Period 1 job, $d_i < 0$, drawn from a continuous, regular distribution with CDF F_d . If employed, worker i receives wage w . If a signed noncompete is enforced in Period 2, the worker incurs a utility loss $k > 0$, the net present value of foregone future wages from the blocked mobility opportunity. Here enforcement includes threatened or actual actions, as well as institutional conditions, that make the restriction effective. Workers may be uncertain about whether the Period 1 contract contains a noncompete (with subjective probability $\hat{p}_n$) and whether a signed noncompete will be enforced (with subjective probability $\hat{p}_e$). The first uncertainty captures limited attention to lengthy contracts; the second captures imperfect knowledge of monitoring, litigation, and legal enforceability.⁴ In the main results, $\hat{p}_n$ and $\hat{p}_e$ are common beliefs within a given informational environment; cross-worker heterogeneity enters through d_i .

Define the worker's total distaste at the time of acceptance as $\theta_i \equiv d_i - \hat{p}_n \hat{p}_e k$. If worker i accepts the job, her Period-1 expected utility is $\mathbb{E}[u_i] = w + \theta_i = w + d_i - \hat{p}_n \hat{p}_e k$. She accepts if and only if $\mathbb{E}[u_i] \geq 0$. After the Period-2 realization, the worker's realized utility is obtained by replacing the subjective product $\hat{p}_n \hat{p}_e$ with the realized product $p_n p_e$. Since $d_i < 0$, higher values of d_i correspond to less dislike of the job. The distribution F_Θ is a leftward shift of F_d by $\hat{p}_n \hat{p}_e k$, so the ex ante probability of accepting the offer is $1 - F_\Theta(-w)$.

Employer's Utility. Let $p_n, p_e \in \{0, 1\}$ denote whether the firm uses a noncompete and whether a signed noncompete is enforced, as distinct from workers' beliefs $\hat{p}_n, \hat{p}_e$. An

⁴For example, courts in California do not enforce noncompetes, but [Prescott and Starr \(2024\)](#) show that workers generally do not know this.

effective noncompete has $p_n = p_e = 1$ and yields private value $\eta > 0$ to the firm.⁵ Under full information, a clause with $p_n p_e = 0$ is payoff-equivalent to no noncompete in (1). The employer chooses w to maximize

$$u_f = \underbrace{(v + p_e p_n \eta - w)}_{\text{Employer surplus}} \cdot \underbrace{(1 - F_\Theta(-w))}_{\text{Labor supply}}. \quad (1)$$

Equation 1 captures the usual tradeoff: higher wages increase labor supply but reduce surplus per worker. We assume $\eta > \hat{p}_n \hat{p}_e k$ whenever $p_n = p_e = 1$.⁶

II.B Results

We consider three informational environments: full information, hidden clauses, and visible clauses with uncertain enforcement. Let $w_\emptyset$ denote the wage when the absence of a noncompete is known, and let w_{NC} (or w_H in the hidden-clause case) denote the wage offered by a firm with an effective noncompete, $p_n = p_e = 1$; the environments differ in workers' Period-1 beliefs about these offers.

Proposition 1 (Full Information). *If $p_n = p_e = 1$ and workers have full information ($\hat{p}_n = p_n, \hat{p}_e = p_e$), then (i) $w_{NC} > w_\emptyset$ and (ii) $u_i(w_{NC}, d_i) \geq 0$ for all workers who sign a noncompete.*

Proposition 1 is the full-information benchmark: an effective noncompete is priced through wages, and signers weakly prefer signing to the outside option.⁷

Undetected Noncompetes. Now suppose workers cannot detect clause presence at hiring. In this hidden-clause comparison, each offer induces the common belief $\hat{p}_n = \bar{p}_n$, and posted wages are non-informative about p_n .

⁵In our setting, η can be interpreted as the firm's value from preventing a former recruiter from helping a competitor identify, screen, or solicit candidates. This interpretation does not require confidential information to be disclosed at any point.

⁶Our framework treats the firm's noncompete choice as exogenous, but it implies simple adoption conditions, formalized in Corollary A1 in Appendix B.2. Intuitively, a detectable noncompete must be worth at least the perceived expected burden it imposes on labor supply, $\hat{p}_n \hat{p}_e k$.

⁷This standard contracting logic is emphasized by Rubin and Shedd (1981); Callahan (1985); Meese (2022).

Proposition 2. *If $\hat{p}_n = \bar{p}_n$ in the hidden-clause environment, then (i) $w_H > w_\emptyset$; (ii) w_H contains no additional term for the realized burden $p_n p_e k$ on the worker's own contract; and (iii) if $\bar{p}_n < 1$ and $\hat{p}_e > 0$, 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 $p_n = p_e = 1$.*

Thus $w_H > w_\emptyset$ should not be read as compensation for the realized clause. The wage reflects η and the common expected burden $\bar{p}_n \hat{p}_e k$; a worker whose clause is revealed and enforced bears the gap between that expectation and k , which can generate ex post regret. This motivates our empirical focus on contract reading and post-signing behavior.

Mandated Visibility. Now suppose the clause is observable, so $\hat{p}_n = p_n = 1$, but workers remain uncertain about enforcement. This case isolates the effect of visibility from the effect of enforcement beliefs.

Proposition 3. *If $\hat{p}_n = p_n = 1$, then (i) for any common $\hat{p}_e$, only workers with sufficiently high d_i accept w_{NC} , and the acceptance cutoff is lower when $\hat{p}_e$ is lower; and (ii) for any $\epsilon > 0$, there exist parameter values such that $0 < w_{NC} - w_\emptyset < \epsilon$.*

Proposition 3 shows that (i) for any common perceived enforcement risk, visible noncompetes are accepted only by workers with sufficiently favorable tastes for the job; lower perceived enforcement risk reduces the acceptance cutoff, making acceptance more likely; and (ii) this cutoff selection need not generate a meaningful compensating differential.

Proposition 4. *If $\hat{p}_n = p_n = 1$ and $\hat{p}_e \in (0, 1)$, then there exists d_i such that $\mathbb{E}[u_i(w_{NC}, d_i)] \geq 0$, but $u_i(w_{NC}, d_i) < 0$ if $p_e = 1$.*

Proposition 4 shows that visibility alone does not eliminate regret when enforcement is uncertain ex ante but may be realized ex post.

Non-Standard Beliefs and Preferences. Appendix B.2 extends the model to optimism about the base rate of noncompetes (Weinstein, 1980) and present bias in evaluating k (Laibson, 1997). These extensions strengthen, rather than overturn, the possibility that some workers sign contracts that make them worse off.

III Experiment Overview, Setting, and Subjects

In this section we present an overview of our experiment, and discuss our setting, subjects, and research partners (Firms A and B).

III.A Overview of Experimental Design

We worked with two investment management firms to operationalize our three-phase experiment. The subjects of the experiment are contract HR recruiters in the finance industry (i.e., headhunters, described next). Both Firms A and B had previously used headhunters and other contract HR specialists for hiring.

In Phase 1 of the experiment, Firm A makes randomized job offers to $\sim 14K$ contract HR recruiters to review résumés submitted for a vacancy at Firm A. The hourly wage was randomized jointly with the presence and salience of a noncompete. In Phase 2, a competitor (“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 Firm A’s résumés. In Phase 3, workers are randomly released early from Firm A’s noncompetes, and we track their applications to another job opening. See Figure F.1 for an overview of the timeline. The experiment was conducted in the summer and fall of 2023. We extensively planned for potential research ethics issues in this experiment (alone and in conversation with our IRB), and we discuss these below and in Appendix C.

What results should we *expect* from the experiment? As a benchmark, we ran a survey on [SocialSciencePrediction.org](https://socialscienceprediction.org), a platform operated by U.C. Berkeley for collecting “expert forecasts of the effects of [...] social programs.” Our survey described the experimental design and asked the expert panel to forecast our results (Table D.1 summarizes).

III.B Setting and Subjects

We highlight three characteristics of our setting and subjects that are important: the subjects are independent contractors, they work in HR, and they are in the finance sector. We discuss each of these characteristics below, and provide additional details about our specific setting (Firms A and B) in the following section.

Independent Contractor. While most published empirical research has focused on the use of noncompete clauses among employees, noncompetes are also common for independent contractors, unpaid interns, and temporarily employed workers.

The GAO report on noncompetes ([U.S. Government Accountability Office, 2023](#)) finds that among companies that hire part-time workers and use noncompetes (for anyone), 53.1% use noncompetes with *all* of their part-time workers. The GAO also reports that a company's primary justification for using noncompetes with part-time workers is to protect trade secrets. Lastly, the GAO report finds that 31% of firms that use noncompetes with part-time workers have enforced those noncompetes in the last five years.

For independent contractors specifically, noncompetes are often included in standard contract templates (see, e.g., [Internet Legal Research Group](#)). Legal advice websites offer guidance on using noncompetes with independent contractors and on when such clauses can be enforced (see, e.g., [LegalZoom.com](#)). A common justification from firms is that, absent a noncompete, contractors with no ongoing commitment to the firm will take valuable firm information and use it with a competitor. There are also many examples of real independent contractor or temporarily employed/volunteer noncompetes that are quite broad. Appendix E contains evidence that broad noncompetes with temporarily employed workers are common, including more than 50 examples (Table E.1).

Given their prevalence, advocacy groups for independent contractors have taken up the cause of limiting noncompetes. A literature in law journals specifically addresses part-time and contractor noncompetes. In response, policy makers have proposed and/or

adopted new laws specifically about noncompetes for independent contractors. Appendix E documents the advocacy and literature surrounding noncompetes for independent contractors, including newly proposed laws in New York City and Washington State.

Finance Setting. Noncompetes are common in the finance industry, covering approximately one in four workers (Starr, Prescott, and Bishara, 2021; Balasubramanian, Starr, and Yamaguchi, 2024). The finance industry has fought to protect the use of noncompetes: For example, a proposed 2023 law in New York state banning noncompetes failed reportedly because of opposition from the finance industry (Ferré-Sadurní, 2023).

Recruiters and HR Staff. 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, Prescott, and Bishara, 2021), above the national average of 18%. This includes recruiters and headhunters engaged on a short-term basis. Throughout the broader U.S. economy, the employment of contracted HR recruiters has grown markedly in the last few decades (Cowgill and Perkowski, 2024).

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 competitors’ workers. 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 employer (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 a company’s candidate flow (including rejected candidates) can be helpful for staffing competitors. Both Firms A and B used noncompete 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 likely to be well-informed about noncompetes relative to other workers. Because they are in HR, they are exposed to how noncompetes affect recruitment and retention. 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 is more likely to depend on working with multiple companies. Finally, contract recruiters work offsite and do not interact with each other, which mitigates concerns about SUTVA violations. Taken together, our evidence comes from a setting where workers are likely to be unusually attentive and informed about employment contracts. Although it might not be an upper bound in a literal sense, weaker deterrence effects may arise in less-informed populations.

III.C Firms A and B

Our two research partners are private holding companies that make investments on behalf of clients, who are mostly wealthy individuals. For both Firm A and Firm B, multiple clients' wealth came from the success of the same company, although the specific companies were different and in separate industries. Clients at each firm are mostly co-founders or early employees (or their heirs) of the same company. Having earned wealth through a business, the clients managed their wealth together. Some of the firms' investment offices were co-located within a larger suite of commonly owned business and philanthropic organizations, and these units sometimes shared resources.

Both firms' investment strategies included a combination of direct investments and asset allocation to other investors (i.e., a "fund-of-funds"). The investors' goals included a combination of preservation and growth. Both firms helped clients plan charitable donations and philanthropic operations on behalf of the clients (particularly Firm B). Firm B has also been an operating partner for some of the companies in which it invested, and Firm B's investors helped manage some balance sheet capital for these firms.

Both firms did business in the U.S. and abroad. Firm A was more active in Europe, and

Firm B was more active in Asia. Both firms often conducted business in Florida. Assets under management are between \$1B and \$10B for both firms. By contrast, Goldman Sachs reported \$3.29 trillion in assets under supervision as of June 30, 2025.⁸ Thus, Firms A and B are relatively small compared to the largest financial organizations, but can be responsive to their clients and still have a relatively large balance sheet.

Staff at Firms A and B generally have a typical finance career path. They often went to selective colleges, joined a large investment bank after college, and later departed for a hedge fund, private equity, or investment/holding company such as Firm A or B.

Our connection to Firms A and B. We met the leadership of Firm A through the authors' personal networks. The leadership was interested in understanding noncompetes, especially given the particular policy climate. Firm A agreed to help run this project using a vacancy, sharing résumés and consulting on the sampling frame to target their preferred type of recruiter. As described in Appendix Q, we planned for a Firm B from the outset to better study mobility and secret sharing. Later, an executive at Firm A received a job offer from Firm B, which was eventually accepted. We leveraged this departure to connect with Firm B, fulfilling our goal of finding a second company.

Our role in the experiment. At each firm, our research partners provided guidance about operationalizing the experiment, both at a high level and in execution details. The power calculations we performed suggested that Firm A would need more recruiters than usual to have an informative experiment (see Section III.D).⁹ Our research team helped provide the extra work needed to execute the study, e.g., by sending out the job offers, sending résumés, answering recruiters' questions, and compiling results. In addition, we funded the extra recruiting hours necessary for the experiment (in this sense, our experiment is

⁸<https://www.pionline.com/asset-management/pi-goldman-sachs-2025-q2-earnings/>

⁹Firm A itself had low turnover and would typically hire on 2-3 recruiters per year, who worked a total of 30-35 hours each. For the larger suite of business and philanthropic organizations under common ownership and co-located with Firm A, we estimate around 75–90 distinct external individuals (conservatively) were engaged in recruiting per year. Appendix F.1 contains additional detail.

similar to [Bloom et al. 2013](#), where the authors paid for consulting services for multiple firms in an experiment in management to give researchers more control).

To keep the experiment as close to the firms’ normal operations as possible, the recruiters were engaged using protocols guided by the firms, and were given a typical recruiting task (screening résumés) for an actual job posting. The recruiters’ candidate feedback was checked for coherence, and in some cases followup questions were asked. The leadership of Firm A was given aggregated feedback about the candidates reviewed by the recruiters in the experiment, with additional details about the recruiters’ assessments available. Firm B was given a list of eligible candidates sourced by the recruiters.

As we discuss in our research ethics appendix (Appendix [C](#)), the leadership at both firms were comfortable viewing the results at an aggregate level. Firm A agreed that it would not sue, identify, or pursue any recruiter for violating an NDA or noncompete, or any firm that hired them. Ultimately, we were never asked by either company to share which recruiters worked for both firms or who violated their NDA or noncompete. Moreover, based on our IRB approval, we understood that we would not be allowed to share identifying information about the subjects with them, even if they had asked.

III.D 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 [I](#), we present a set of descriptive statistics about these potential candidates based on their profiles. Their average charac-

teristics are similar to BLS category 13-1071: They are mostly female (66% in our sample, 75% in the [BLS, 2022](#)) and mostly white (72% in our sample, 76% in the BLS). HR workers in the BLS earn about \$32/hour. Our workers' average asking rate is higher (\$51/hour), although some recruiters were willing to work below their asking rate.

Stratification. We drew a stratified random sample of $\sim 14\text{K}$ recruiters from the population of 29K. The strata were defined by (i) whether the recruiter's state permitted enforcement of noncompete agreements,¹⁰ (ii) quartiles of the recruiter's advertised hourly rate, (iii) gender, and (iv) prior experience in finance and/or HR/recruiting.

Within each stratum, we randomly assigned recruiters across the six treatment cells (two wage levels $\times$ three noncompete conditions) and verified covariate balance prior to fielding. All subjects in the 29K sample were assigned treatment conditions, including those who were ultimately not invited.

We then drew a stratified random sample of $\sim 14,000$ recruiters to receive an offer from Firm A. These 14K are our main subjects, although we also use data about the broader population of 29K in some of our analysis, including an IV/2SLS analysis in which the randomized invitation is an instrument to being hired by Firm A. The sample size of $\sim 14\text{K}$ was based on power calculations for the first two phases, assuming job acceptance rates of 10-20%. Because the 14K customized job offers could not be deployed simultaneously, they were released in 478 randomized batches over about 30 days. Our specifications generally include batch and strata fixed effects to account for timing and sampling structure. See Appendix [F.2](#) for details on the randomization, stratification, and batching procedures.

Because the invited sample was stratified, it does not reflect the population. Accordingly, we use inverse probability weights based on the representation of the strata in the population. The balance table from the full population is in Table [J.1](#), and in Table [J.2](#) for the unweighted invited sample. Table [I](#) columns (3)-(6) provide descriptive statistics on the unweighted and weighted sample.

¹⁰We based this classification on the coding of state noncompete laws described [here](#).

Table I: **Summary Statistics for the Recruiter Population and Invited Samples**

	(1)	(2)	(3)	(4)	(5)	(6)
	Population		Invited, Unweighted		Invited, Weighted	
Variable	Mean	SD	Mean	SD	Mean	SD
Recruiting Experience	0.730	0.444	0.759	0.428	0.730	0.444
Finance Experience	0.466	0.499	0.580	0.494	0.466	0.499
Female	0.661	0.473	0.550	0.497	0.661	0.473
Hourly Rate (Advertised)	51.5	49.8	54.9	51.7	51.3	50.4
Noncompete Enforcement State	0.697	0.459	0.570	0.495	0.697	0.459
White	0.725	0.446	0.743	0.437	0.724	0.447
Observations	29,005		13,851		13,851	

Notes: Columns summarize the full recruiter population, the invited sample, and the inverse-probability-weighted invited sample. Hourly Rate is the advertised platform rate. Noncompete Enforcement State indicates that the recruiter’s state permitted noncompete enforcement.

IV Phase 1: Job Offers and Hiring

In the first phase of the experiment, we work with Firm A to send $\sim 14K$ 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 whether the noncompete increased Firm A’s wage bills and performance on the recruiters’ task.

IV.A Experimental Design

The experiment used a single online platform through which recruiters received the offer, reviewed the contract, submitted work, and logged hours. Each recruiter received an hourly recruiting contract whose randomized terms covered all work under the engagement. Recruiters who accepted could submit work and log hours through the platform, and the contract remained open unless one side closed it.

To ensure all recruiters were paid for at least one task, the initial engagement asked them to review eight résumés submitted to Firm A and identify promising candidates for an associate position. Pay for this task was capped at one hour at the offered hourly rate.

Recruiters could complete the assignment on their own timeline over roughly one to two weeks. Appendix [F.4](#) contains example outreach language.

The initial outreach stated the hourly rate and linked to an eight-page work agreement that workers had to sign before beginning work. In every condition, the agreement included NDA/confidentiality provisions; it also included a Florida choice-of-law and forum clause. Contract review and signing occurred online, allowing us to measure page-level reading times. In all conditions except for page one of the salient condition, workers could use a “go to the end and sign” button to skip directly to the signature page.

Although Firm A only promised the initial task, several pieces of evidence suggest that the engagement was not naturally understood as a closed, one-task relationship. Instead, many recruiters appeared to treat it as a possible gateway to ongoing follow-up work. This is common for independent contractors and freelancers, where initial projects are often seen as a gateway to repeat work, and the goal of an initial task is frequently to build a positive reputation and loyalty which might lead to recurring engagements (e.g., see discussions of independent contractor project lifecycles by [Laker et al. 2021](#); [Cicmil 2023](#); [Freelancer.com 2025](#)). Two sets of evidence suggest that this is true in both the context of our study and in our experiment.

1. Contract data from the platform we used show that ~14% of initial contracts lead to follow-on work with the same firm (excluding the experiment); among those that continue, cumulative follow-on duration averages 426 days (median 63 days). Appendix [G](#) reports additional platform-wide evidence on contract durations.
2. In our specific experiment, 97% of workers who completed Firm A’s task kept the contract open after the initial engagement and 26% explicitly asked about, or stated their readiness or interest in—without prompting—the next task.

Note that, after the initial engagement, Firm A was free to work with the recruiters more. However, it was our policy not to give them more work during the experimental period so that all subjects were treated equally.

Randomized components. Each job offer randomized two features. First, the hourly rate was randomized to either \$25/hr or \$60/hr, which corresponded to the 25th and 75th percentile of the asking rate distribution in our population.¹¹ Second, the three noncompete treatments were “hidden,” “salient,” and “no noncompete.”

- In the “**hidden noncompete**” condition, the clause appeared on page 7 and was not mentioned in the outreach email. This placement of the noncompete is the most common, and was previously used by Firm A.
- In the “**salient noncompete**” condition, the clause was disclosed in the outreach email, moved to page 1 of the agreement, and required a separate, additional signature acknowledging the noncompete before the worker could proceed.
- In the “**no noncompete**” condition, the contract matched the hidden condition except that the noncompete clause was removed.

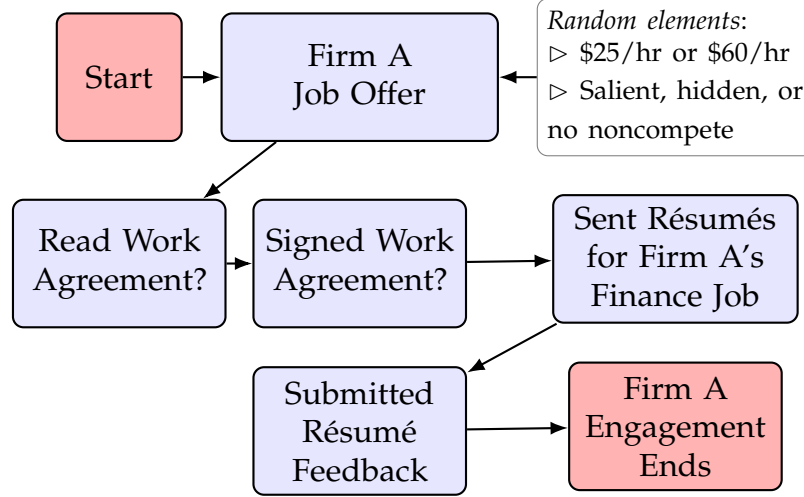
Figures F.2 and F.3 show page one of the salient noncompete and hidden/no noncompete treatments, while Figures F.4 and F.5 show page 7. In the two noncompete conditions, the clause reads:

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). The breadth of the noncompete is, however, comparable to many noncompetes for temporarily employed workers or independent contractors. Appendix E presents over 50 examples, including a three-year noncompete for a summer intern, a five-year noncompete for a tutoring independent contractor, and a one-year noncompete for a short-term recruiter.

¹¹By comparison, according to the BLS, the 25th percentile hourly wage for “human resource specialists” is \$24.76/hour, and the 75th percentile is \$44.42/hour.

Figure I: Phase 1 Experimental Design Summary



Outcomes. For all invited workers, we observe whether they opened the contract, time spent on each page, whether they bargained, whether they signed and completed the assignment, and their hours billed and total pay. Figure I summarizes Phase 1.

IV.B Empirical Specification

To estimate the causal effect of Firm A including a noncompete in a job offer on phase one outcomes, the estimating equation is

$$y_i = \beta_1 O_i^S + \beta_2 O_i^H + \beta_3 w_i^A + \gamma_{s(i)} + \psi_{t(i)} + \epsilon_i, \quad (2)$$

where O_i^S and O_i^H denote the randomized salient and hidden noncompete offers (both $\in \{0, 1\}$) so that “no noncompete” is the reference category. Firm A’s offered wage is w_i^A , $\gamma_{s(i)}$ are strata fixed effects, and $\psi_{t(i)}$ are batch fixed effects. We use inverse probability weights and [White-Huber](#) heteroskedasticity-consistent standard errors.¹² We also report the fully saturated 3×2 results and interaction tests to justify this restricted specification. Because

¹²This is appropriate because our regressions are cross-sectional: the unit of observation and randomization is the individual recruiter, with one observation per recruiter, so there is no within-individual dependence to account for by clustering. The heteroskedasticity-consistent variance estimator instead allows the regression errors to be heteroskedastic, as is natural for linear probability models and weighted specifications.

wages and noncompetes are cross-randomized, β_1 , β_2 , and β_3 identify the causal effects of offering a salient noncompete, a hidden noncompete, and a higher wage, respectively.

For acceptance outcomes, we also report “wage equivalents,” defined as $-\beta_1/\beta_3$ for salient noncompetes and $-\beta_2/\beta_3$ for hidden noncompetes. These ratios translate the estimated noncompete effects into dollar terms relative to the wage treatment. Our wage equivalence approach assumes that the effect of wages on acceptance is linear and that this linearity can be inferred from the two wage levels (\$25 and \$60 per hour). In Appendix H, we present a dichotomous way to interpret the wage treatments. We also report each wage equivalent as a percentage of the average offered wage (\$42.50 per hour).

IV.C Phase 1 Results

Working for Firm A. Table II presents the fully saturated results from the 3×2 design. Relative to job offers with no noncompetes and a wage offer of \$25 per hour, recruiters offered \$60 per hour and no noncompete or a hidden noncompete have a higher likelihood of opening the contract, signing the contract, and completing the task. Workers offered \$25/hr and a hidden noncompete experience no differences in contract opening, signing, or task completion, while those offered \$25/hr and a salient noncompete are less likely to open and sign the contract. Interestingly, we observe no differentials in contract opening between no noncompete at \$25/hr and a salient noncompete at \$60/hr.

Our preferred empirical model described in Section IV.B, which allows us to estimate wage equivalents, relies on a lack of interaction effects between noncompete use and wage effects. The bottom panel of Table II evaluates this restriction in the 3×2 specification. Across the three outcomes, the pairwise differential wage effects are small in magnitude, and the joint tests fail to reject the restrictions. Notably, while the 3×2 results suggest a deterrence effect of salient noncompetes, the lack of differential effects by size of the wage offer suggests that workers who open the contract despite the salient noncompete are a selected group that is not particularly wage sensitive on the extensive margin of opening.

Table II: **Phase 1 3 x 2 Treatment Effects on Contract Opening, Signing, and Task Completion**

	(1) $\mathbb{I}\left(\begin{smallmatrix} \text{Opened} \\ \text{Contract} \end{smallmatrix}\right)$	(2) $\mathbb{I}\left(\begin{smallmatrix} \text{Signed} \\ \text{Contract} \end{smallmatrix}\right)$	(3) $\mathbb{I}\left(\begin{smallmatrix} \text{Completed} \\ \text{Task} \end{smallmatrix}\right)$
Offered Hidden Noncompete, \$60/hr	.052*** (.015)	.038*** (.013)	.047*** (.012)
Offered Hidden Noncompete, \$25/hr	-.0051 (.015)	-.014 (.012)	-.0067 (.011)
Offered No Noncompete, \$60/hr	.074*** (.016)	.056*** (.014)	.062*** (.012)
Offered Salient Noncompete, \$60/hr	-.013 (.015)	.031** (.013)	.032*** (.011)
Offered Salient Noncompete, \$25/hr	-.071*** (.014)	-.021* (.012)	-.0072 (.011)
Baseline Mean (No Noncompete, \$25/hr)	0.20	0.13	0.09
Weighted	Y	Y	Y
<i>Differential Wage Effects (\$60-\$25):</i>			
Hidden vs. No Noncompete	-0.02 (0.02)	-0.004 (0.02)	-0.008 (0.02)
Salient vs. No Noncompete	-0.02 (0.02)	-0.004 (0.02)	-0.02 (0.02)
Salient vs. Hidden	0.00004 (0.02)	-0.0002 (0.02)	-0.01 (0.02)
Joint Test <i>p</i> -value	0.67	0.97	0.38
Observations	13,851	13,851	13,851
R^2	.026	.021	.02

Notes: Stars denote *p*-values: * *p*<0.10, ** *p*<0.05, *** *p*<0.01. The omitted cell is no noncompete with a 25 dollar per hour Firm A wage offer. Models include strata and batch fixed effects; heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Weighted columns use inverse-probability sampling weights. Indicator coefficients are percentage points. Joint test *p*-value tests whether the hidden-by-high-wage and salient-by-high-wage interactions are jointly zero.

Table III presents our preferred specification. Relative to the no-noncompete condition, the salient noncompete lowers contract opening by 34%, signing by 15%, and task completion by 15%. By contrast, hidden noncompetes do not affect opening rates—a placebo test, since nothing before page 7 differs from the no-noncompete contract—and have no detectable effect on task completion.

Table III: Phase 1 Effects on Contract Opening, Signing, and Task Completion

	(1) $\mathbb{1}\left(\begin{smallmatrix} \text{Opened} \\ \text{Contract} \end{smallmatrix}\right)$	(2) $\mathbb{1}\left(\begin{smallmatrix} \text{Signed} \\ \text{Contract} \end{smallmatrix}\right)$	(3) $\mathbb{1}\left(\begin{smallmatrix} \text{Completed} \\ \text{Task} \end{smallmatrix}\right)$
Offered Salient Noncompete	-.079*** (.01)	-.023** (.0092)	-.018** (.0083)
Offered Hidden Noncompete	-.014 (.011)	-.016* (.0092)	-.011 (.0084)
Firm A Wage Offer	.0018*** (.00024)	.0015*** (.00021)	.0015*** (.00019)
Weighted No-Noncompete Mean	Y 0.23	Y 0.15	Y 0.12
Salient - Hidden	-0.07*** (0.01)	-0.007 (0.009)	-0.008 (0.008)
Salient Wage Equivalent	43.95*** (8.3)	15.14** (6.4)	12.38** (5.7)
Hidden Wage Equivalent	7.52 (6.1)	10.57* (6.2)	7.13 (5.7)
Observations	13,851	13,851	13,851
R^2	.026	.021	.02

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The omitted group is no noncompete. Models include strata and batch fixed effects; heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. No-Noncompete Mean is the omitted-group mean. Weighted uses inverse-probability sampling weights. Wage offers and wage equivalents are dollars per hour; indicator coefficients are percentage points.

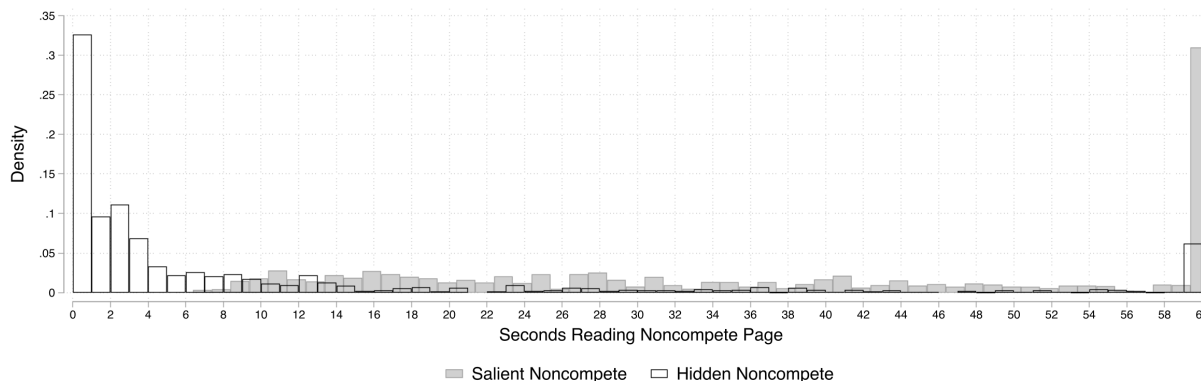
The wage equivalent of the salient noncompete is \$12.38 per hour, or 29% of the average offered wage. The corresponding estimate for the hidden noncompete is \$7.13 per hour (16%), though we cannot reject zero at canonical levels of statistical significance.

Expert Forecasts. Appendix Table D.1 shows that experts overestimated the deterrent effect of salient noncompetes, but correctly predicted hidden-noncompete signing rates conditional on knowing the signing rate in the no-noncompete baseline.

Contract Reading. Our theory suggests that noncompete detection matters. Consistent with this, most workers in the hidden condition appear not to notice the noncompete, and those who do are less likely to sign. Figure II shows the distribution of time spent on the

noncompete page. In the hidden and no-noncompete conditions, the median worker spent about 2 seconds on page 7, regardless of whether a noncompete appeared, and 33% of workers skipped it entirely. By contrast, in the salient condition, where the noncompete is on page 1, no recruiter spent less than 6 seconds on the noncompete page.

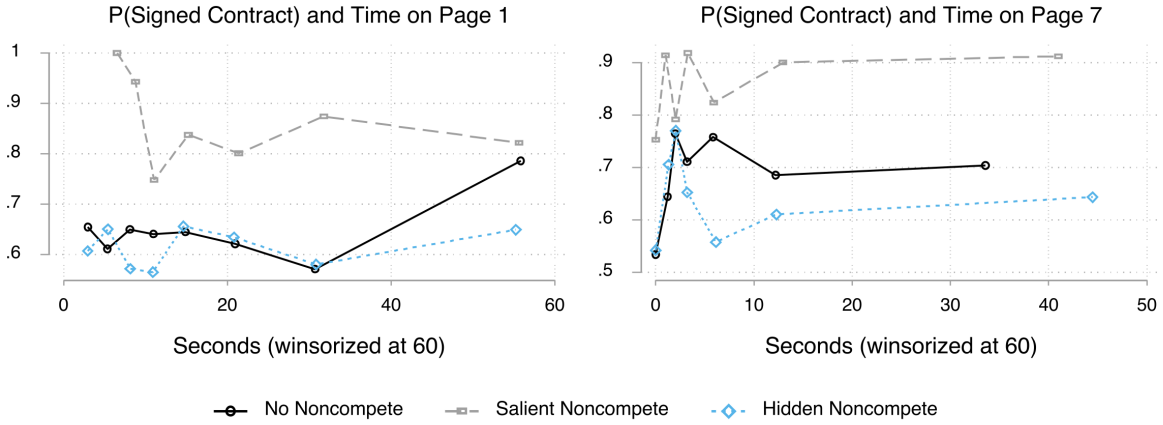
Figure II: Histogram of Time Spent Reading Noncompete Page



Notes: Weighted sample includes workers who opened the contract. Reading time winsorized at 60 seconds.

Workers who spent more time on the hidden noncompete page were less likely to sign the contract and, more noisily, less likely to complete Firm A's task. Figure III shows that, among recruiters who spent at least 5 seconds on page 7, those offered a hidden noncompete were about 10 percentage points less likely to sign the contract than those offered no noncompete. Tables J.6 and J.7 report the corresponding reading regressions. These patterns suggest that some workers who discovered the hidden noncompete were deterred from working for Firm A, but many did not notice it. By contrast, conditional on opening the contract, workers offered a salient noncompete were relatively likely to sign, consistent with learning about the restriction before opening the contract.

Figure III: Probability of Signing Contract Given Reading on Pages 1 and 7



Notes: Weighted sample includes workers who opened the contract. Reading time winsorized at 60 seconds.

Heterogeneity and Composition. In Appendix M, we study heterogeneity in completing Firm A's task. Hidden noncompetes do not statistically significantly reduce completion in any subsample, except among heavy readers (Table J.8). By contrast, salient noncompetes deter women more than men and deter recruiters with high asking rates more than those with low asking rates. We do not detect differential salient effects by home-state noncompete enforceability. The heterogeneous acceptance differentials align with differences in the composition of workers who completed Firm A's task (Tables J.3 and J.4). Notably, among workers who completed Firm A's task, workers with salient noncompetes are less likely to have a high asking rate—a signal of lower quality.

Bargaining. About 1% of the invited subsample attempted to bargain over wages, although all offers were take-it-or-leave-it. Table J.5 estimates bargaining responses using Equation (2). Workers bargain more when randomly offered lower wages, but we find no aggregate evidence that salient or hidden noncompetes increase bargaining.¹³ Columns (2)–(4) suggest less bargaining in the salient condition among workers who opened the contract, and more bargaining among long readers in the hidden condition. However, the

¹³This is consistent with prior evidence that workers rarely negotiate over noncompetes (Starr, Prescott, and Bishara, 2021; Rothstein and Starr, 2022).

latter group is small, so does not contribute much to the aggregate estimate.

Job Performance. In Appendix L, we examine how requiring a noncompete affects performance on Firm A's task. Although performance is outside our formal framework, noncompetes could affect both selection into the job and performance conditional on accepting. Our measures are limited and the estimates are generally imprecise, but they suggest that salient noncompetes reduce performance and that at least part of this difference reflects treatment rather than selection.

IV.D Firm A's Wage Bills and Compensating Differentials

When Firm A cross-randomizes wages and noncompetes, does Firm A pay higher wages to those who sign the noncompete? Our conceptual framework addresses this question. Proposition 1 showed that wages could rise if workers had full information, while Proposition 2 and Proposition 3 suggested that uncertainty over the presence of the noncompete or its enforceability could mitigate any wage increases.

In Table IV we examine these wage premiums. We find no evidence in columns (1)-(3) that the noncompetes reduce Firm A task completion rates differentially when workers are given a high wage (\$60/hour) versus a low wage (\$25/hour). 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 who sign noncompetes are not paid more in this experiment.

Compensating Differentials. The fact that workers are paid similarly by Firm A regardless of whether they were offered a noncompete does not necessarily imply that Firm A does not pay a compensating differential when it offers a noncompete. This is because a) Firm A could in reality endogenously change wages when it offers a noncompete, and b) there may be other non-wage costs to requiring a noncompete. We discuss these in turn.

First, per the randomization protocol, Firm A cannot endogenously change wages

Table IV: Phase 1 Effects on Firm A Task Completion, Hourly Wages, and Billing

Dependent Variable	(1) 1(Completed Firm A Task)	(2) Offered \$25/hr	(3) Offered \$60/hr	(4) Hourly wages	(5) Total billed
Sample	Overall			Finished Firm A Task	
Offered Salient Noncompete	-.0072 (.011)	-.0073 (.011)	-.029** (.013)	-1.2 (1.3)	-.66 (1.4)
Offered Hidden Noncompete	-.0067 (.011)	-.0068 (.011)	-.014 (.013)	-.31 (1.3)	.44 (1.3)
High Wage (Firm A)=1	.062*** (.012)				
Salient × High Wage	-.022 (.016)				
Hidden × High Wage	-.0076 (.017)				
Constant	.093*** (.0077)	.093*** (.0076)	.15*** (.0095)	47*** (.89)	45*** (.89)
Weighted	Y	Y	Y	Y	Y
No-Noncompete Mean	0.12	0.09	0.15	46.87	45.17
Observations	13,851	6,927	6,924	1,646	1,646
R ²	.02	.025	.022	.074	.078

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. The omitted group is no noncompete. Columns 1-3 estimate Firm A task completion overall and by wage offer; columns 4-5 condition on completing Firm A's task. Models include strata and outreach-date fixed effects; heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Weighted uses inverse-probability sampling weights. High Wage is a 60 dollar per hour Firm A offer; indicator coefficients are percentage points.

when it includes a noncompete. Thus, the prior analysis should be understood to show that workers are not more likely to sort toward (random) higher wages when offered a noncompete than without the noncompete. If instead Firm A had been allowed to endogenously change wages, and we require an all-else-equal condition in which Firm A is required to have the same task completion rate as in the no noncompete condition over the same time period, then the results from Table III indicate that Firm A would have to increase compensation by \$12.38 per hour in the salient condition and \$7.13 per hour in the hidden condition. This difference in wages would compensate for the extent to which the noncompetes reduce willingness to complete the task.¹⁴ Alternatively, instead

¹⁴The size of the wage equivalent depends on expected future hours under the open-ended hourly contract.

of raising wages to have the same completion rate as in the no noncompete condition over the same time frame, Firm A could have simply kept wages lower but waited longer to fill the position. Thus the extent to which Firm A might endogenously offer higher wages when it also offers a noncompete depends on the extent to which it wants to match the task completion rate in the no noncompete condition and the cost of waiting to hire.

Second, by offering a noncompete, Firm A may have to “pay” in other ways that are not reflected in direct wage payments. For example, the evidence above suggests that, relative to offering a hidden noncompete, offering a salient noncompete tends to select workers with lower asking rates and results in lower task performance. In the following section, we also find that those who signed a salient noncompete are more likely to break their noncompete than those who signed a hidden noncompete (though both are less likely to join a competitor than a worker with no noncompete). These analyses suggest that there are further costs to the firm of offering a salient noncompete.

V 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 workers who signed the contract with Firm A and who were sent Firm A’s materials (i.e., résumés to review). Firm A was aware of this plan.¹⁵ We are interested in how noncompetes affect workers’ willingness to take a job with a competitor, both before and after a reminder from Firm A, and whether noncompetes dissuade workers from leaking Firm A’s confidential information in violation of their NDA.

¹⁵Firm A did not enforce the noncompetes in this experiment, and we did not share which workers accepted Firm B’s offer. Firm A did send a reminder that is customary for companies using noncompetes, and some of Firm A’s workers chose to share about their experience with Firm B on their profiles.

V.A Experimental Design

As noted above, 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 F.5 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 years of experience. All Firm B offers were sent around the same time. On average, there were 26 days between the completion of the Firm A task and the receipt of the Firm B offer (median 27 days, range 15–39 days).

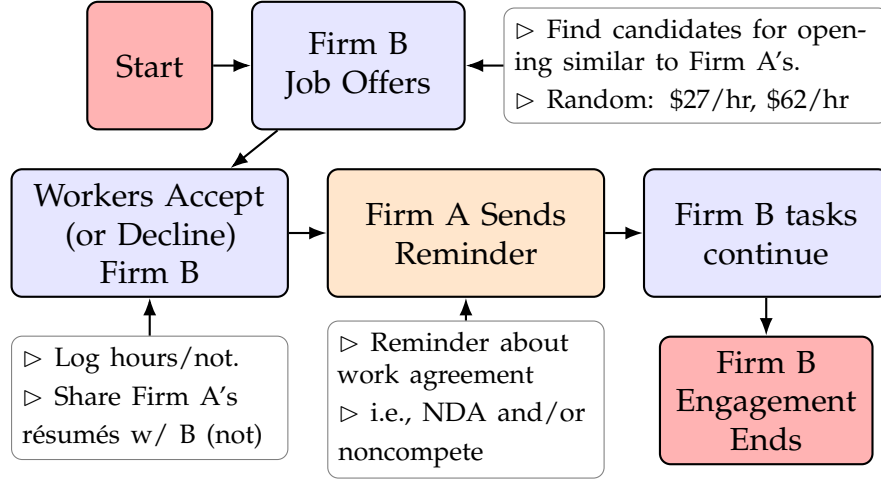
All recruiters hired by Firm A could easily fulfill Firm B's task by finding new résumés or sharing the résumés provided by Firm A.¹⁶ Phase 2 also includes a randomized wage treatment. Hourly wages for Firm B's offer were randomized at \$27 or \$62 per hour. These rates are approximately the 25th and 75th percentiles, but are slightly more than Phase 1.

Figure IV visualizes Phase 2, which consists of two sub-phases, separated by a reminder from Firm A about workers' contractual obligations. These reminders are often used by employers in exit interviews, or post-employment letters or emails. The reminder language is in Appendix F.6. In Phase 2A, workers are initially approached by Firm B, and can choose to work for Firm B or not. In Phase 2B (following the reminder by Firm A), the workers are given more opportunities to source leads for Firm B's job opening.

Phase 2B began when Firm A's reminders were sent. Firm A's reminders were sent approximately four weeks after Firm B's initial offers were distributed (~27 days). Following Firm A's reminders, Firm B sent a brief message to all workers reaffirming its continued interest in working together. This message aimed to keep recruitment consistent across all subjects in Phase 2B, irrespective of whether the subject had initially accepted,

¹⁶Recruiters were sent Firm A's résumés via a protected link to a ZIP file that required downloading to open. Had the résumés been deleted from the recruiter's device, the link still worked.

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



Notes: Only workers who signed the contract with Firm A were given offers from Firm B.

declined, or not responded to Firm B's initial offer. This message was sent from Firm B two days after Firm A's reminder, or 29 days after Firm B's initial offer.

V.B Variables

For Phase 2, let y_i denote either $\mathbb{1}\{\text{Joined Firm B}_i\}$ or $\mathbb{1}\{\text{Shared Firm A's Résumés with Firm B}_i\}$.

We study each outcome both before and after Firm A's reminder. Across our specifications, we vary our approach to workers who never become eligible to work for Firm B. For some of our estimation strategies, outcomes are defined as $y_i = 0$ for these workers. For the others, we treat y_i as selectively observed (only for workers who worked for Firm A).

Let $I_i = 1$ for the 14K recruiters randomly invited to take Firm A's job, with $I_i = 0$ for the uninvited. Let $O_i^S \in \{0, 1\}$ and $O_i^H \in \{0, 1\}$ denote the randomly assigned salient and hidden noncompete offers made by Firm A (no-noncompete is the omitted category). Only those invited receive noncompete terms such that $O_i^S = O_i^H = 0$ if $I_i = 0$. Finally, let $P_i = \mathbb{1}\{\text{Joined Firm A}_i\}$, and let $S_i = P_i O_i^S$ and $H_i = P_i O_i^H$ denote realized salient and hidden noncompete status, and $R_i^A = P_i w_i^A$ denote the realized wage for recruiters who completed a Firm A's task. Throughout, we control for randomization-stratum fixed effects and, when feasible, batch fixed effects, along with Firm A's wage offers. Appendix K.1

gives the full notation, timing conventions, and sample definitions.

V.C Empirical Specifications

In this section we describe four complementary empirical specifications we use to estimate the causal effect of Firm A requiring a noncompete and the effect of workers signing one. All specifications use the sample weights except where noted.

Effect of Requiring a Noncompete. Using all $\sim 14\text{K}$ invited workers, we estimate:

$$y_i = \beta_1 O_i^S + \beta_2 O_i^H + \beta_3 w_i^A + \gamma_{s(i)} + \psi_{t(i)} + \epsilon_i, \quad \text{if } I_i = 1. \quad (3)$$

This is the Phase 2 analogue of Phase 1’s Equation 2: β_1 and β_2 identify the causal effect of Firm A requiring a salient or hidden noncompete, relative to no noncompete. The coefficients reflect both treatment effects of signing a noncompete and selection into one.

Effect of Signing a Noncompete. Conceptually, we would like to estimate how signing a noncompete with Firm A affects engagements with Firm B relative to working with Firm A at the same wage without a noncompete. Estimating this treatment effect is challenging because it is not random whether a recruiter works with Firm A, at which wage, either under a noncompete or not. To address these issues, we present three approaches.

Our first approach estimates an IV/2SLS model using Firm A’s randomized offer design. Invitation is random in the 29K-worker experiment, and invitees are further randomized to noncompete and wage terms. These assignments generate exogenous variation in recruiters’ choice sets, which we use to instrument for working at Firm A and for doing so with or without a noncompete at a high or low wage. This is a randomized choice-set design as seen elsewhere in the literature.¹⁷ Since non-invitees cannot work at Firm A, noncompliance is one-sided; the invitation instrument therefore requires the full 29K

¹⁷In such designs, the random assignment changes the set of options available to a participant, and participants then choose from the options they are offered (e.g., [Abdulkadiroğlu et al., 2017](#); [Berg et al., 2025](#))

experimental population. The population-level, linear IV equation we estimate is:

$$y_i = \beta_1 P_i + \beta_2 S_i + \beta_3 H_i + \beta_4 R_i^A + \gamma_{s(i)} + \epsilon_i, \quad \text{for } I_i \in \{0, 1\}. \quad (4)$$

For workers not randomly invited to receive a Firm A offer ($I_i = 0$), we code $P_i = S_i = H_i = w_i^A = R_i^A = 0$. We treat (P_i, S_i, H_i, R_i^A) as endogenous and instrument them with the randomized vector $(I_i, O_i^S, O_i^H, w_i^A)$. We do not use weights since estimation is on the full experimental population. Batch fixed effects are omitted because they are collinear with I_i . The excluded instruments are strong in practice, with Kleibergen–Paap rk Wald F -statistics above 180 in the IV specifications reported below. As with our Phase 1 regressions (2), Equation 4 imposes a linearity assumption. Appendix H shows a dichotomous version.

The exclusion restrictions are satisfied in this setting because recruiters could only become eligible for Firm B’s Phase 2 outreach by first working for Firm A. Consequently, the randomized Firm A offer terms can affect Phase 2 outcomes only through the Firm A participation, noncompete, and compensation channels explicitly modeled in Equation (4).

IV Interpretation. By instrumenting for the realized Firm A wage R_i^A using the randomly offered wage, the IV model in Equation 4 asks whether noncompetes affect mobility and secret sharing *net of realized compensation from Firm A*. This is an important quantity because a central defense of noncompetes is that workers voluntarily accept them in exchange for compensation or opportunity. This specification tests whether the noncompete itself affects subsequent behavior—separately from realized Firm A compensation. Thus, in Equation 4, β_2 and β_3 capture the incremental effects of signing salient and hidden noncompetes, respectively, holding fixed the realized Firm A wage, while β_4 captures the effect of the realized Firm A wage on later outcomes. A version of this model that does not hold Firm A’s wage fixed yields nearly identical results (Appendix Table J.10).¹⁸

¹⁸By omitting Firm A wage variables from both stages, the results in Appendix Table J.10 estimate the effect of signing a noncompete — *inclusive* of wage-related consequences of realized Firm A work, rather than partialing them out. This specification answers the question of whether signing a noncompete contract

Heckman and Skipper specifications. We report two additional specifications. We estimate a two-step Heckman (1979) selection model that treats Phase 2 outcomes as observed only for workers who worked for Firm A and uses randomly assigned invite-date indicators as excluded instruments (see the variation in Figure J.7). Our final approach examines “skippers” in the hidden and no-noncompete conditions: workers who signed Firm A’s contract without reading page 7 at all. For post-reminder outcomes, this latter approach identifies a reminder-specific treatment effect of learning one had signed a hidden noncompete; for pre-reminder outcomes, it provides a placebo test. Appendix K.2 presents both specifications in full, along with the IV identifying assumptions. In these specifications we also include Firm B’s randomized wage offer to calculate wage equivalents.¹⁹

V.D Phase 2 Results: Working for Firm B

Table V examines mobility to Firm B. It shows a consistent pattern: before the reminder, we find no evidence that noncompetes reduce mobility to Firm B; after the reminder, workers who were offered and/or signed noncompetes are less likely to accept Firm B’s job.

Columns 1 and 5 estimate the effect of Firm A requiring a noncompete, which combines treatment effects of signing and selection into the contract. The remaining specifications instead isolate the effect of signing, and they point to the same pattern as the reduced-form estimates: before the reminder, we find no evidence of lower mobility to Firm B; after the reminder, signing a noncompete reduces mobility. We also find no meaningful differences by enforceability in the recruiter’s home jurisdiction (Appendix M).

Across specifications, we find larger effects for hidden than for salient noncompetes. For example, requiring a salient noncompete reduces working for Firm B by 36%, relative to 52% in the hidden condition. Why are signers of the salient noncompete actually *more mobile* than signers of the hidden noncompete? This pattern is consistent with Proposition 3:

reduces subsequent mobility and secret sharing under the compensation environment workers selected.

¹⁹Note that this does not create a bad-control problem because both specifications condition on joining Firm A, and Firm B’s offer is random conditional on joining Firm A.

when noncompetes are easily detectable, the workers willing to sign are those who do not believe they will be enforced.

Table V: Firm A Noncompetes and Joining Firm B Before and After Reminder

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	$\mathbb{1}(\text{Accepted Firm B})$							
	Before the Reminder				After the Reminder			
Offered Salient Noncompete	-.0052 (.0072)				-.018*** (.005)			
Offered Hidden Noncompete	-.0014 (.0073)				-.026*** (.0048)			
Firm A Wage Offer	.0011*** (.00017)				.00044*** (.00011)			
Signed Salient		.061* (.032)	.02 (.029)			-.12*** (.03)	-.11*** (.027)	
Signed Hidden		.047 (.033)	-.0025 (.028)	.085 (.071)		-.2*** (.028)	-.19*** (.026)	-.12** (.062)
Firm A Realized Wage		-.00037 (.00081)	.0014 (.0014)	-.0004 (.0021)		-.00052 (.00072)	-.0011 (.0013)	-.0013 (.0018)
Firm B Wage Offer			.0048*** (.00065)	.0047** (.0019)			.0038*** (.00061)	.0062*** (.0016)
Inverse Mills Ratio (λ)			.19 (.18)				-.15 (.16)	
Sample	Invited	Pop.	Heckman	Skippers	Invited	Pop.	Heckman	Skippers
Estimator	OLS	2SLS	OLS	OLS	OLS	2SLS	OLS	OLS
Weighted	Y	N	N	Y	Y	N	N	Y
No-Noncompete Mean	0.09	0.64	0.64	0.65	0.05	0.39	0.39	0.38
KP RK Wald F		183.58				183.58		
Cragg-Donald Wald F		257.32				257.32		
Heckman: χ^2 (Excl. Inst.)			33.88***				33.88***	
Salient - Hidden	-0.004 (0.007)	0.01 (0.03)	0.02 (0.03)		0.009** (0.004)	0.08*** (0.03)	0.08*** (0.03)	
Salient Wage Equiv.			-4.16 (6.18)				29.36*** (8.50)	
Hidden Wage Equiv.			0.52 (5.93)	-18.30 (17.00)			49.96*** (10.47)	20.00* (11.47)
Observations	13,851	29,005	13,851	314	13,851	29,005	13,851	314
R^2	.019	.57		.37	.019	.25		.48

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The outcome is having an open Firm B contract; the omitted group is no noncompete. Columns 1-4 are pre-reminder and columns 5-8 are post-reminder. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. Column samples and designs are listed in the headings; skippers signed Firm A's contract without reading page 7. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Signed Salient and Signed Hidden indicate workers who signed those Firm A contracts and completed Firm A's task. Robust standard errors are in parentheses. Wage offers and wage equivalents are dollars per hour; indicator coefficients are percentage points. Note, the no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample.

Wage Equivalents. Because Firm B also randomized wages, we can express the post-reminder mobility effects as benchmarking wage equivalents: \$29.3/hr for the salient noncompete (66% of the average Firm B wage offer), and \$49.9/hr for the hidden noncompete (112% of the average Firm B wage offer). Together with the Phase 1 estimates, this suggests an asymmetry, particularly for hidden noncompetes: they are comparatively easy to enter, but difficult to exit once workers receive a reminder. We interpret this comparison as descriptive rather than as a literal cost ratio across stages. Still, it is consistent with hidden noncompetes generating smaller acceptance losses up front while substantially reducing post-employment mobility, without evidence of wage premia.

V.E Phase 2 Results: Secret Sharing

Table VI applies the same Phase 2 empirical approaches to secret leakage. We measure leakage unconditional on joining Firm B: each worker who received Firm A's résumés is coded as leaking secrets or not, regardless of whether they joined Firm B.²⁰ On average, 4% of workers who completed Firm A's task shared a Firm A résumé with Firm B—counting non-joiners as non-sharers. Pre-reminder, higher wages from Firm B increase sharing, but we find no detectable effects of noncompetes on sharing either pre or post reminder, and can rule out small effects; the same is true in subgroup analyses (Appendix M).²¹

Expert Forecasts. Appendix Table D.1 shows that experts forecasted very different Phase 2 results. Experts predicted that salient noncompetes would deter workers from joining Firm B pre-reminder, and that workers offered salient noncompetes would be least likely to join Firm B post-reminder. They also expected much more secret sharing than we observed

²⁰Appendix N describes the construction of the résumé-sharing outcomes in greater detail and reports estimates using alternative count-based measures. Note that we made slight textual alterations to résumés shared by Firm A so that, if a recruiter later identified a candidate independently, we could distinguish such cases from sharing Firm A's résumés.

²¹Appendix N.1 reports analogous estimates conditional on joining Firm B. Those results are also generally null, although they are not directly comparable because joining Firm B is an endogenous outcome and changes the estimation sample.

Table VI: Firm A Noncompetes and Sharing Firm A Résumés with Firm B Before and After Reminder

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	1(Shared Firm A Resumes with Firm B)							
	Before the Reminder				After the Reminder			
Offered Salient Noncompete	-.00074 (.0021)				.00079 (.00071)			
Offered Hidden Noncompete	.00077 (.0022)				.0012 (.00089)			
Firm A Wage Offer	.000092* (.00005)				3.0e-06 (.000021)			
Signed Salient		.0082 (.012)	.0014 (.013)			.0041 (.0058)	.0014 (.0059)	
Signed Hidden		.017 (.012)	.013 (.012)	.044 (.037)		.0021 (.0054)	.0012 (.0057)	-.0039 (.0093)
Firm A Realized Wage		-.00019 (.00029)	.0004 (.0006)	8.1e-06 (.00094)		-.00022 (.00014)	-.00017 (.00028)	-.00034 (.00023)
Firm B Wage Offer			.0011*** (.00028)	.0022** (.001)			-.000049 (.00013)	-.0001 (.00017)
Inverse Mills Ratio (λ)			.084 (.077)				.011 (.036)	
Sample	Invited	Pop.	Paid by Firm A	Skippers	Invited	Pop.	Paid by Firm A	Skippers
Estimator	OLS	2SLS	Heckman	OLS	OLS	2SLS	Heckman	OLS
Weighted	Y	N	N	Y	Y	N	N	Y
No-Noncompete Mean	0.01	0.03	0.03	0.04	0.00	0.01	0.01	0.01
KP RK Wald F		183.58				183.58		
Cragg-Donald Wald F		257.32				257.32		
Heckman: χ^2 (Excl. Inst.)			33.88***				33.88***	
Salient - Hidden	-0.002 (0.002)	-0.008 (0.01)	-0.01 (0.01)		-0.0004 (0.001)	0.002 (0.006)	0.0002 (0.006)	
Salient Wage Equiv.			-1.26 (11.94)				28.68 (143.56)	
Hidden Wage Equiv.			-12.57 (11.96)	-19.82 (20.87)			24.62 (134.06)	-37.91 (108.79)
Observations	13,851	29,005	13,851	314	13,851	29,005	13,851	314
R ²	.013	.041		.38	.011	.01		.21

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The outcome equals one if the worker shared at least one Firm A résumé with Firm B; non-joiners are coded as non-sharers. The omitted group is no noncompete. Columns 1-4 are pre-reminder and columns 5-8 are post-reminder. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. Column samples and designs are listed in the headings; skippers signed Firm A's contract without reading page 7. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Robust standard errors are in parentheses. Indicator coefficients are percentage points. Note, the no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample.

at baseline—and that noncompetes would reduce secret sharing—even though we gave them the baseline no-noncompete pre- and post-reminder joining rates.

Post-Reminder Sentiment Analysis. Appendix [I](#) analyzes subjects’ written replies to the reminder. Subjects who signed noncompetes responded less positively to the reminder.

Job Search after Release of Noncompete. In Phase 3, as an alternative test of the mobility results, Firm A randomly released workers early from their noncompetes. We measure their job application activity for another job opening within the finance industry. We find that early release increased application activity. Appendix [O](#) contains the full details.

VI Earnings

How do noncompetes affect workers’ earnings? In Table [VII](#) we study cumulative earnings from Firms A and B using our four main specifications. We omit the randomized Firm A and Firm B wage-offer controls because the randomized hourly rates directly enter realized earnings for workers who perform paid work.²² To aid interpretation, we present a Poisson Pseudo-Maximum Likelihood (PPML) model alongside our OLS results. We study earnings separately from Firms A and B in Appendix [P](#).

The results in this section include only income from Firms A and B within the experiment. Workers may have earned income from other sources, which we do not fully observe. Accordingly, these results reflect the within-experiment effects of noncompetes on earnings rather than total income. The direction of any resulting bias is ambiguous: if displaced workers replaced these earnings elsewhere, our estimates may overstate total losses, but if noncompetes discouraged working for other employers, the true losses could be larger. Section [VII](#) and Appendix [Q](#) use platform work-history data to examine whether the Firm A/B earnings losses were offset by other observed work on the same platform.

We find that Firm A requiring a noncompete reduces total earnings from Firm A and B

²²Since wage offers were randomized orthogonally to noncompete assignment, omitting these controls does not affect identification of the noncompete effects; the estimates should be interpreted as effects on realized earnings under the experiment’s wage-offer distribution. The skipper/reminder specifications use a simplified set of fixed effects because the skipper sample is small.

Table VII: Firm A Noncompetes and Earnings from Firms A and B

	(1)	(2)	(3)	(4)	(5)	(6)
	Firm A + B Compensation					
Offered Salient Noncompete	-.19** (.079)	-1.1** (.47)				
Offered Hidden Noncompete	-.13* (.076)	-.81* (.47)				
Signed Salient			-3** (1.5)	-3.7** (1.5)		
Signed Hidden			-3.3** (1.4)	-3.2** (1.4)	-.15** (.062)	-7.6** (3.2)
Inverse Mills Ratio (λ)				1.8 (8.8)		
Sample	Invited	Invited	Pop.	Paid by Firm A	Skippers	Skippers
Estimator	PPML	OLS	2SLS	Heckman	PPML	OLS
Weighted	Y	Y	N	N	Y	Y
No-Noncompete Mean	6.51	6.51	53.91	53.91	55.28	55.28
KP RK Wald F			301.54			
Cragg-Donald Wald F			370.34			
Heckman: χ^2 (Excl. Inst.)				33.51***		
Salient - Hidden	-0.05 (0.08)	-0.30 (0.44)	0.37 (1.44)	-0.47 (1.43)		
Observations	13,851	13,851	29,005	13,851	321	321
R^2		.012	.81			.045

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Earnings are experimental payments from Firms A and B only. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. The omitted group is no noncompete. All models include strata fixed effects; columns 1-2 also include outreach-date fixed effects. Skipper specifications use simplified strata-component fixed effects. Heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Weighted uses inverse-probability sampling weights. PPML coefficients are log points; other coefficients are dollars. Offered rows use randomized assignment; Signed rows use realized Firm A contract status. Note, the no-noncompete mean in the 2SLS model is conditional on working for Firm A and being in the estimation sample.

by 12.2% when the noncompete is hidden, and 17.3% 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 or offered work by Firm B), (b) any compensating differentials at Firm A or B arising from the noncompete, and (c) any effects of noncompetes on preventing individuals from working for Firm B.

Given that, as shown above, workers did not receive higher wages for noncompetes, we turn to noncompete sorting effects as potential explanations for earnings differences. Appendix P disaggregates these effects by payments from Firm A (Table P.1) and Firm B (Table P.2) separately. This reveals that the salient effect on earnings is driven by avoiding Firm A. In contrast, earnings losses from requiring a hidden noncompete come from avoiding Firm B. Indeed, for all estimation approaches we find that signing a salient or hidden noncompete reduces earnings by 5-7% on average, including a 14% drop for skippers—and these differences all arise because of earnings losses with Firm B.

Appendix Figures M.6 to M.8 examine heterogeneity in the causal effects of signing a noncompete on earnings. 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—especially Firm B earnings—while the estimates for signing a *salient* noncompete are noisier.

VII Work beyond Firms A and B

A remaining concern is that the observed earnings losses from Firm A and B could be overstated if workers subject to noncompetes replaced Firm A/B work with other jobs. To assess this, we collected work-history records from the platform used in the experiment for contracting and payroll for the full population of ~29,000 recruiters, covering engagements with other employers before, during, and after the experiment. Appendix G describes the data, coverage, and validation, and Appendix Q reports full details of the analysis. Because monthly platform payments are sparse and highly skewed, our primary outcome is whether a recruiter has an open platform contract in a given month. We use the same empirical strategies described above to study all open contracts, contracts excluding Firms A and B, and contracts classified as plausibly covered by Firm A's noncompete. Appendix Q.7 describes the procedure we used to classify the recruiters' other contracts.

The platform data provide little support for the concern that workers offset the non-compete losses from Firms A and B by substituting toward other same-platform work. Requiring a noncompete lowers aggregate platform contracting when Firms A and B are included, especially after Firm A’s reminder. When Firms A and B are excluded, estimates are less precise, but they do not indicate offsetting substitution into other platform work (see Figure Q.1 and Table Q.1). The skipper design permits a clean event-study and difference-in-differences analysis pre- and post-reminder. The results suggest that learning about the noncompete reduced noncompete-covered work on the platform (outside of Firm A and B), but had no effect on work plausibly uncovered by the noncompete, leading to a negative but statistically insignificant overall effect (see Figure Q.2 and Table Q.3).

Several caveats of this analysis remain: We observe work on one platform, not all labor-market activity, and the platform earnings estimates in Appendix Q.6 are too noisy to support stable dollar estimates. Our conclusion is thus narrower: We find no evidence that work lost from Firms A and B is offset by substitution to other same-platform work.

VIII Discussion and Conclusion

In this final section, we briefly discuss external validity, summarize the main robustness results, and conclude with the paper’s contributions.

VIII.A Generalizability

Contract Recruiters. Contract HR professionals in finance are a distinctive population: likely more familiar with noncompetes, more dependent on multi-firm work, and more comfortable negotiating employment terms than the average worker.

At the same time, many still skipped or barely read the noncompete page. We therefore view this setting less as a literal upper bound than as a comparatively high-awareness environment; if limited attention and large behavioral responses emerge here, they may

plausibly extend to less-informed workers. Consistent with that interpretation, Figure J.8 and Starr, Prescott, and Bishara (2021) show that inattention to noncompetes is not unique to this setting: roughly one-third of workers report either not reading their noncompete at all or reading it only quickly, and only about 10% report negotiating over it.

Trade Secrets, Contract Terms, and Enforcement. Our secret-leakage result is most directly about sharing résumés or client-list-like information. Among firms that use noncompetes, over 90% say they are used to protect client lists (U.S. Government Accountability Office, 2023). However, it is not the only reason, and our results may generalize less to other trade secrets. More broadly, our estimates also depend on the particular noncompete used here, including its scope and duration, the absence of actual litigation, and the fact that Firm B actively solicited workers with noncompetes. If workers respond differently to lawsuits, or if competitors are deterred from making offers *ex ante*, our estimates likely understate the effect of noncompetes on mobility and earnings.

Robustness. Our main results are robust to treating wages as categorical (Appendix H), contamination-bias adjustments (Table J.12), unweighted specifications (Appendix R), multiple-hypothesis corrections (Table J.13), and randomization-inference *p*-values (Table J.14). A remaining limitation is that we do not have complete earnings records.

VIII.B 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. In our platform work-history data, we find no evidence that noncompete workers offset these losses by substituting toward other observed platform work. These results push against positive noncompete-earnings correlations in the literature. They are broadly consistent with more plausibly causal studies of state noncompete policies (Marx, Strumsky, and Fleming, 2009; Johnson, Lavetti, and Lipsitz, 2023). 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 do not enforce them ([Prescott, Bishara, and Starr, 2016](#); [Rothstein and Starr, 2022](#)) and (b) we find that noncompetes cause reductions in mobility and earnings regardless of their actual enforceability in court. The results also contradict claims that noncompetes are necessary to protect trade secrets.

A second contribution is to show that disclosure changes selection, not just information. Making the noncompete salient reduces acceptance, but those who still sign are more likely to disregard the restriction *ex post*, consistent with selection on beliefs about enforcement; in our setting, greater salience does not generate offsetting wage premia when wages are randomized. Given this, firms may have limited reasons to be upfront about their use of noncompetes, and policies that require upfront disclosure require firms to face this adverse selection—or choose not to use noncompetes.

More broadly, the framework of inattention and uncertainty about contract terms may extend to other contract terms, including NDAs ([Hoffman and Lampmann, 2019](#); [Sockin, Sojourner, and Starr, 2021](#)), non-solicitation agreements ([Graves, 2022](#)), and training repayment agreements ([Hoffman and Burks, 2017](#); [Harris, 2020](#)). These settings may likewise feature limited attention, weak *ex ante* premia, and adverse selection when terms are made more salient, and our experimental design may therefore be useful beyond noncompetes.

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Appendix: For Online Publication Only

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

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A Overview of Preregistration and Analysis

Our study was preregistered on the AEA RCT Registry ([AEARCTR-0006906](#)), and our preregistration materials and IRB protocol specify the experimental design, treatment arms, randomization procedures, and the primary outcome families studied. All main causal estimands in the paper — including treatment contrasts, timing, and outcome definitions — follow directly from this preregistered design and from the experimental randomization.

In this section, we compare our preregistration with the analyses contained in the paper. This appendix (i) summarizes the preregistered experimental design, treatment arms, and outcome families, (ii) maps preregistered outcomes to the corresponding main-text and appendix tables, and (iii) clearly labels analyses that were not explicitly preregistered. Table [A.1](#) provides a navigational crosswalk between preregistered outcome families and their corresponding empirical analyses.

Table A.1: **Mapping of Preregistered Outcome Families to Main Text and Appendix Tables and Figures**

Preregistered Outcome Family	Description	Tables	Figures
Job acceptance	Acceptance of Firm A job offer	Tables II, III ; J.6, J.8; R.2	Figures I, J.1, R.1
Contract attention / awareness	Time spent reviewing contract materials (salience and awareness)	Tables J.6, J.8; R.3	Figures II, III, J.2–J.6, R.2–R.3
Wages and compensating differentials	Wages, wage bills, and wage-equivalent measures	Tables III, IV; J.11; R.2, R.5	—
Worker mobility	Acceptance of Firm B job offer (pre/post reminder)	Table V; R.6	Figures IV, J.7, M.2, M.3, O.2
Information sharing	Sharing Firm A information with Firm B	Table VI; R.7	Figures M.4, M.5
Job performance	Performance on Firm A task	Table L.1	—
Earnings	Earnings from Firms A and B	Table VII; R.8	Figures M.6–M.8
Job search after release	Applications following randomized noncompete release	Tables O.1–O.4	Figures O.1, O.2

Notes: These tables do not include our robustness check tables such as **J.12** (contamination bias) or **J.13** (multiple hypothesis testing), which include many of our preregistered outcome variables in one table. In addition, where outcomes are decomposed into multiple intermediate measures (e.g., stages of acceptance), these are analyzed as components of the preregistered outcome family rather than as separately preregistered outcomes. Unlettered table and figure numbers refer to the main text; lettered numbers refer to appendix sections; Appendix O contains unweighted analogues.

Main outcomes. Our preregistration explicitly states our intent to study the effects of noncompete clauses and their salience on job acceptance and wages, compensating differentials, worker mobility to a competing firm, information sharing (secret leakage), job performance, and earnings. These outcomes correspond to all main-text tables and form the basis of the core causal analyses throughout the paper. Thus, the headline results and policy-relevant conclusions are based on preregistered outcome families and treatment contrasts.

Additional outcomes and mechanisms. Although the vast majority of outcomes and analysis dimensions were preregistered, there are two analyses that were not explicitly listed in the preregistration, though they were conceptually related to preregistered mechanisms:

1. *Bargaining attempts.* While our preregistration discusses worker responses to job offers, we did not explicitly list bargaining attempts as a variable to be measured.
2. *Text-based sentiment analysis.* While we discussed worker reactions to reminder messages, we did not preregister the use of machine-learning-based sentiment analysis (DistilBERT) on text responses.

Although the variables above go beyond preregistered outcomes, they are tightly linked to preregistered mechanisms.

Specifications. We did not preregister exact regression functional forms. However, all reported specifications correspond to preregistered estimands implied by the experimental randomization. Our power calculations were based on the core reduced-form specifications for Phase 1, Phase 2, and Phase 3 (Equations 2, 3, and 28), without additional controls. The reported regressions differ only by the inclusion of standard covariates and robustness adjustments, which do not affect identification. As a result, specification choices affect precision but not the identifying variation.

Our analysis plan explicitly contemplated using review of the noncomplete page to address selection into subsequent outcomes, including performance and observable work, and anticipated conditioning analyses on review status. Several estimation approaches used in the paper — including instrumental-variables specifications and Heckman-style selection corrections — were not preregistered. These approaches are used to address selection and compliance issues that arise naturally in multi-stage experimental settings and do not introduce new outcome definitions or treatment contrasts.

Ex ante expectations. Our analysis plan also articulated ex ante expectations regarding the direction and relative magnitude of several effects, including that salient noncompetes would reduce job acceptance, that compensating differentials would be larger for salient than for hidden noncompetes, and that review of the noncomplete would be associated with differential subsequent behavior. These expectations were intended to motivate the empirical analyses and power calculations, rather than to define decision rules for estimation or reporting. All results are reported irrespective of whether they align with these ex ante expectations. In several cases, the data rejected our ex ante expectations, which we view as informative for both theory and policy.

Heterogeneous effects. Our analysis plan preregistered examining heterogeneity by gender, asking rate, prior experience, race proxies, and related background characteristics. However, we did not preregister specific subgroup contrasts, interaction specifications, or functional forms. Accordingly, while the dimensions of heterogeneity were preregistered, the detailed subgroup estimates are presented descriptively rather than as confirmatory tests.

Summary. All primary outcome families and core analysis dimensions were specified prior to data access. Deviations from the preregistration are limited to estimation choices and ancillary analyses that do not affect identification or the set of estimands studied.

B Theoretical Appendix

B.1 Proofs for Results in Main Paper

Throughout this subsection, $\mathbb{E}[u_i(w, d_i)]$ denotes the worker's ex ante utility at the time of acceptance, computed under the worker's subjective beliefs $(\hat{p}_n, \hat{p}_e)$. After the Period-2 realization, $u_i(w, d_i)$ denotes realized utility under the realized indicators (p_n, p_e) . Under full information, the two coincide.

Lemma A1. *Consider two contracts. The first induces distaste distribution F_Θ and yields no noncompete benefit ($\eta = 0$). The second induces a left-shifted distaste distribution $F_{\Theta'}$, where $\Theta' = \Theta - \delta$ for some $\delta \geq 0$, and yields employer benefit $\eta > \delta$. Define*

$$\phi_\Theta(w) \equiv w + \frac{1 - F_\Theta(-w)}{f_\Theta(-w)}.$$

Then the optimal wages are

$$w_{*\Theta} = \phi_\Theta^{-1}(v) \quad \text{and} \quad w_{*\Theta'} = \phi_{\Theta'}^{-1}(v + \eta - \delta) + \delta.$$

Proof. For the first contract, the firm's objective is

$$(v - w)[1 - F_\Theta(-w)].$$

Differentiating with respect to w gives

$$-(1 - F_\Theta(-w)) + (v - w)f_\Theta(-w) = f_\Theta(-w)(v - \phi_\Theta(w)).$$

By regularity, ϕ_Θ is increasing, so the unique optimum satisfies $\phi_\Theta(w_{*\Theta}) = v$, i.e.

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

For the second contract, the objective is

$$(v + \eta - w)[1 - F_{\Theta'}(-w)].$$

Because $\Theta' = \Theta - \delta$, we have $F_{\Theta'}(x) = F_\Theta(x + \delta)$. Let $x = w - \delta$. Then

$$(v + \eta - w)[1 - F_{\Theta'}(-w)] = (v + \eta - \delta - x)[1 - F_\Theta(-x)].$$

Thus the firm's problem is equivalent to choosing x to maximize

$$(v + \eta - \delta - x)[1 - F_\Theta(-x)],$$

whose unique optimum satisfies

$$\phi_{\Theta}(x) = v + \eta - \delta.$$

Hence

$$x = \phi_{\Theta}^{-1}(v + \eta - \delta), \quad w_{*\Theta'} = x + \delta = \phi_{\Theta}^{-1}(v + \eta - \delta) + \delta.$$

□

Lemma A2. *Consider the two contracts in Lemma A1. The second contract pays a higher wage.*

Proof. By Lemma A1,

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

Because ϕ_{Θ}^{-1} is increasing and $\eta > \delta$, we have $v + \eta - \delta > v$. Hence the bracketed term is strictly positive, and adding $\delta \geq 0$ yields

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

□

B.1.1 Proof for Proposition 1

Proposition 1 (Full Information). *If $p_n = p_e = 1$ and workers have full information ($\hat{p}_n = p_n, \hat{p}_e = p_e$), then (i) $w_{NC} > w_{\emptyset}$ and (ii) $u_i(w_{NC}, d_i) \geq 0$ for all workers who sign a noncompete.*

Proof. For a contract with no noncompete, the perceived burden is $\delta = 0$ and the employer benefit is zero. For an effective noncompete, $p_n = p_e = 1$ and full information implies $\hat{p}_n = \hat{p}_e = 1$, so the perceived burden is $\delta = k$ and the employer benefit is η . The main text assumption gives $\eta > k$. Applying Lemma A2 delivers part (i): $w_{NC} > w_{\emptyset}$.

For part (ii), under full information ex ante and realized utility coincide. A worker signs the noncompete only if

$$u_i(w_{NC}, d_i) = w_{NC} + d_i - k \geq 0.$$

Thus every worker who signs is weakly better off than taking the outside option. □

B.1.2 Proof for Proposition 2

Proposition 2. *If $\hat{p}_n = \bar{p}_n$ in the hidden-clause environment, then (i) $w_H > w_{\emptyset}$; (ii) w_H contains no additional term for the realized burden $p_n p_e k$ on the worker's own contract; and (iii) if $\bar{p}_n < 1$ and $\hat{p}_e > 0$, 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 $p_n = p_e = 1$.*

Proof. In the hidden-clause case, workers assign any given contract the common probability $\bar{p}_n$ of containing a noncompete. Hence the perceived burden is $\delta = \bar{p}_n \hat{p}_e k$. Since $\eta > \bar{p}_n \hat{p}_e k$, Lemma A1 applies and gives

$$w_H = \phi_{\Theta}^{-1}(v + \eta - \bar{p}_n \hat{p}_e k) + \bar{p}_n \hat{p}_e k.$$

Part (i) then follows from Lemma A2: $w_H > w_{\emptyset}$.

For part (ii), the labor-supply term in the firm's wage-setting problem is determined by the common perceived burden $\bar{p}_n \hat{p}_e k$, not by the realized event $p_n = 1$ or the realized burden $p_n p_e k$ on a particular worker's contract. Thus workers who actually end up bound are accounted for in wage setting only through the common expected burden. The inequality $w_H > w_{\emptyset}$ in part (i) is therefore not a realized-clause compensating differential; it also reflects the firm's private value η when the hidden restriction is effective.

For part (iii), ex ante utility from accepting the hidden contract is

$$\mathbb{E}[u_i(w_H, d_i)] = d_i + w_H - \bar{p}_n \hat{p}_e k = d_i + \phi_{\Theta}^{-1}(v + \eta - \bar{p}_n \hat{p}_e k).$$

So workers accept whenever

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

If the worker later learns that the contract contains a noncompete and the restriction is enforced ($p_n = p_e = 1$), realized utility becomes

$$u_i(w_H, d_i) = d_i + w_H - k = d_i + \phi_{\Theta}^{-1}(v + \eta - \bar{p}_n \hat{p}_e k) - (1 - \bar{p}_n \hat{p}_e)k.$$

Therefore $u_i(w_H, d_i) < 0$ whenever

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

Because $\bar{p}_n < 1$ and $\hat{p}_e \leq 1$, the interval

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

is nonempty. For any d_i in this interval, $\mathbb{E}[u_i(w_H, d_i)] \geq 0$ but realized utility satisfies $u_i(w_H, d_i) < 0$ if $p_n = p_e = 1$. $\square$

B.1.3 Proof for Proposition 3

Proposition 3. *If $\hat{p}_n = p_n = 1$, then (i) for any common $\hat{p}_e$, only workers with sufficiently high d_i accept w_{NC} , and the acceptance cutoff is lower when $\hat{p}_e$ is lower; and (ii) for any $\epsilon > 0$, there exist parameter values such that $0 < w_{NC} - w_{\emptyset} < \epsilon$.*

Proof. When the clause is visible, $\hat{p}_n = p_n = 1$, so the perceived burden is $\delta = \hat{p}_e k$. Since $\eta > \hat{p}_e k$, Lemma A1 gives

$$w_{NC} = \phi_{\Theta}^{-1}(v + \eta - \hat{p}_e k) + \hat{p}_e k.$$

For part (i), ex ante utility from accepting the visible noncompete is

$$\mathbb{E}[u_i(w_{NC}, d_i)] = d_i + w_{NC} - \hat{p}_e k = d_i + \phi_{\Theta}^{-1}(v + \eta - \hat{p}_e k).$$

Hence workers accept only if

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

For any common $\hat{p}_e$, this is a cutoff in d_i . Since ϕ_{Θ}^{-1} is increasing, lowering $\hat{p}_e$ lowers the cutoff $-\phi_{\Theta}^{-1}(v + \eta - \hat{p}_e k)$, so acceptance becomes easier. This proves part (i).

For part (ii), define

$$\Delta(\eta, \hat{p}_e) \equiv w_{NC} - w_{\emptyset} = \phi_{\Theta}^{-1}(v + \eta - \hat{p}_e k) + \hat{p}_e k - \phi_{\Theta}^{-1}(v).$$

Fix any $\epsilon > 0$. Choose $\hat{p}_e > 0$ small enough that $\hat{p}_e k < \epsilon/2$. By continuity of ϕ_{Θ}^{-1} , choose $\xi > 0$ small enough that

$$0 < \phi_{\Theta}^{-1}(v + \xi) - \phi_{\Theta}^{-1}(v) < \epsilon/2.$$

Now set $\eta = \hat{p}_e k + \xi$. Then $\eta > \hat{p}_e k$, and

$$\Delta(\eta, \hat{p}_e) = \phi_{\Theta}^{-1}(v + \xi) - \phi_{\Theta}^{-1}(v) + \hat{p}_e k < \epsilon.$$

Because both terms on the right-hand side are positive, $\Delta(\eta, \hat{p}_e) > 0$. Hence

$$0 < w_{NC} - w_{\emptyset} < \epsilon,$$

proving part (ii). □

B.1.4 Proof for Proposition 4

Proposition 4. *If $\hat{p}_n = p_n = 1$ and $\hat{p}_e \in (0, 1)$, then there exists d_i such that $\mathbb{E}[u_i(w_{NC}, d_i)] \geq 0$, but $u_i(w_{NC}, d_i) < 0$ if $p_e = 1$.*

Proof. In the visible-clause case, $\hat{p}_n = p_n = 1$. By Lemma A1 with $\delta = \hat{p}_e k$,

$$w_{NC} = \phi_{\Theta}^{-1}(v + \eta - \hat{p}_e k) + \hat{p}_e k.$$

Ex ante utility from accepting is

$$\mathbb{E}[u_i(w_{NC}, d_i)] = d_i + w_{NC} - \hat{p}_e k = d_i + \phi_{\Theta}^{-1}(v + \eta - \hat{p}_e k).$$

So workers accept whenever

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

If enforcement is later realized ($p_e = 1$), utility becomes

$$u_i(w_{NC}, d_i) = d_i + w_{NC} - k = d_i + \phi_{\Theta}^{-1}(v + \eta - \hat{p}_e k) - (1 - \hat{p}_e)k.$$

Therefore $u_i(w_{NC}, d_i) < 0$ whenever

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

Because $\hat{p}_e \in (0, 1)$, the interval

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

is nonempty. For any d_i in this interval, $\mathbb{E}[u_i(w_{NC}, d_i)] \geq 0$ but $u_i(w_{NC}, d_i) < 0$ if $p_e = 1$. $\square$

B.2 Supplementary Results and Proofs

Corollary A1. [Noncompete Adoption Cutoffs] Let τ equal the worker's perceived expected noncompete burden.

1. **Detectable noncompete (contract-specific beliefs).** If the presence of a noncompete is contract-specific at hiring, so that an offer with a noncompete induces perceived burden

$$\tau^{NC} = \hat{p}_n \hat{p}_e k$$

(for some $\hat{p}_n \in [0, 1]$ reflecting the worker's perceived probability of being bound, e.g. due to attention), while an offer with no noncompete induces $\tau^0 = 0$, then the firm's maximized expected profits are

$$\Pi^{NC} \equiv \max_w (v + \eta - w) [1 - F_d(\hat{p}_n \hat{p}_e k - w)], \quad \Pi^0 \equiv \max_w (v - w) [1 - F_d(-w)],$$

and the firm (weakly) prefers adopting a noncompete if and only if

$$\eta \geq \hat{p}_n \hat{p}_e k \quad (\text{strictly if } \eta > \hat{p}_n \hat{p}_e k; \text{ indifferent if } \eta = \hat{p}_n \hat{p}_e k).$$

2. **Noncompete not detectable at the employer level (common accurate priors).** If noncompetes are not detectable on an individual employer basis at hiring and workers instead assign a common prior $\bar{p}_n \in [0, 1]$ (accurate in the sense that it equals the true prevalence) to the presence of a noncompete regardless of the firm's actual choice, then both contracts induce the same perceived burden

$$\tau^{NC} = \tau^0 = \bar{p}_n \hat{p}_e k,$$

so the firm's maximized expected profits are

$$\tilde{\Pi}^{NC} \equiv \max_w (v + \eta - w) [1 - F_d(\bar{p}_n \hat{p}_e k - w)], \quad \tilde{\Pi}^0 \equiv \max_w (v - w) [1 - F_d(\bar{p}_n \hat{p}_e k - w)],$$

and the firm (weakly) prefers adopting a noncompete if and only if

$$\eta \geq 0 \quad (\text{strictly if } \eta > 0; \text{ indifferent if } \eta = 0).$$

Intuition. Corollary A1 compares the firm's private value from an effective noncompete, η , with the labor-supply cost created by workers' beliefs. When the noncompete is contract-specific at hiring, adopting the clause changes the burden workers attach to that offer: at a given wage, the perceived expected cost $\hat{p}_n \hat{p}_e k$ makes acceptance less attractive. The firm can offset this by raising wages, but adoption is privately profitable only if the private value η is at least as large as that perceived burden.

When noncompetes are not detectable at the employer level, a given firm's own choice does not change the labor-supply curve it faces. Workers price all offers using the same common prior $\bar{p}_n \hat{p}_e k$, whether or not this firm adopts. The firm therefore captures η without facing an additional contract-specific recruitment cost, so any positive private value makes adoption privately attractive. These are private adoption cutoffs, not welfare cutoffs: in the non-detectable case, adoption can be privately attractive even though workers may later bear a realized burden if the clause is revealed and effective.

Proof. Fix any perceived burden τ . A worker accepts at wage w if and only if $d \geq \tau - w$, which occurs with probability $1 - F_d(\tau - w)$.

(i) Detectable noncompete. Let $t \equiv \hat{p}_n \hat{p}_e k$. Under a noncompete, acceptance is $1 - F_d(t - w)$; under no noncompete, acceptance is $1 - F_d(-w)$. For any u , set $w = u + t$. Then

$$\begin{aligned} (v + \eta - w)[1 - F_d(t - w)] &= (v + \eta - (u + t))[1 - F_d(-u)] \\ &= (v - u)[1 - F_d(-u)] + (\eta - t)[1 - F_d(-u)]. \end{aligned}$$

Since $1 - F_d(-u) \geq 0$ for all u , if $\eta < t$ then the right-hand side is weakly smaller than $(v - u)[1 - F_d(-u)]$ for every u , implying $\Pi^{NC} \leq \Pi^0$. If $\eta > t$, the right-hand side is weakly larger for every u and strictly larger for any u with $1 - F_d(-u) > 0$, implying $\Pi^{NC} \geq \Pi^0$ (and strictly so in nondegenerate cases). If $\eta = t$, the two objectives coincide pointwise, so $\Pi^{NC} = \Pi^0$. This yields the cutoff $\eta \geq t$.

(ii) Not detectable with common accurate priors. Let $\bar{t} \equiv \bar{p}_n \hat{p}_e k$. In this case the acceptance probability is $1 - F_d(\bar{t} - w)$ under both contracts. Therefore, for every w ,

$$(v + \eta - w)[1 - F_d(\bar{t} - w)] = (v - w)[1 - F_d(\bar{t} - w)] + \eta [1 - F_d(\bar{t} - w)].$$

Since $1 - F_d(\bar{t} - w) \geq 0$, the profit under a noncompete is weakly larger than under no noncompete for every w if and only if $\eta \geq 0$ (and weakly smaller for every w if $\eta \leq 0$). Taking maxima over w yields $\tilde{\Pi}^{NC} \geq \tilde{\Pi}^0$ if and only if $\eta \geq 0$ (with indifference at $\eta = 0$). $\square$

Proposition A1. Suppose that workers cannot detect noncompetes, $\hat{p}_e > 0$, and have an overoptimistic belief about the base rate of noncompetes, so that $\hat{p}_n < \bar{p}_n$. Some workers who sign the noncompete are ex ante worse off.

Proof. Lemma A1 shows that the wage 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:

$$d_i + \phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) + \hat{p}_n \hat{p}_e k - \bar{p}_n \hat{p}_e k \geq 0 \quad (5)$$

$$\implies d_i + \phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) + \hat{p}_e k(\hat{p}_n - \bar{p}_n) \geq 0 \quad (6)$$

$$\implies d_i + \phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) - \hat{p}_e k(\bar{p}_n - \hat{p}_n) \geq 0 \quad (7)$$

$$\implies -\phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) + \hat{p}_e k(\bar{p}_n - \hat{p}_n) \leq d_i \quad (8)$$

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.

$$d_i + \phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) \geq 0 \quad (9)$$

$$\implies d_i \geq -\phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) \quad (10)$$

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

$$\underline{d}'_i \leq \underline{d}_i \quad (11)$$

$$-\phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) \leq -\phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e k) + \hat{p}_e k(\bar{p}_n - \hat{p}_n) \quad (12)$$

$$0 \leq \hat{p}_e k(\bar{p}_n - \hat{p}_n) \quad (13)$$

Because $\hat{p}_e > 0$, $k > 0$, and $\bar{p}_n - \hat{p}_n > 0$, the interval $[\underline{d}'_i, \underline{d}_i]$ is nonempty. Hence 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. $\square$

Proposition A2. Suppose that workers exhibit hyperbolic discounting (Laibson, 1997), with $\beta \in (0, 1)$, and that $\hat{p}_n \hat{p}_e > 0$. Although the net present value of lost wages is k , the worker discounts this further to βk . The worker does 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 the wage 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:

$$d_i + \phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e \beta k) + \hat{p}_n \hat{p}_e \beta k - \hat{p}_n \hat{p}_e k \geq 0 \quad (14)$$

$$\implies d_i + \phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e \beta k) + \hat{p}_n \hat{p}_e k(\beta - 1) \geq 0 \quad (15)$$

$$\implies d_i + \phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e \beta k) - \hat{p}_n \hat{p}_e k(1 - \beta) \geq 0 \quad (16)$$

$$\implies -\phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e \beta k) + \hat{p}_n \hat{p}_e k(1 - \beta) \leq d_i \quad (17)$$

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.

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

$$\implies d_i \geq -\phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e \beta k) \quad (19)$$

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

$$\underline{d}'_i \leq \underline{d}_i \quad (20)$$

$$-\phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e \beta k) \leq -\phi_{\Theta}^{-1}(v + \eta - \hat{p}_n \hat{p}_e \beta k) + \hat{p}_n \hat{p}_e k(1 - \beta) \quad (21)$$

$$0 \leq \hat{p}_n \hat{p}_e k(1 - \beta). \quad (22)$$

Because $\hat{p}_n \hat{p}_e > 0$, $k > 0$, and $1 - \beta > 0$, the interval $[\underline{d}'_i, \underline{d}_i]$ is nonempty. Hence 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 discounting of future wage penalties. $\square$

C Research Ethics

We are often asked about the ethical considerations in this experiment, given that the experiment randomizes some workers into noncompetes that may limit their ability to work. We understand these concerns, and we planned extensively to address the ethical issues raised by this experiment (both by ourselves and in conversation with our IRB). In this section, we describe how our experiment was proactively designed to minimize possible harm.

Steps to Limit Harm. Here are several key facts relating to the research ethics of this experiment.

1. Firm A previously used hidden noncompetes. The main “treatment” in our experiment from Firm A’s perspective consisted of making the noncompete more salient and/or removing it altogether. In some cases we discuss the treatment effects of noncompetes (rather than of “no noncompetes”) because this is the orientation of the literature. However, from Firm A’s perspective, this experiment is about understanding the effect of removing the noncompete or making it more salient.
2. As a condition of our experiment, neither the noncompete nor the NDA was ever enforced. Identifying information was not shared back with our partner firms.
3. Subjects were proactively released approximately 3 months early from the six-month noncompete they signed. This is discussed in Phase 3.
4. Any recruiter who worked for Firm A and completed all aspects of the task on time was left a five-star review with a positive, descriptive message in the review. [Pallais \(2014\)](#) suggests this kind of labor market feedback helps workers obtain future work. Over 95% of our subjects followed basic instructions and received this review. The others did not get a negative review, but were simply not reviewed.
5. There was no deception in the experiment and recruiters were not forced or coerced into any behavior. The job offer was clear that the recruiter was offered a résumé

review task, with no promise of future work. The recruiters were welcome to opt out of the work for Firm A if they wanted. The recruiters could read the contract or not. Regardless of their choices they faced no legal risk.

The Reminder. Phase 2 of the experiment includes a reminder message of the workers' ongoing contractual obligations that they agreed to when they signed the contract. This reminder letter reflects the typical exit process, where workers are reminded of their ongoing contractual obligations (see e.g., [here \[link\]](#)). Draft reminder-letters similar to the ones used in this study are also common (see e.g., [here \[link\]](#) and [here \[link\]](#)) and were used at Firm A before the experiment. These include letters specifically designed to be sent within a month after the employee leaves (see [here \[link\]](#)). Despite the commonality of these practices, we nevertheless took additional steps to limit harm, as described in the next section.

Noncompete Exceptions. To mitigate any other harm arising from the noncompete or the reminder, we adopted a policy of releasing any recruiter from the noncompete who asked for an exception to avoid a specific harm even after they signed it. This was used in five instances in which a recruiter asked if the noncompete required them to quit other work. We told all five that they did *not* need to quit other work. Workers were not granted an exception simply for expressing general annoyance in response to the reminder. Although some workers did state the noncompete reminder was annoying, the vast majority acknowledged the reminder by saying “okay” or “thank you.”

Sometimes we are asked whether workers contacted Firm A again about taking Firm B's job offer. To the best of our knowledge, no worker explicitly named Firm B and asked Firm A about Firm B's job offer. However, there were a few questions from recruiters asking about the scope of Firm A's noncompete around the time of Firm B's offer. We presume these questions may have been motivated by the Firm B offer. In these cases, the worker was re-sent the language of the noncompete, without any further interpretation or guidance.

Sample Size Considerations. The experiment did require more recruiters than would usually be required to help fill one vacancy at Firm A. In this sense, the experiment ended up binding more workers to noncompetes than would otherwise have occurred with Firm A. This was necessary to (a) ensure that all recruiters are doing the same task, and (b) address issues of statistical power. Sample sizes were chosen via backward induction (since Phase 2 power is inversely related to first phase power to identify the deterrent effect of noncompetes on Firm A take-up), targeting approximately 500-600 workers in each noncompete treatment group in the second phase.

Subject Population. Among all feasible populations to study, the population of contract HR recruiters is relatively unlikely to suffer harm from noncompetes. This is because they

are likely to understand the importance of knowing what's in a contract, to be aware of the potential harms of noncompetes, and to be comfortable with negotiation.

All elements of the project were approved by the University of Maryland IRB, such that we minimized harm to the subject pool while adequately powering our experiment and ensuring the internal validity of the study.

More broadly, the benefits of this study will be relevant to policymakers currently considering the necessity of noncompetes among independent contractors (as well as employees). It will also be relevant to firms that currently use noncompetes with independent contractors, like Firm A, so that they might understand how such restrictions are impacting their business. To the best of our knowledge, the leadership at Firms A and B has a more negative view of noncompetes after this experiment and the firms will likely use noncompetes less with independent contractors in the future.

D Expert Forecasts

What results should we *expect* from the experiment? As a benchmark, we ran a survey on [SocialSciencePrediction.org](https://socialscienceprediction.org), a platform operated by U.C. Berkeley for collecting “expert forecasts of the effects of [...] social programs.” Our survey described the experimental design and asked the expert panel to forecast our results (Table [D.1](#) summarizes).

Table D.1: Expert Forecasts for Phase 1 Signing, Phase 2 Mobility, and Secret Sharing

	(1) Experimental Result	(2) Mean Expert Result	(3) Difference	(4) Expert N
<i>% Signing Contract</i>				
Salient noncompete	13.0	10.7	-2.4***	34
Hidden noncompete	13.7	15.8	2.1	34
<i>% Joining Firm B Pre-Reminder</i>				
Salient noncompete	68.6	35.5	-33.1***	33
Hidden noncompete	65.8	48.4	-17.3***	33
<i>% Joining Firm B Post-Reminder</i>				
Salient noncompete	27.2	17.8	-9.5***	33
Hidden noncompete	17.3	21.4	4.2	33
<i>% Secret Sharing Pre-Reminder</i>				
No noncompete	4.3	37.9	33.6***	33
Salient noncompete	4.4	27.3	22.9***	33
Hidden noncompete	5.4	37.3	31.9***	33
<i>% Secret Sharing Post-Reminder</i>				
No noncompete	0.4	25.3	25.0***	33
Salient noncompete	1.2	21.4	20.2***	33
Hidden noncompete	1.4	21.5	20.0***	33

Notes: Forecasts are from the Social Science Prediction Platform. Column 1 reports experimental percentages; column 2 reports mean U.S. expert forecasts; column 3 reports forecast minus experiment (stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$); column 4 reports the number of U.S. responses. For signing and Firm B joining questions, experts were given the no-noncompete baseline rate. Columns 1-3 are in percentage points. See Starr, Evan, Cowgill, Bo, and Brandon Freiberg. 2024. "Noncompetes Field Experiment." Social Science Prediction Platform. April 3. <https://socialscienceprediction.org/s/xfy0c6>.

E Noncompete Clauses Among Independent Contractors and Part-Time Work

In this section, we present evidence that noncompetes for independent contractors and temporary work are indeed common beyond the immediate setting of our experiment. Our evidence is organized in the sections below.

1. Broad, “hidden” noncompetes with temporarily employed workers are common.
2. Associations and unions for freelancers voice concern about them.
3. Public policy has specifically sought to address noncompetes for independent contractors.
4. A strand of academic literature specifically addresses them.

Noncompetes in Freelance/Temporary Work. There is direct evidence that noncompetes are commonly used for independent contractors, unpaid interns, and temporarily employed workers. As a headline statistic, the GAO report on noncompetes ([U.S. Government Accountability Office, 2023](#)) finds that among companies that use noncompetes and hire part-time workers, 53.1% of those companies use noncompetes with *all* of their part-time workers. The GAO also reports that a company’s primary justification for using noncompetes with part-time workers is to protect trade secrets. Lastly, the GAO report finds that 31% of firms that use noncompetes with part-time workers have enforced those noncompetes in the last five years. These statistics help set a baseline that noncompetes are regularly used for part-time workers.

For independent contractors specifically, noncompetes are often included in [standard contract templates \[link\]](#). There are many [legal articles \[link\]](#) describing how to use noncompetes with independent contractors and when they can be enforced (subject to the state’s reasonableness criteria). While independent contractor noncompetes might seem draconian to some, firms justify their necessity by emphasizing that they have no ongoing relationship with temporary contractors who might misappropriate their valuable, proprietary information.

In addition to templates and advice articles, there are many examples of real independent contractor or temporarily employed/volunteer noncompetes that are quite broad. See Appendix Table [E.1](#) for more than 50 examples. We list a few here.

1. [5-year noncompete for window cleaning independent contractor \[link\]](#).
2. [5-year noncompete for tutoring independent contractor \[link\]](#).
3. [3-year noncompete for unpaid intern \[link\]](#).
4. [2-year noncompete for volunteer junior coach \(female high school student over 16 years old\) for nonprofit Girls on the Run \[link\]](#).

5. 1-year California noncompete for contractor for moving services [\[link\]](#), entirely unenforceable.
6. 1-year recruiter noncompete for 3 months of work [\[link\]](#).
7. 1.5-year noncompete for temporary packers at Amazon [\[link\]](#).

Public Discussion and Advocacy by Contractors. Contract workers often discuss noncompetes. The Freelancers Union [advocates against them \[link\]](#). For example, there are [various legal articles \[link\]](#) and [Freelancer forums \[link\]](#) that discuss the challenges freelancers face with noncompetes.

Public Policy around Noncompetes for Independent Contractors. While many states enforce noncompetes against independent contractors (e.g., [in Florida, \[link\]](#)), other policy-makers have taken action specifically on freelance noncompetes. Here are a few examples:

1. [New York City is considering a ban on noncompetes specifically for freelancers \[link\]](#). The bill reads: “The council finds and declares that covenants not to compete are increasingly becoming common in contracts between hiring parties and freelance workers.”
2. When the state of Washington banned noncompetes for employees earning below \$100K in 2020, [it also banned noncompetes for independent contractors earning below \\$250K \[link\]](#).
3. [The FTC’s policy statement in 2022 on gig workers \[link\]](#) references that noncompetes may be an unfair method of competition in gig work.

Legal Scholarship. While the academic literature on noncompetes is vast, it also includes recent contributions specifically about noncompetes and contractors. See e.g.,

- [Sandor, M.J., 2022](#). Independent Contractors & Noncompetition Covenants: A Modified Approach. *Fordham L. Rev.*, 91.
- [Misner, C., 2023](#). Dependent Contractors? The Case for Giving Non-Competes a Central Role in Worker-Classification Tests under Federal Law. *Cornell L. Rev.*, 109, p. 763.

Given that these noncompetes are both common and the subject of public concern, advocacy, and scholarship, one may wonder why workers still sign them. This is part of what motivated our theoretical model in Section II. We theorize that some workers are not aware of them (as they are “hidden”) — and even when they are aware of them, they are uncertain if they will be enforced. We designed details of the experiment (e.g., measuring how long each worker spent on the noncompete page) to shed light on these mechanisms.

Table E.1: Examples of Noncompete Clauses for Independent Contractors, Temporary Workers, Interns, and Volunteers

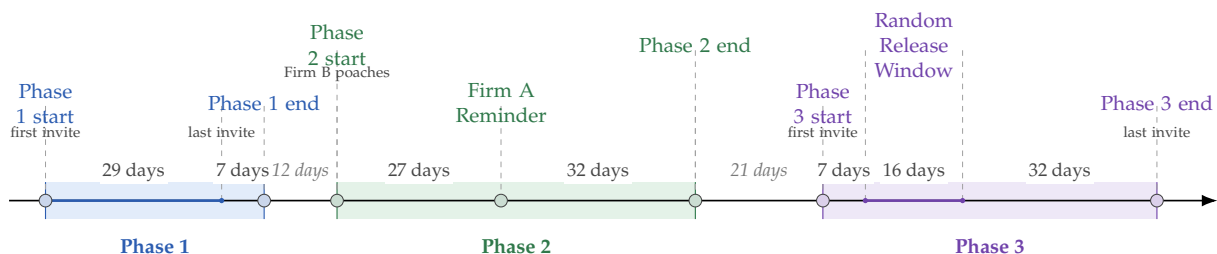
	Job Role	Duration	Link
	<i>Templates</i>		
1	Independent Contractor	Varies	https://www.ilrg.com/...
2	Independent Contractor	1 Year	https://www.jotform.com...
3	Independent Contractor	Not specified	https://noncompete.com/contractor/...
4	Independent Contractor	Not specified	https://employmentcontracts.com...
5	Medical Services Contractor	Variable	https://www.chellelaw.com/sample...
	<i>Actual Contracts</i>		
6	\$15/hour Independent Contractor	50 Years	https://nationalstemsociety.org...
7	Window Cleaner	5 Years	https://www.legrandwindowcleaning.com...
8	Tutor	5 Years	https://frogtutoring.com/...
9	Professional Security Contractor	5 Years	https://www.lasorsa.com/...
10	Volunteer (High Schooler) with Nonprofit	2 Years	https://www.girlsontherun.org/...
11	Seasonal Amazon Packers	1.5 Years	https://www.theverge.com/...
12	Independent Contractor for Moving Services	1 Year	https://thestudentmovers.com/independent-...
13	Freelance Graphic Designer	1 Year	https://www.liquidpurple.com/terms...
14	Hair Stylist/Barbershop Contractor	Variable	https://efirms.com/...
	<i>Legal Guidance for Freelancers and Firms</i>		
15	Independent Contractor	Not specified	https://www.legalzoom.com/...
16	Independent Contractor	Not specified	https://www.oysterhr.com/...
17	Freelancer	Not specified	https://www.adp.com/...
18	Freelancer	Not specified	https://www.fmcolaw.com/...
19	Freelancer	1-3 Years	https://www.guidingcreatives.co...
20	Freelancer	Not specified	https://medium.com/...
21	Hair Stylist	Not specified	https://legalclarity.org/...
	<i>Contract Terms/Case Studies/Discussions</i>		
22	Mental Health Independent Contractor	1 Year	https://www.reddit.com/r/legaladvic...
23	Software Intern	3 Years	https://www.reddit.com/r/cscareer...
24	Temporary Recruiter	1 Year	https://www.reddit.com/r/recruiting/...
25	Commission-Only Recruiter	Not specified	https://www.reddit.com/r/recruiting...
26	Insurance Adjuster	2 Years	https://www.pacourts.us/...
27	Personal Trainer	2 Years	https://www.tradesecretslaw.com/...
28	Personal Trainer	Not specified	https://www.acefitness.org/...
29	Speech Therapist	2 Years	https://www.berrylegal.com/...
30	Freelance Writer	2 Years	https://wellfedwriter.com/...
31	Freelance Writer	1 Year	https://www.reddit.com/r/freelanceWriters/...
32	Freelance Writer	Not specified	https://katboogaard.com/...
33	Freelance Writer	2 Years	https://www.reddit.com/r/legaladvice/...
34	Wedding Photographer	1 Year	https://www.reddit.com/r/photography...
35	Freelance Photographer	2 Years	https://www.reddit.com/r/photography...
36	Freelance Photographer and Videographer	2 Years	https://www.reddit.com/r/freelance...
37	Real Estate Photographer	1 Year	https://www.reddit.com/r/RealEstatePhotography...
38	Freelance Videographer	2 Years	https://www.reddit.com/r/videography...
39	Freelance Graphic Designer	1 Year	https://www.reddit.com/r/graphic_design...
40	Freelance Designer	2 Years	https://www.reddit.com/r/freelance...
41	Maintenance Worker	Not specified	https://www.slnlaw.com/...
42	CS Internship	6 Months	https://www.reddit.com/r/cscareer...
43	IT Contractor	1 Year	https://www.justanswer.com/...
44	Part-time Staffing Company Sales	2 Years	https://www.reddit.com/r/recruiting...
45	Tutoring company wants to use noncompetes	Not specified	https://www.avvo.com/...
46	Tutor	Not specified	https://www.reddit.com/r/education...
47	Therapist	2 Years	https://familytherapybasics.com/...
48	Massage Therapist	1 Year	https://www.justanswer.com/...
49	Yoga Instructor	1 Year	https://www.npr.org/...
50	Telecom Independent Contractor	Not specified	https://www.slnlaw.com/...
51	Dog Walker	1 Year	https://www.reddit.com/r/legaladvice/...
52	Dog Sitter	2 Years	https://prosperouspetbusiness.com/...

Notes: This table lists examples of noncompete clauses and guidance sources for non-employee or temporary work arrangements; displayed links are shortened for space, and durations are taken from the cited source material where available.

F Experimental Details

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

Figure F.1: Experimental Design Timeline



Notes: This figure shows the full timeline for the experiment. Figures I, IV, and O.1 contain details about Phases 1, 2, and 3, respectively.

F.1 Firm A Recruiters

Firm A did not have high turnover, and we estimate it hired on average 2-3 recruiters per year, who worked a total of 30-35 hours each. However, Firm A offices were co-located with a larger suite of business and philanthropic organizations under common ownership (described in Section III.C).

Based on historical trends in this business group, we estimate they purchase around 3,500–4,900 hours of external recruiting assistance per year. We arrived at this estimate by asking executives about their historical levels of hiring full-time roles (estimated 70 per year), and the amount of external recruiting work per full-time hire (conservatively, 50–70 hours). Although this number does represent recruiting work by external HR specialists, some recruiters were re-used across multiple recruiting tasks or needs. We estimate around 75–90 distinct external individuals (conservatively) were engaged in recruiting per year.

F.2 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,²³ 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

²³We based this classification on the coding of state noncompete laws described [here](#).

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.

As described in the main text, we assigned treatment conditions *ex ante* to all subjects even for those not included in our random sample of invited recruiters. The balance table from the full population is shown in Table J.1. The balance table from the unweighted invited sample is shown in Table J.2.

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 many 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 descending order of batch size. This guaranteed that we had some coverage for each stratum.

F.3 Relevant Pages of the Employment Contract

Figure F.2: Page 1 of Salient Noncompete Treatment

NON-COMPETE COVENANT

NON-COMPETE COVENANT. During the paid engagement with the Company and for a period of 6 months after the separation of the engagement for any reason, Recipient will not directly or indirectly engage in any business that competes with the Company, including but not limited to, businesses 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.

Directly or indirectly engaging in any competitive business includes, but is not limited to: (i) engaging in a business as Company, partner, or agent, (ii) becoming an employee or independent contractor of any third party that is engaged in such business, (iii) becoming interested directly or indirectly in any such business, or (iv) soliciting any customer, applicant, or employee of the Company for the benefit of a third party that is engaged in such business. Recipient agrees that this non-compete agreement will not adversely affect Recipient's livelihood.

Acknowledgement of Non-compete Covenant:



A rectangular box with a light gray border. Inside the box, the text "SIGN HERE" is written in large, bold, light gray capital letters. To the left of this text is a small "x" icon. Below the text is a horizontal line. To the right of the line, the word "clear" is written in a small, blue, sans-serif font.

Figure F.3: Page 1 of No Noncompete and Hidden Noncompete Treatments

NON-DISCLOSURE AND WORK AGREEMENT

This Non-Disclosure and Work Agreement (this "Agreement") is made effective as of the "Effective Date", by and between [REDACTED].

Information will be disclosed to the Recipient for assistance with recruiting.

The Company has requested and the Recipient agrees that the Recipient will protect the confidential material and information which may be disclosed between the Company and the Recipient. Therefore, the parties agree as follows:

EFFECTIVE DATE. The effective date shall be determined by the date at which this agreement is signed and agreed to by the Company.

COMPENSATION. The Company will compensate the Recipient as to the amount agreed to on the platform.

Continue to next page

Go to the end and sign

Figure F.4: Page 7 of No Noncompete and Salient Noncompete Treatments

TERM. The obligations of this Agreement shall survive 5 years from the Effective Date or until the Company sends the Recipient written notice releasing the Recipient from this Agreement. After that, the Recipient must continue to protect the Confidential Information that was received during the term of this Agreement from unauthorized use or disclosure for an additional 5 years.

APPLICABLE LAW. This Agreement shall be governed by the laws of the State of Florida.

CONFLICT RESOLUTION. In the event of a dispute between the parties, the parties hereby agree to use the Tallahassee Superior Court as the venue. The parties hereby also agree that the prevailing party shall be entitled to reasonable attorney fees and costs incurred as a result of the dispute.

Continue to next page

Go to the end and sign

Notes: The version of the contract sent from Firm A's account refers to a "Tallahassee Superior Court." The same appears on page 7 of the hidden noncompete (Figure F.5). Florida state trial courts are not generally called superior courts; the relevant state trial court in Tallahassee is the Leon County Circuit Court, part of Florida's Second Judicial Circuit. We believe this wording reflects a jurisdiction-specific editing error in a general contract template. The language is constant across conditions. We have no evidence that any recruiter noticed this wording during the experiment; Figure J.4 suggests that about one-third of subjects skipped this page entirely. The authors noticed the wording only after the experiment, during proofreading.

Figure F.5: Page 7 of Hidden Noncompete Treatment

TERM. The obligations of this Agreement shall survive 5 years from the Effective Date or until the Company sends the Recipient written notice releasing the Recipient from this Agreement. After that, the Recipient must continue to protect the Confidential Information that was received during the term of this Agreement from unauthorized use or disclosure for an additional 5 years.

APPLICABLE LAW. This Agreement shall be governed by the laws of the State of Florida.

NON-COMPETE COVENANT. During the paid engagement with the Company and for a period of 6 months after the separation of the engagement for any reason, Recipient will not directly or indirectly engage in any business that competes with the Company, including but not limited to, businesses 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.

Directly or indirectly engaging in any competitive business includes, but is not limited to: (i) engaging in a business as Company, partner, or agent, (ii) becoming an employee or independent contractor of any third party that is engaged in such business, (iii) becoming interested directly or indirectly in any such business, or (iv) soliciting any customer, applicant, or employee of the Company for the benefit of a third party that is engaged in such business. Recipient agrees that this non-compete agreement will not adversely affect Recipient's livelihood.

CONFLICT RESOLUTION. In the event of a dispute between the parties, the parties hereby agree to use the Tallahassee Superior Court as the venue. The parties hereby also agree that the prevailing party shall be entitled to reasonable attorney fees and costs incurred as a result of the dispute.

Continue to next page

Go to the end and sign

Notes: The version of the contract sent from Firm A's account refers to a "Tallahassee Superior Court." The same appears on page 7 of the salient and no-noncompete conditions (Figure F.4). Florida state trial courts are not generally called superior courts; the relevant state trial court in Tallahassee is the Leon County Circuit Court, part of Florida's Second Judicial Circuit. We believe this wording reflects a jurisdiction-specific editing error in a general contract template. The language is constant across conditions. We have no evidence that any recruiter noticed this wording during the experiment; Figure J.4 suggests that about one-third of subjects skipped this page entirely. The authors noticed the wording only after the experiment, during proofreading.

F.4 Initial Outreach Language

Below is the language used to reach out to candidates for the first time. The highlighted portions indicate randomized components; the noncompete language was included only in the salient-noncompete condition.

Dear [Name],

I looked at your profile and you seemed like a good fit for this job. I wanted to invite you directly and move forward if you're available. If you're interested, the job is yours. Just sign/approve the noncompete agreement and work agreement (link below) and accept the job to get started.

—
We are seeking an HR professional's help reviewing a set of job applications of MBA graduates. We are hiring an investment associate (i.e., a junior investment professional) to join a family-run holding company. The family company has activities in finance, technology and healthcare industries, and is active in philanthropy, publishing, and sustainability/ESG (environmental/social/governance issues for businesses).

We have approximately eight applications for review, and would like some brief feedback about which candidates we should proceed with interviews. We will provide a list of standard questions for you to complete about each candidate (no essay responses). You can work from your home or office and correspond with us virtually.

Pay is hourly (with a maximum of 1 hour allowable for the initial engagement). You should be either based in the US, or familiar with US institutions and culture (such as US schools and employers) so that you can better understand the candidates' qualifications. We ask that you NOT contact the candidate (at least not for now), and instead send us your feedback after reading the job applications before we move to the next stage with the candidates.

Interested HR professionals should:

- 1. Click through to sign our non-compete agreement and work agreement here: [Link]*
- 2. Read our brief instructions doc (attached as a PDF to this message) to learn more about this task.*
- 3. Accept the job.*

Rate: \$60.00/hr [\$25.00/hr]

Limit: 1 hrs/week

F.5 Outreach by Firm B

Below is the language used by Firm B to reach out to workers previously hired by Firm A. The highlighted portions are the randomized components.

Dear [Name],

We saw your profile, and we're interested in hiring you as a freelance HR partner. We have an ongoing need for various HR-related tasks at our company, including help with screening and recruiting candidates. We're in finance, and I saw on your [platform] profile that you did similar work for another finance company and it looks like the engagement went well.

If you're interested, click accept on the job offer below, and we can start sending you tasks. I've set the limit right now for one hour per week, but this can adjust depending on the assignments and your availability.

To express your interest, take a look at our job opening below and send us the resume (or an online profile) for 1-2 good candidates. This will help us assess what kinds of tasks would be good to send you.

—

HELP US HIRE: MBAs from top business schools with 3-5 years of related experience.

JOB OPENING: Finance Professional (Associate Level)

Details below.

About Our Company

We are a leading independent investment firm with headquarters in Florida. Unlike traditional holding companies, we're a partnership-backed investment institution with a focus on cutting-edge sectors like technology, finance, and healthcare. We leverage our global network to uncover the most promising private and public investment opportunities and are proud to uphold a firm-wide commitment to sustainability and thought leadership.

Job Description. *We provide our Investment Associates with a wide range of responsibilities, such as:*

- Identifying, assessing, and formalizing new fund investments and other opportunistic investments.*
- Undertaking due diligence tasks like industry-specific research, valuation, and comparative analysis.*
- Compiling and presenting investment memorandums to our investment committee.*

- *Taking an active part in portfolio management, including asset allocation analysis, risk management, and ongoing investment performance monitoring.*
- *Building and nurturing professional relationships with various industry actors, fund managers, and middlemen*

What We're Looking For. *We value academic excellence and real-world experience in our team members. Our ideal Investment Associate will have*

- *A solid academic record with a MBA from a top-tier business school.*
- *3-5 years of relevant industry experience, preferably in investment banking, institutional family office, endowment/foundation, fund of funds, or investment consultancy.*
- *Financial modeling proficiency and a solid understanding of capital markets.*

Ideal Candidate's Skills. *As a potential member of our team, we hope you'll bring:*

- *Excellent quantitative and analytical abilities.*
- *Exceptional oral and written communication skills.*
- *A self-motivated, go-getter attitude.*
- *Impeccable organizational and time management skills, coupled with attention to detail.*
- *An uncompromising work ethic and high integrity standards.*

We hope you'll consider joining our dedicated team. Together, we can shape the future of investment.

Rate: US \$27.00/hr [\$62.00/hr]

Limit: 1 hrs/week

F.6 Reminder Language

Below is the language sent by Firm A to remind the workers it hired about their contractual obligations.

Dear [Name]: As part of our work together, you agreed both to a noncompete clause as well as to nondisclosure (confidentiality) restrictions. This is a reminder of those obligations.

The noncompete clause prohibits you from working for, or directly or indirectly engaging in, any business that competes with our company for a period of six months. This includes businesses engaged in finance, technology, healthcare, publishing, philanthropy, or sustainability/ESG activities.

The confidentiality restrictions prohibit you from sharing confidential information we shared with you with third parties. This includes disclosing, copying, or using information, documents, or other materials we shared or discussed with you. You can refer to your work agreement for the full language of these restrictions.

*We believe that we can rely on you to comply with these ongoing obligations moving forward. We will monitor your compliance with these requirements. Please note that we take your continuing compliance with the **noncompete**, non-disclosure and confidentiality obligations seriously and expect you to comply with them.*

F.7 Release Language

Below is the language to release workers from their noncompetes. The highlighted portions are sent to the noncompete-treated individuals.

*Dear [Name]: We are writing today to **release you from the noncompete obligations you agreed to in your contract, and to** remind you of your ongoing confidentiality obligations.*

By releasing you from your noncompete obligations, you are now free to work for businesses that compete with our company (including businesses engaged in finance, technology, healthcare, publishing, philanthropy, or sustainability/ESG activities).

As a reminder, the confidentiality restrictions you agreed to prohibit you from sharing confidential information we shared with you. This includes disclosing, copying, or using information, documents, or other materials we shared or discussed with you. You can refer to your work agreement for the full language of these restrictions.

***While we are releasing you from the noncompete agreement, p** [P]lease note that we take your continuing compliance with the confidentiality obligations seriously and expect you to comply with them.*

G Platform Work-History Data and Contract Durations

To better understand our subjects and the effects of our intervention, we collected additional data from the platform we used for payroll. Using this platform, we collected data about our subjects' work engagements before, during, and after the experimental period from other employers on that platform. In Appendix G.1 we describe this data, and in Appendix G.2 we present average contract durations. Later, in Appendix Q, we use this data to study how our randomized noncompete treatments affected our subjects' employment with other firms.

G.1 Data Collection, Coverage, and Validation

The data comes from a structured, API-like interface offered by the platform to developers. This produced a dataset consisting of 176,953 total engagements with 102,053 distinct employer accounts (across our 29K recruiter accounts). We collected this work history for the ≈ 29 K contract recruiters initially identified by Firm A as qualified to be hired (described in Table I). Note that only a randomly selected ≈ 14 K were invited to work for Firm A. However, we collected work histories for the full population of 29K.

This dataset is useful for two reasons. First, we contextualize Firm A and B's tasks among other engagements (Section G.2). Second, we can measure how our noncompete interventions affected engagement with other employers on the platform (outside Firms A and B), which we study in Section Q.

The documentation and customer support around the structured data interface was minimal and/or unclear, and as a result we have extensively tested and validated the data against verifiable benchmarks. This included how our main experiment appeared in the platform's records, and how work history appeared for workers who deleted their accounts.

To the best of our knowledge, our snapshot includes each account's full work history — from its inception on the platform, until two years after our initial experimental treatments. As far as we know the data are comprehensive with one exception: When a worker issues a full, 100% refund for a particular job or task, the record of that task is not visible to us. As far as we can tell, this kind of refund is rare.²⁴ For some of our questions about earnings, it may be appropriate to code refunds as zero (insofar as the worker never enjoyed the payment).

²⁴For the ≈ 2 K recruiters who worked for Firm A, we can audit the level of attempted refunds to Firm A. We found exactly one person who attempted a refund. We expect this recruiter was hoping to avoid being bound by the noncompete by refunding the payment. In our data for this paper we coded this person as having worked for Firm A because they were initially paid, completed the work for Firm A and were given access to Firm A's candidate résumés.

G.2 Contract Durations and Follow-On Work

We can use the platform data described in Appendix G.1 to place Firm A’s work in the context of the typical engagement lengths for our workers with other employers. We present several descriptive statistics and their context below, and conclude with brief discussion at the end.

Average Contract Duration (All). Across the 29K recruiters, the average contract duration is approximately 274 days (≈ 9 months), with a median of 40 days (1.3 months). This average includes both introductory and repeat gigs, but excludes work for Firms A and B (as do all numbers in this section).

The 274-day average includes both initial and follow-on contracts. In the remainder of this section, we break down these contract lengths based on “initial” versus “repeat” contracts between a worker and employer on the platform. Although our data allows us to report these averages based on each worker×employer’s first engagement *on this platform*, this might have been preceded by a separate, off-line engagement that is outside our dataset.

Initial Contracts. For an initial contract between a worker and an employer, the average length is 283 days (median 43 days). To understand the meaning of these numbers, it is important to note that inside each contract, we are not able to see how many separate tasks are included and/or how they are spaced over time. Part of the rationale for the platform’s hourly contracting feature was to allow more open-ended scope than fixed-price contracts for pre-specified tasks.

It is likely that an “initial contract” covers both an introductory task—i.e., a single bundle of tasks requiring a few hours—and a longer set of tasks. If the performance is good on the first task, the recruiter could be given more work as part of the same hourly contract, without starting a new contract. All of this would fall within a single contract.

As such, an average of 283 days of a first-time contract likely includes both introductory/test and ongoing work. As mentioned in Section IV.A, the recruiters in Firm A’s tasks behaved as if this were the case: 97% kept the contract open even after the initial task was complete, and 26% proactively requested the next task (without prompting) as part of the initial hourly contract.

Average and median initial contract durations (283 and 43 days) are slightly longer than the average and median durations across all contracts (274 and 40 days). This pattern reflects a highly skewed distribution driven by two factors. First, a non-trivial number of initial contracts remain open for extended periods, generating very long durations. Second, when an initial contract leads to follow-on work, the work is often split across multiple shorter engagements. In these cases, both the initial contract and subsequent follow-ons tend to be shorter individually, even though their cumulative duration can be substantial.

Non-Initial Contracts. Approximately 14% of initial contracts lead to at least one future contract with the same employer. Among initial contracts that successfully lead to follow-on work, the duration of contracts averages 169 days (median 18 days). Among all follow-on contracts (e.g., a non-initial contract), the cumulative duration of follow-on work for a given employer×recruiter pair is much longer (average 426 days, median 63 days).

Discussion. We raise these numbers to place Firm A and B’s tasks in context. These numbers support the prevalence of extended work relationships rather than purely one-off tasks. This is relevant to our portrayal of the contract as “the start of an ongoing relationship” (Section [IV.A](#)).

H Wage Equivalents: Dichotomous Interpretation

The wage equivalence approach in the main text uses the relationship between a \$1 increase in offered wages and the likelihood of completing Firm A's task. Because Firm A's wages were cross-randomized only at two points (\$25 and \$60 per hour), this approach assumes that the effect of wages on acceptance is linear. Because the experimental design does not include intermediate wage levels, we cannot test this assumption directly. In this appendix, we present a dichotomous way to interpret the wage treatments, and implement it in our data. Rather than assuming a linear relationship between wages and acceptance, we compare responses across discrete \$35 wage differences in appendix tables. This approach effectively treats the wage variation as a binary contrast between "higher" and "lower" wage conditions, avoiding the need to impose a functional form on the wage effect.

Table H.1 reports this analysis. The coefficients on the noncompete variables are identical, but the coefficient on an indicator for offering a wage of \$60/hr is 0.052, indicating a 5.2 percentage point increase in the likelihood of working for Firm A relative to the \$25/hr offer. This coefficient is used in the wage-equivalent analysis. The salient wage equivalent in column (3) indicates, for example, that the 1.8 percentage point drop in completion rates due to the salient noncompete is equivalent to 0.35 of the 5.2 percentage point increase in joining Firm A when they offer \$60/hr rather than \$25/hr.

Table H.2 reports the same analysis for the phase 2 wage equivalents. The substantive results are the same as in Table V, while the wage equivalents are now, as discussed above, interpreted as reflecting the \$35 wage differential between Firm B's high wage offer (\$62/hr) and its low wage offer (\$27/hr).

Table H.1: Phase 1 Contract Opening, Signing, and Firm A Task Completion with a Binary \$25/hr vs. \$60/hr Firm A Wage Treatment

	(1) Opened Contract	(2) Signed Contract	(3) Worked for Firm A
Offered Salient Noncompete	-.079*** (.01)	-.023** (.0092)	-.018** (.0083)
Offered Hidden Noncompete	-.014 (.011)	-.016* (.0092)	-.011 (.0084)
Firm A Wage Offer=60	.063*** (.0085)	.053*** (.0074)	.052*** (.0067)
Weighted	Y	Y	Y
No-Noncompete Mean	0.23	0.15	0.12
Salient - Hidden	-0.07*** (0.01)	-0.007 (0.009)	-0.008 (0.008)
Salient Wage Equivalent	1.26*** (.24)	0.43** (.18)	0.35** (.16)
Hidden Wage Equivalent	0.21 (.17)	0.30* (.18)	0.20 (.16)
Observations	13,851	13,851	13,851
R^2	.026	.021	.02

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models include strata fixed effects and batch fixed effects.

[White-Huber](#) heteroskedasticity-consistent standard errors are reported in parentheses. Salient noncompete means the noncompete was disclosed before contract review and placed on page 1; hidden noncompete means the noncompete appeared on page 7 and was not mentioned in outreach. No-Noncompete Mean reports the mean of the dependent variable for the omitted no-noncompete group within the relevant column sample. Weighted indicates whether inverse-probability weights based on the sampling strata are used. High-wage indicators equal one for the higher randomized wage offer: \$60/hr for Firm A and \$62/hr for Firm B. Wage equivalents are computed as the negative noncompete coefficient divided by the relevant wage-offer coefficient. In the wage-equivalent rows, values are shares of the \$35 high-low wage difference, not dollars per hour. For indicator outcomes, coefficients are changes in probability and can be read as percentage-point effects.

Table H.2: Joining Firm B Before and After Firm A's Reminder with a Binary \$27/hr vs. \$62/hr Firm B Wage Treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	$\mathbb{1}(\text{Accepted Firm B})$							
	Before the Reminder				After the Reminder			
Offered Salient Noncompete	-.0052 (.0072)				-.018*** (.005)			
Offered Hidden Noncompete	-.0014 (.0073)				-.026*** (.0048)			
Signed Salient		.061* (.032)	.02 (.029)			-.12*** (.03)	-.11*** (.027)	
Signed Hidden		.047 (.033)	-.0025 (.028)	.085 (.071)		-.2*** (.028)	-.19*** (.026)	-.12** (.062)
Inverse Mills Ratio (λ)			.19 (.18)				-.15 (.16)	
Sample	Invited	Pop.	Heckman	Skippers	Invited	Pop.	Heckman	Skippers
Estimator	OLS	2SLS	OLS	OLS	OLS	2SLS	OLS	OLS
Weighted	Y	N	N	Y	Y	N	N	Y
No-Noncompete Mean	0.09	0.64	0.64	0.65	0.05	0.39	0.39	0.38
KP RK Wald F		183.58				183.58		
Cragg-Donald Wald F		257.32				257.32		
Heckman: χ^2 (Excl. Inst.)			33.88***				33.88***	
Salient - Hidden	-0.004 (0.007)	0.01 (0.03)	0.02 (0.03)		0.009** (0.004)	0.08*** (0.03)	0.08*** (0.03)	
Salient Wage Equiv.			-0.12 (0.18)				0.84*** (0.24)	
Hidden Wage Equiv.			0.01 (0.17)	-0.52 (0.49)			1.43*** (0.30)	0.57* (0.33)
Observations	13,851	29,005	13,851	314	13,851	29,005	13,851	314
R^2	.019	.57		.37	.019	.25		.48

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are [White-Huber](#) heteroskedasticity-consistent. The dependent variable is an indicator for having an open contract with Firm B. The omitted category is the no-noncompete condition. Columns (1)-(4) report pre-reminder estimates; columns (5)-(8) report post-reminder estimates. Columns (1) and (5) use the invited sample, columns (2) and (6) use the full population, columns (3) and (7) use the Heckman selection correction among workers who completed Firm A's task, while columns (4) and (8) report the skipper specification. All Firm A and B wage controls are treated as indicators for the high wage. Salient noncompete means the noncompete was disclosed before contract review and placed on page 1; hidden noncompete means the noncompete appeared on page 7 and was not mentioned in outreach. Signed Salient and Signed Hidden indicate workers who signed Firm A's contract in the salient- and hidden-noncompete conditions, respectively. No-Noncompete Mean reports the mean of the dependent variable for the omitted no-noncompete group within the relevant column sample. Weighted indicates whether inverse-probability weights based on the sampling strata are used. High-wage indicators equal one for the higher randomized wage offer: \$60/hr for Firm A and \$62/hr for Firm B. Wage equivalents are computed as the negative noncompete coefficient divided by the relevant wage-offer coefficient. In the wage-equivalent rows, values are shares of the \$35 high-low wage difference, not dollars per hour. For indicator outcomes, coefficients are changes in probability and can be read as percentage-point effects. Pre-reminder columns measure outcomes before Firm A reminded workers of their post-employment obligations; post-reminder columns measure outcomes after that reminder. Skippers are workers in the hidden- or no-noncompete conditions who signed Firm A's contract without reading page 7. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. Completed Firm A task uses workers who completed Firm A's task.

I 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 each post-reminder message as positive, negative, or no response. In the no-noncompete group, approximately 13% of responses were coded as negative, while 47% were coded as positive. Table [I.1](#) reports regression results for the salient- and hidden-noncompete groups who finished Firm A’s task (columns 1 and 2) or who were 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.

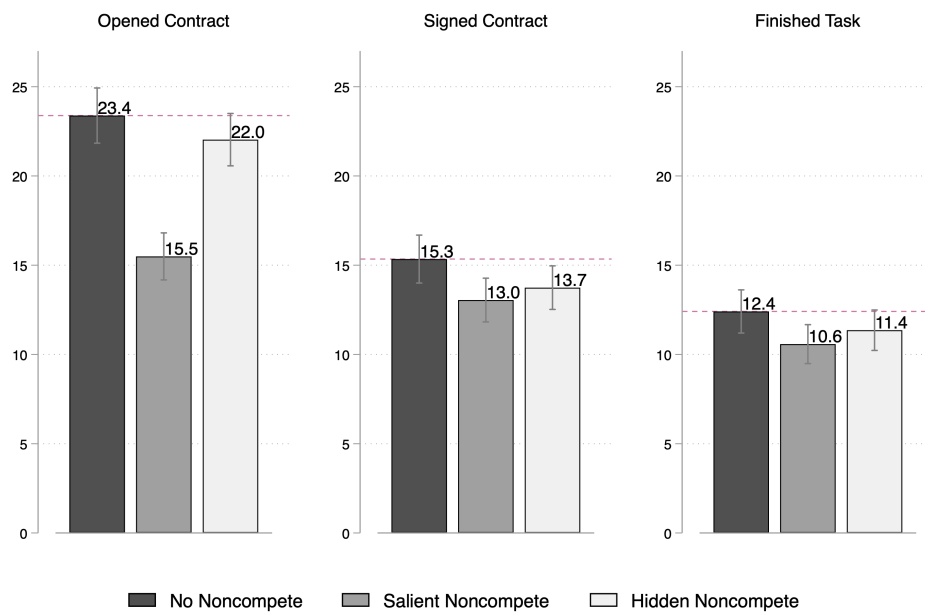
Table I.1: **Post-Reminder Response Sentiment by Firm A Noncompete Status**

	(1) 1(Negative Sentiment)	(2) 1(Positive Sentiment)	(3) 1(Negative Sentiment)	(4) 1(Positive Sentiment)
Signed Salient	.067** (.029)	-.15*** (.037)		
Signed Hidden	.059** (.028)	-.18*** (.035)	.061 (.049)	-.25*** (.065)
Firm B Wage Offer	.00047 (.00065)	-.0005 (.00084)	.003* (.0016)	.00035 (.0017)
Sample	Paid by Firm A	Paid by Firm A	Skippers	Skippers
Weighted	Y	Y	Y	Y
No-Noncompete Mean	0.13	0.47	0.14	0.57
Salient - Hidden	0.008 (0.03)	0.03 (0.03)		
Observations	1,646	1,646	314	314
R^2	.066	.11	.41	.51

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcomes indicate positive or negative sentiment in workers' written responses to Firm A's reminder; no response is coded zero for both. The sample is Firm A task completers, with columns 3-4 limited to page 7 skippers. The omitted group is no noncompete. Models include strata fixed effects, outreach-date fixed effects, and Firm A wage offer; heteroskedasticity-robust standard errors are in parentheses. Weighted uses inverse-probability sampling weights. Indicator coefficients are percentage points.

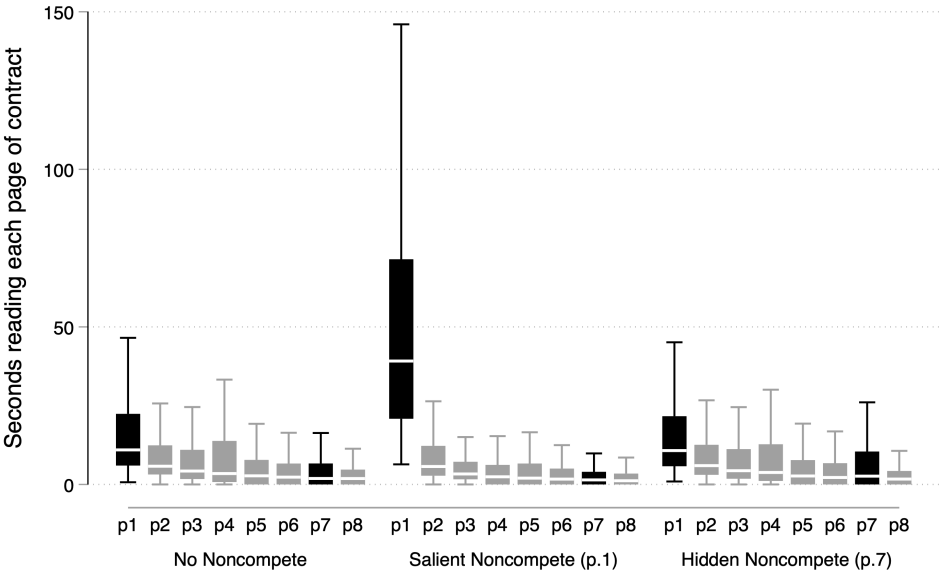
J Supplemental Tables and Figures

Figure J.1: Among those invited, what percentage opened the contract, signed it, and completed the task?



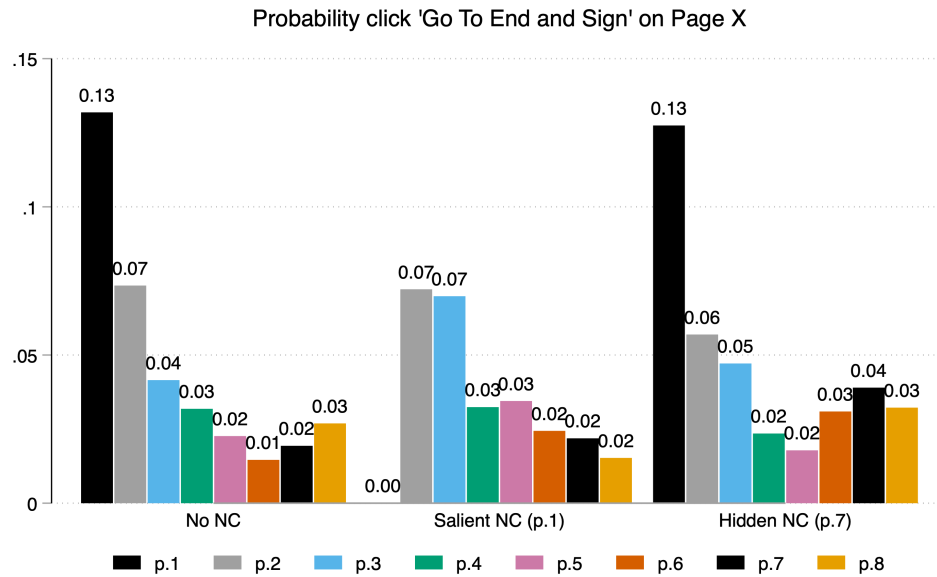
Notes: This figure shows the weighted probabilities in the invited sample of opening the contract, signing the contract, and completing Firm A's task, according to the noncompete treatment condition.

Figure J.2: Box and Whisker Plot of Time Spent Reading Each Page of Contract



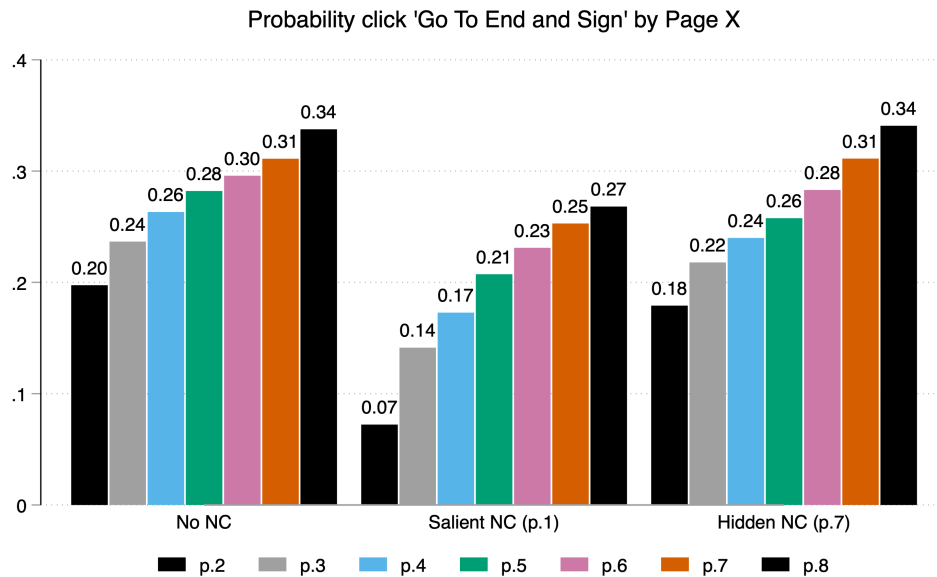
Notes: Page 1 of the Salient noncompete condition is the noncompete page. Page 7 of the Hidden noncompete condition is the noncompete page. Results are weighted. The sample is all those who opened the contract.

Figure J.3: Probability of Skipping on a Given Page



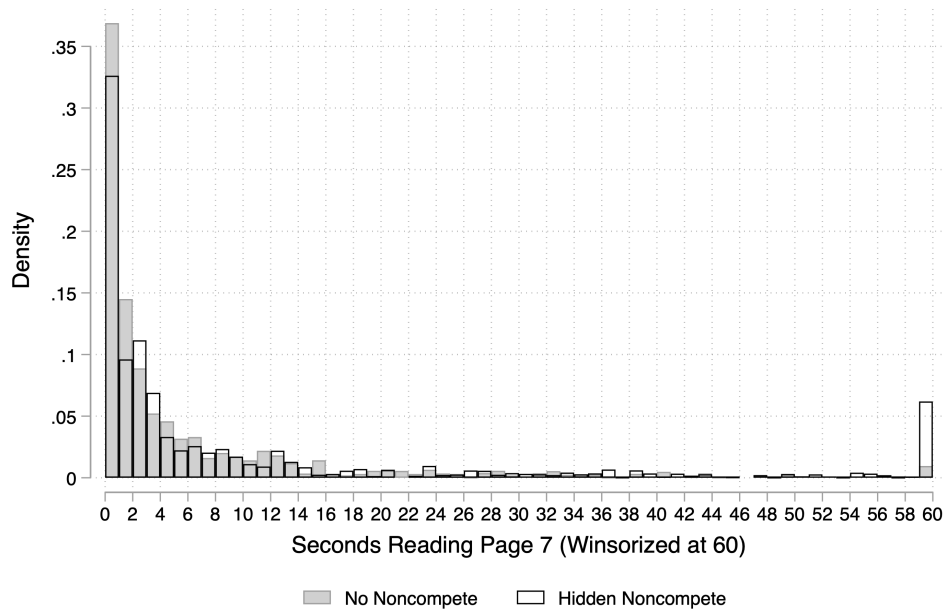
Notes: This figure shows the probability that a worker who opened the contract clicked the skip option on each page. The sample includes those who opened the contract.

Figure J.4: Probability of Skipping On or Before a Given Page



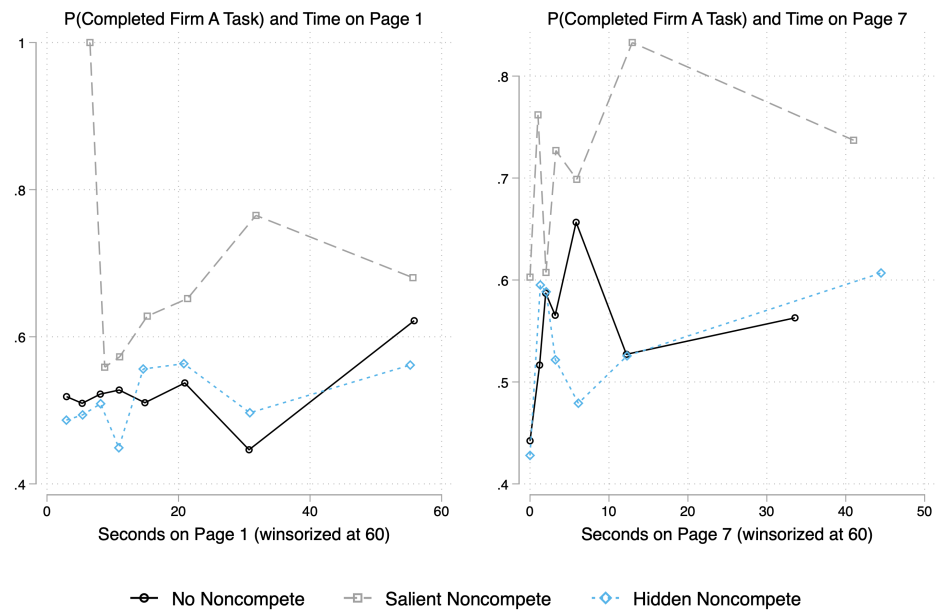
Notes: This figure shows the probability that a worker who opened the contract clicked the skip option on or before each page. The sample includes those who opened the contract.

Figure J.5: Histogram of Time Spent Reading Page 7



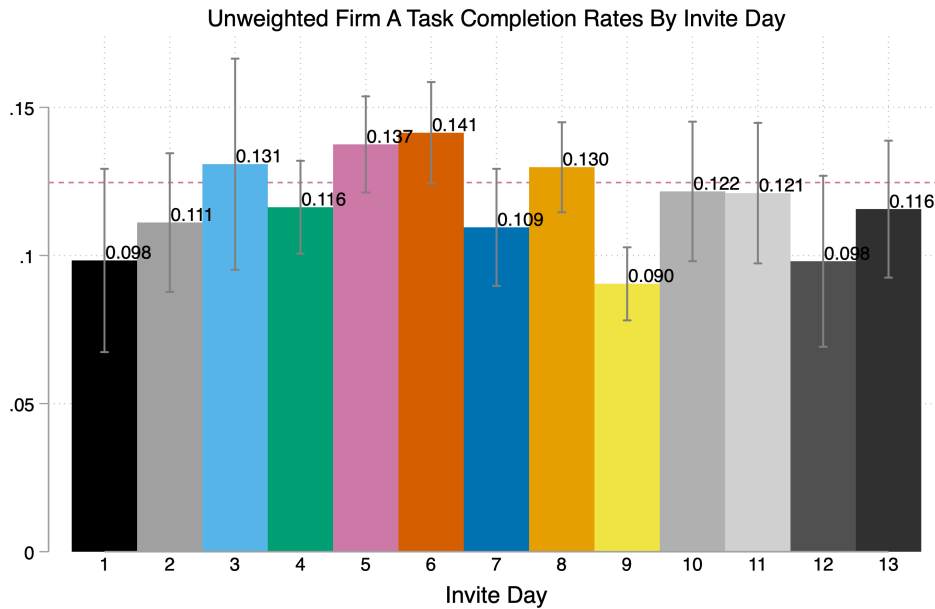
Notes: Results are weighted. Reading variables are winsorized at 60 seconds. The sample is all workers who opened the contract.

Figure J.6: Probability of Completing Firm A Task Given Reading on Pages 1 and 7



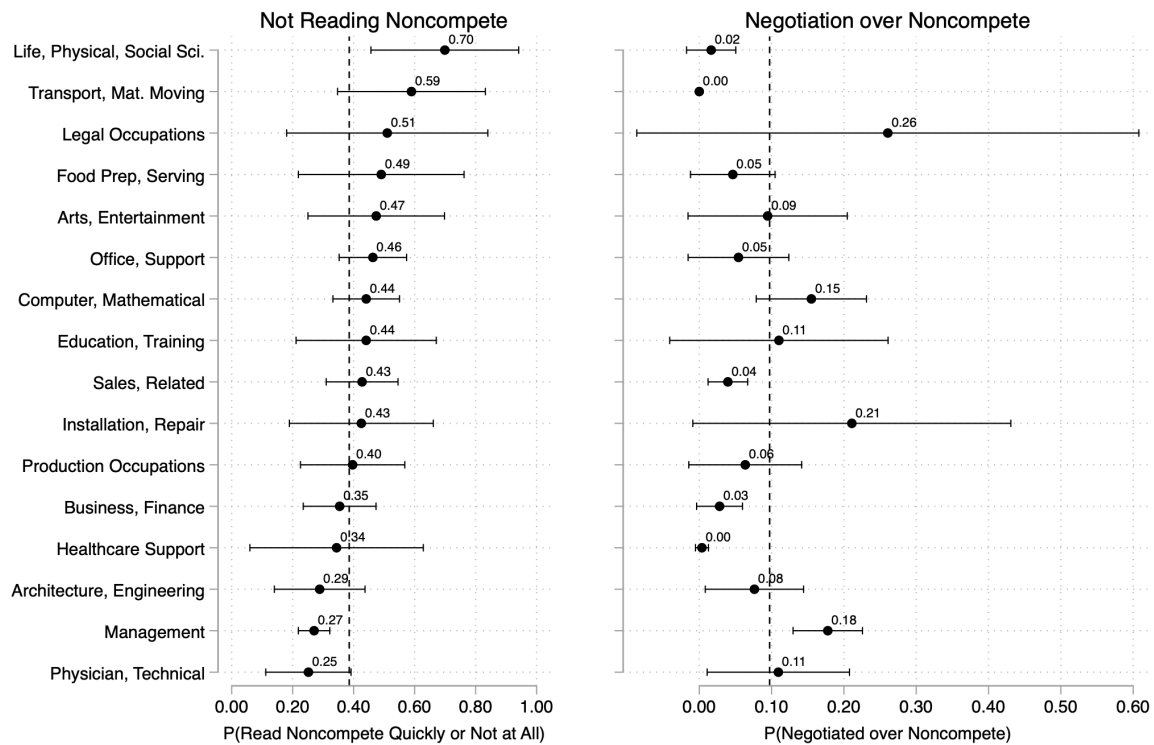
Notes: Reading variables are winsorized at 60 seconds. The sample includes those who opened the contract.

Figure J.7: Variation in Instrumental Variable for Heckman Selection Correction



Notes: This figure shows how Firm A task completion rates vary by randomized invite date. The instrument is the randomized invite date.

Figure J.8: 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, Prescott, and Bishara \(2021\)](#). It shows the occupation-specific (weighted) probability of not reading the noncompete or reading it quickly, 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 J.1: Phase 1 Treatment-Cell Balance in the Full Recruiter Population

	Recruit Experience	Finance Experience	Female	Hourly Rate	Enforcement State	Predicted White
Hidden(25/hour)	0.72	0.48	0.66	51.12	0.70	0.73
Hidden(60/hour)	0.73	0.46	0.66	51.11	0.70	0.73
No Noncompete (25/hour)	0.73	0.46	0.66	52.16	0.70	0.72
No Noncompete (60/hour)	0.73	0.47	0.67	51.94	0.70	0.73
Salient(25/hour)	0.74	0.46	0.65	51.18	0.70	0.72
Salient(60/hour)	0.73	0.47	0.66	51.42	0.70	0.72

Notes: Treatment conditions were assigned before invitation. The table reports pretreatment covariate means across the six Phase 1 cells: noncompete condition by Firm A wage offer. Predicted White is based on a name classifier; Enforcement State indicates that the recruiter's state permitted noncompete enforcement.

Table J.2: Phase 1 Treatment-Cell Balance in the Invited Recruiter Sample

	Recruit Experience	Finance Experience	Female	Hourly Rate	Enforcement State	Predicted White
Hidden(25/hour)	0.75	0.59	0.55	54.73	0.57	0.74
Hidden(60/hour)	0.75	0.59	0.55	54.77	0.57	0.74
No Noncompete (25/hour)	0.76	0.58	0.55	54.89	0.57	0.75
No Noncompete (60/hour)	0.77	0.57	0.56	55.62	0.57	0.75
Salient(25/hour)	0.77	0.57	0.54	54.22	0.57	0.74
Salient(60/hour)	0.76	0.58	0.55	54.88	0.57	0.74

Notes: The table reports pretreatment covariate means in the unweighted invited sample across the six Phase 1 cells: noncompete condition by Firm A wage offer. Predicted White is based on a name classifier; Enforcement State indicates that the recruiter's state permitted noncompete enforcement.

Table J.3: Characteristics of Firm A Task Completers by Noncompete Treatment

	(1) 1(High Asking)	(2) 1(Female)	(3) 1(Predicted White)	(4) 1(Not Enforce Noncompetes)	(5) 1(Recruiter)	(6) 1(Finance)	(7) 1(Recruiter and Finance)
Offered Salient Noncompete	-.074** (.03)	-.017 (.03)	.0077 (.028)	.001 (.03)	-.00066 (.023)	.03 (.03)	.029 (.029)
Offered Hidden Noncompete	.004 (.029)	.028 (.03)	.029 (.027)	.0052 (.03)	-.017 (.023)	.024 (.03)	.0072 (.029)
Weighted No-Noncompete Mean	N 0.54	N 0.66	N 0.69	N 0.30	N 0.80	N 0.42	N 0.21
Salient - Hidden	-0.08*** (0.03)	-0.05 (0.03)	-0.02 (0.03)	-0.004 (0.03)	0.02 (0.02)	0.006 (0.03)	0.02 (0.03)
Observations	1,647	1,647	1,647	1,647	1,647	1,647	1,647
R ²	.0052	.0014	.00071	.000021	.00041	.00069	.00066

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. The unweighted sample is workers who completed Firm A's task; no controls are included. The omitted group is no noncompete. Heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. No-Noncompete Mean is the omitted-group mean. Indicator coefficients are percentage points. High Asking is above-median advertised hourly rate; Noncompetes Not Enforce indicates a state where noncompetes were not enforceable; Predicted White is based on a name classifier.

Table J.4: Characteristics of Firm A Task Completers by Noncompete Treatment and Wage Offer

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A Dependent Variable:</i>	$\mathbb{I}\left(\begin{smallmatrix} \text{High} \\ \text{Asking} \end{smallmatrix}\right)$		$\mathbb{I}(\text{Female})$		$\mathbb{I}\left(\begin{smallmatrix} \text{Predicted} \\ \text{White} \end{smallmatrix}\right)$			
Offered Salient Noncompete	-.13*** (.048)	-.04 (.038)	-.0072 (.048)	-.026 (.038)	.1** (.044)	-.051 (.036)		
Offered Hidden Noncompete	-.023 (.048)	.02 (.037)	-.038 (.048)	.068* (.038)	.051 (.045)	.016 (.034)		
Weighted	N	N	N	N	N	N		
Firm A Wage Offer	25	60	25	60	25	60		
No-Noncompete Mean	0.54	0.54	0.68	0.64	0.63	0.72		
Salient - Hidden	-0.10** (0.05)	-0.06 (0.04)	0.03 (0.05)	-0.09** (0.04)	0.05 (0.04)	-0.07* (0.04)		
Observations	629	1,018	629	1,018	629	1,018		
R ²	.012	.0025	.0011	.0061	.0083	.0037		
<i>Panel B Dependent Variable:</i>	$\mathbb{I}\left(\begin{smallmatrix} \text{Not Enforce} \\ \text{Noncompetes} \end{smallmatrix}\right)$		$\mathbb{I}(\text{Recruiter})$		$\mathbb{I}(\text{Finance})$		$\mathbb{I}\left(\begin{smallmatrix} \text{Recruiter} \\ \text{and Finance} \end{smallmatrix}\right)$	
Offered Salient Noncompete	-.031 (.048)	.021 (.038)	-.0094 (.036)	.0041 (.03)	.034 (.049)	.028 (.038)	.025 (.047)	.032 (.037)
Offered Hidden Noncompete	.015 (.049)	-.00075 (.037)	-.02 (.037)	-.015 (.03)	.023 (.049)	.025 (.038)	.0028 (.047)	.0097 (.036)
Weighted	N	N	N	N	N	N	N	N
Firm A Wage Offer	25	60	25	60	25	60	25	60
No-Noncompete Mean	0.31	0.29	0.84	0.77	0.37	0.44	0.21	0.21
Salient - Hidden	-0.05 (0.05)	0.02 (0.04)	0.01 (0.04)	0.02 (0.03)	0.01 (0.05)	0.002 (0.04)	0.02 (0.05)	0.02 (0.04)
Observations	629	1,018	629	1,018	629	1,018	629	1,018
R ²	.0014	.00041	.00047	.00046	.00083	.00065	.00053	.00074

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. The unweighted sample is workers who completed Firm A's task; no controls are included. The omitted group is no noncompete. Heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Wage offers are dollars per hour. Indicator coefficients are percentage points. High Asking is above-median advertised hourly rate; Noncompetes Not Enforce indicates a state where noncompetes were not enforceable; Predicted White is based on a name classifier.

Table J.5: Phase 1 Effects on Wage Bargaining by Contract-Reading Sample

Sample:	(1) Overall	(2) Opened Contract	(3) > 0 seconds	(4) > 15 seconds
Offered Salient Noncompete	.002 (.0021)	-.0072* (.0042)	-.0075 (.0053)	-.019 (.021)
Offered Hidden Noncompete	.003 (.0023)	.0041 (.0057)	.0014 (.007)	.037* (.021)
Firm A Wage Offer	-.00032*** (.000053)	-.00036** (.00015)	-.00035* (.00019)	-.00088 (.00056)
Weighted No-Noncompete Mean	Y 0.01	Y 0.01	Y .017	Y .004
Observations	13,851	2,895	1,887	367
R ²	.031	.072	.08	.4

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. The outcome is a wage-bargaining indicator. Columns use the samples named in the headings. The omitted group is no noncompete. Models include strata and outreach-date fixed effects; heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Weighted uses inverse-probability sampling weights. Wage offers are dollars per hour; indicator coefficients are percentage points.

Table J.6: Phase 1 Contract Signing by Page 7 Reading Time

Dependent Variable:	(1)	(2)	(3)	(4)	(5)
	1(Signed contract)				
Sample:	Those who spent ___ seconds on p.7				
	0	> 0	> 15	> 30	> 45
Offered Salient Noncompete	.22*** (.05)	.16*** (.027)	.22*** (.08)	.058 (.19)	.45 (.34)
Offered Hidden Noncompete	.0048 (.046)	-.055* (.03)	-.12* (.07)	-.21* (.13)	-.51* (.25)
Firm A Wage Offer	.002* (.0011)	.0012* (.0007)	-.0019 (.0018)	-.0057** (.0027)	.0003 (.005)
Weighted	Y	Y	Y	Y	Y
No-Noncompete Mean	0.53	0.71	0.72	0.67	0.87
Observations	1,005	1,887	367	172	86
R ²	.16	.12	.37	.58	.84

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. The outcome is signing Firm A's contract. Columns restrict to workers with the page 7 reading time shown in the heading. The omitted group is no noncompete. Models include strata and outreach-date fixed effects; heteroskedasticity-robust standard errors are in parentheses. Hidden noncompetes appear on page 7; salient noncompetes appear on page 1. Weighted uses inverse-probability sampling weights. Reading times are seconds; indicator coefficients are percentage points.

Table J.7: Phase 1 Contract Signing by Page 7 Reading-Time Quartile

Dependent Variable:	(1)	(2)	(3)	(4)
	1(Signed Contract)			
Sample:	Quartile of Seconds on p.7			
	1st	2nd	3rd	4th
Offered Salient Noncompete	.24*** (.048)	.19*** (.047)	.11** (.046)	.18*** (.064)
Offered Hidden Noncompete	-.0026 (.046)	.069 (.052)	-.14*** (.05)	-.094* (.057)
Firm A Wage Offer	.0019* (.0011)	.00097 (.0012)	.002* (.0012)	-.0014 (.0016)
Weighted	Y	Y	Y	Y
No-Noncompete Mean	0.54	0.67	0.75	0.70
Min Seconds	0	.74	2.66	10.47
Max Seconds	0	2.63	10.43	3939.09
Observations	1,048	641	680	517
R ²	.16	.25	.24	.27

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. The sample reached page 7; columns are quartiles of page 7 reading time among hidden-noncompete workers, with Min and Max in seconds. The outcome is signing Firm A's contract. The omitted group is no noncompete. Models include strata and outreach-date fixed effects; heteroskedasticity-robust standard errors are in parentheses. Hidden noncompetes appear on page 7; salient noncompetes appear on page 1. Weighted uses inverse-probability sampling weights. Indicator coefficients are percentage points.

Table J.8: Phase 1 Task Completion by Page 7 Reading Time

Dependent Variable:	(1)	(2)	(3)	(4)	(5)
Sample:	1(Completed Firm A Task)				
	Those who spent ___ seconds on p.7				
	0	> 0	> 15	> 30	> 45
Offered Salient Noncompete	.17*** (.052)	.15*** (.036)	.2* (.11)	-.047 (.24)	.59** (.27)
Offered Hidden Noncompete	-.021 (.045)	-.018 (.033)	-.032 (.075)	-.18 (.13)	-.35* (.17)
Firm A Wage Offer	.0022* (.0011)	.0026*** (.00082)	-.003 (.002)	-.0073** (.0032)	-.0018 (.0052)
Weighted	Y	Y	Y	Y	Y
No-Noncompete Mean	0.44	0.57	0.56	0.57	0.78
Observations	1,005	1,887	367	172	86
R ²	.16	.091	.34	.52	.9

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. The outcome is completing Firm A's task. Columns restrict to workers with the page 7 reading time shown in the heading. The omitted group is no noncompete. Models include strata and outreach-date fixed effects; heteroskedasticity-robust standard errors are in parentheses. Hidden noncompetes appear on page 7; salient noncompetes appear on page 1. Weighted uses inverse-probability sampling weights. Reading times are seconds; indicator coefficients are percentage points.

Table J.9: Phase 1 Task Completion by Page 7 Reading-Time Quartile

Dependent Variable:	(1)	(2)	(3)	(4)
	1(Completed Firm A Task)			
Sample:	Quartile of Seconds on p.7			
	1st	2nd	3rd	4th
Offered Salient Noncompete	.18*** (.05)	.18*** (.059)	.059 (.054)	.24*** (.074)
Offered Hidden Noncompete	-.028 (.045)	.092 (.061)	-.15*** (.053)	-.0081 (.06)
Firm A Wage Offer	.0022** (.0011)	.003** (.0015)	.0038*** (.0013)	-.0016 (.0017)
Weighted	Y	Y	Y	Y
No-Noncompete Mean	0.45	0.53	0.62	0.55
Min Seconds	0	.74	2.66	10.47
Max Seconds	0	2.63	10.43	3939.09
Observations	1,048	641	680	517
R ²	.16	.21	.26	.26

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. The sample reached page 7; columns are quartiles of page 7 reading time among hidden-noncompete workers, with Min and Max in seconds. The outcome is completing Firm A's task. The omitted group is no noncompete. Models include strata and outreach-date fixed effects; heteroskedasticity-robust standard errors are in parentheses. Hidden noncompetes appear on page 7; salient noncompetes appear on page 1. Weighted uses inverse-probability sampling weights. Indicator coefficients are percentage points.

Table J.10: 2SLS Model for Phase 2 Outcomes Not Including Firm A Wages, Before and After Reminder

	(1)	(2)	(3)	(4)
Dependent Variable:	1(Accepted Firm B)		1(Shared Firm A Résumés)	
	Before Reminder	After Reminder	Before Reminder	After Reminder
Signed Salient	.062* (.032)	-.12*** (.03)	.0084 (.012)	.0043 (.0058)
Signed Hidden	.047 (.033)	-.2*** (.028)	.017 (.012)	.0022 (.0054)
Sample	Pop.	Pop.	Pop.	Pop.
Estimator	2SLS	2SLS	2SLS	2SLS
Weighted	N	N	N	N
No-Noncompete Mean	0.64	0.39	0.03	0.01
KP RK Wald F	315.27	315.27	315.27	315.27
Cragg-Donald Wald F	373.45	373.45	373.45	373.45
Salient - Hidden	0.02 (0.03)	0.08*** (0.03)	-0.008 (0.01)	0.002 (0.006)
Observations	29,005	29,005	29,005	29,005
R ²	.57	.25	.04	.0083

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. The outcome is displayed in each column heading. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. The omitted group is no noncompete. Columns 1 and 3 are pre-reminder and columns 2 and 4 are post-reminder. These models represent the same 2SLS models as in (4) in the main text, except they exclude a Firm A's wages from the model entirely (in both stages). We find similar results as in (4), likely because of the absence of interaction effects between wages and noncompetes in Firm A takeup (Table II) and the absence of noncompete effects on realized Firm A wages or wage bills (Table IV). Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Robust standard errors are in parentheses. Indicator coefficients are percentage points.

Table J.11: **Post-Reminder Firm B Joining, and Firm B Billing**

	(1)	(2)	(3)	(4)	(5)
Sample	Completed Firm A Task				
	Firm B Wage Offer: \$27/hr		Firm B Wage Offer: \$62/hr		
Dependent Variable	Post-Reminder Accept Firm B			Avg.\$/hr	Avg.\$ Billed
Signed Salient	.0016 (.045)	.013 (.045)	-.18*** (.052)	.013 (1.3)	-.95 (1.2)
Signed Hidden	-.11*** (.037)	-.097*** (.037)	-.26*** (.051)	.94 (1.3)	-2.5** (.97)
Firm B Wage Offer=62	.26*** (.047)				
Signed Salient × Firm B Wage Offer=62	-.18*** (.068)				
Signed Hidden × Firm B Wage Offer=62	-.15** (.061)				
Firm A Wage Offer=60	.012 (.027)	.041 (.034)	.00051 (.043)	.29 (1.1)	-.55 (.96)
Weighted	Y	Y	Y	Y	Y
No-Noncompete Mean	0.36	0.24	0.49	44.47	7.19
Observations	1,646	819	825	1,646	1,646
R ²	.15	.22	.19	.086	.095

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. The sample is Firm A task completers; columns 2-3 split by Firm B wage offer. Columns 1-3 estimate post-reminder Firm B joining; columns 4-5 report Firm B wage offer and total Firm B billing. The omitted group is no noncompete. Models include strata and outreach-date fixed effects; heteroskedasticity-robust standard errors are in parentheses. Signed Salient and Signed Hidden indicate workers who signed those Firm A noncompetes. Weighted uses inverse-probability sampling weights. The high Firm B wage offer is 62 dollars per hour; indicator coefficients are percentage points.

Table J.12: **Contamination-Bias-Adjusted Estimates for Phase 1 and Phase 2 Outcomes**

Panel A: Phase 1 Outcomes

Dependent Variable	(1) 1(Opened Contract)	(2) 1(Signed Contract)	(3) 1(Completed Task)	(4) 1(Bargained)
Offered Salient Noncompete	-.079*** (.01)	-.023** (.0091)	-.018** (.0082)	.002 (.0021)
Offered Hidden Noncompete	-.013 (.011)	-.016* (.0091)	-.011 (.0083)	.003 (.0022)
Weighted	Y	Y	Y	Y
No-Noncompete Mean	0.23	0.15	0.12	0.01
Observations	13,851	13,851	13,851	13,851

Panel B: Phase 2 Outcomes

Dependent Variable	(1) 1(Accepted Firm B)	(2) 1(Signed Contract)	(3) 1(Completed Task)	(4) 1(Bargained)
	Pre-Reminder	Post-Reminder	Pre-Reminder	Post-Reminder
Signed Salient Noncompete	.043 (.035)	-.087*** (.033)	-.0019 (.015)	.006 (.0067)
Signed Hidden Noncompete	.011 (.032)	-.18*** (.031)	.015 (.016)	.011 (.0072)
Weighted	Y	Y	Y	Y
No-Noncompete Mean	0.65	0.36	0.04	0.00
Observations	1,647	1,647	1,647	1,647

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Estimates use the contamination-bias adjustment of Goldsmith-Pinkham et al. Panel A reports Phase 1 invited-sample outcomes; Panel B reports Phase 2 outcomes among Firm A task completers. The omitted group is no noncompete. Heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. No-Noncompete Mean is the omitted-group mean. Weighted uses inverse-probability sampling weights. Indicator coefficients are percentage points. Offered rows use randomized offers; Signed rows use realized contract status.

Table J.13: **Multiple-Hypothesis-Adjusted p-values for Phase 1 and Phase 2 Outcomes**

Panel A: Phase 1 Outcomes for Invited Subsample

Dependent Variable	(1) 1(Opened Contract)	(2) 1(Signed Contract)	(3) 1(Completed Task)	(4) 1(Bargained)
Salient Noncompete	-.079*** (0.000)	-.023** (0.012)	-.018** (0.027)	.002 (0.353)
Hidden Noncompete	-.014 (0.212)	-.016* (0.081)	-.011 (0.208)	.003 (0.184)
Salient Q-Sharpened	[.001]	[.046]	[.069]	[.36]
Salient Romano-Wolf	{.0022}	{.22}	{.31}	{.79}
Hidden Q-Sharpened	[.28]	[.17]	[.28]	[.28]
Hidden Romano-Wolf	{.78}	{.54}	{.78}	{.75}
Observations	13,851	13,851	13,851	13,851
R ²	.026	.021	.02	.031

Panel B: Phase 2 Outcomes for Completed Firm A Task Subsample

Dependent Variable	(1) 1(Accepted Firm B)	(2) 1(Signed Firm A)	(3) 1(Shared Firm A Resumes)	(4) 1(Completed Task)
	Pre-Reminder	Post-Reminder	Pre-Reminder	Post-Reminder
Salient Noncompete	.038 (0.305)	-.089*** (0.010)	-.00041 (0.980)	.0086 (0.223)
Hidden Noncompete	.015 (0.671)	-.18*** (0.000)	.015 (0.377)	.012 (0.116)
Salient Q-Sharpened	[.34]	[.046]	[.48]	[.28]
Salient Romano-Wolf	{.79}	{.2}	{.98}	{.78}
Hidden Q-Sharpened	[.48]	[.001]	[.36]	[.22]
Hidden Romano-Wolf	{.9}	{.013}	{.79}	{.65}
Observations	1,646	1,646	1,646	1,646
R ²	.092	.11	.1	.096

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. Panels A and B report the invited-sample and Firm A-completer specifications, respectively. For each coefficient, parentheses show the main-text p-value, brackets show the Q-sharpened p-value, and braces show the Romano-Wolf adjusted p-value from 10,000 bootstrap replications. Indicator coefficients are percentage points. Offered rows use randomized Firm A offers; Signed rows use realized Firm A contract status.

Table J.14: Randomization-Inference p-values for Phase 1 and Phase 2 Outcomes

Panel A: Phase 1 Outcomes

Dependent Variable	(1) 1(Opened Contract)	(2) 1(Signed Contract)	(3) 1(Completed Task)	(4) 1(Bargained)
Offered Salient Noncompete	-.072*** (.0082)	-.013* (.0072)	-.011 (.0067)	.0025 (.0024)
Offered Hidden Noncompete	.0013 (.0087)	-.0056 (.0073)	-.0067 (.0068)	.0019 (.0023)
Salient RI P-value	0	.03	.062	.242
Hidden RI P-value	.848	.375	.259	.373
Weighted	N	N	N	N
Observations	13,851	13,851	13,851	13,851

Panel B: Phase 2 Outcomes

Dependent Variable	(1) 1(Accepted Firm B)	(2) 1(Signed Contract)	(3) 1(Shared Firm A Resumes)	(4) 1(Bargained)
	Pre-Reminder	Post-Reminder	Pre-Reminder	Post-Reminder
Signed Salient Noncompete	.038 (.029)	-.12*** (.029)	.0058 (.012)	.0011 (.0058)
Signed Hidden Noncompete	.00035 (.028)	-.2*** (.026)	.019 (.012)	.002 (.0054)
Salient RI P-value	.144	0	.625	.838
Hidden RI P-value	.989	0	.101	.717
Weighted	N	N	N	N
Observations	1,647	1,647	1,647	1,647

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panels A and B report the invited-sample and Firm A-completer specifications, respectively. Heteroskedasticity-robust standard errors are in parentheses; randomization-inference p-values from 1,000 replications appear below each regression. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Weighted uses inverse-probability sampling weights. Indicator coefficients are percentage points. Offered rows use randomized offers; Signed rows use realized contract status.

K Phase 2 Specification Details

K.1 Variables and Notation

For Phase 2, we let y_i denote one of two binary outcomes:

1. $\mathbb{1}\{\text{Joined Firm B}_i\}$;
2. $\mathbb{1}\{\text{Shared Firm A's Résumés with Firm B}_i\}$.

In the reduced-form and IV specifications, we define these outcomes on the full experimental sample, so $y_i = 0$ for workers who never become eligible to work for Firm B.²⁵ In the selection-correction specification, we instead treat Phase 2 outcomes as observed only for workers who worked for Firm A. The design-based IV and model-based selection-correction approaches therefore address the same substantive question under different identifying assumptions.

We measure both outcomes before and after Firm A's reminder. For payroll-timing reasons, $\mathbb{1}\{\text{Joined Firm B}_i\}$ is coded based on whether the worker had an open contract with Firm B at the relevant time of measurement. This differs from our Phase 1 implementation of $\mathbb{1}\{\text{Joined Firm A}_i\}$, which is coded as completing Firm A's task and being paid. This timing convention lets us measure Firm B participation separately before and after the reminder, whereas most Firm B payments were processed in a single later payroll batch.

Randomized variables. Let I_i denote whether worker i was sent a job offer by Firm A. Because Firm A invited a random sample from the 29K, I_i is exogenous. Let $O_i^S = \mathbb{1}\{\text{Offered a Salient Noncompete}_i\}$ and $O_i^H = \mathbb{1}\{\text{Offered a Hidden Noncompete}_i\}$. These terms are experimentally randomized, and $O_i^S = 1$ or $O_i^H = 1$ implies $I_i = 1$.

Endogenous variables. Let $P_i = \mathbb{1}\{\text{Joined Firm A}_i\}$, a choice by the worker. Define the realized contract indicators $S_i = P_i O_i^S$ and $H_i = P_i O_i^H$, so that S_i and H_i indicate that worker i signed a salient or hidden noncompete with Firm A and completed Firm A's task. Let $R_i^A = P_i w_i^A$ denote the realized wage for recruiters who accepted a Firm A offer.

Controls. As in Phase 1, $\gamma_{s(i)}$ denotes randomization-stratum fixed effects, $\psi_{t(i)}$ denotes batch fixed effects, and w_i^A and w_i^B denote the wage offers from Firms A and B.

K.2 Full Empirical Specifications

This subsection provides the details omitted from Section V.C.

²⁵For the sharing outcome, the unconditional measure is also zero if the worker is invited by Firm B and either declines or accepts without sharing.

IV specification: additional details. Equation 4 is estimated on the full experimental sample of $\sim 29K$, without weights (since we are using the full population) and with White-Huber heteroskedasticity-consistent standard errors. The corresponding first stage is the linear projection of (P_i, S_i, H_i, R_i^A) on the randomized vector $(I_i, O_i^S, O_i^H, w_i^A)$ and controls. Because batch fixed effects are collinear with I_i , we include randomization-stratum fixed effects $\gamma_{s(i)}$ but omit batch fixed effects. The Kleibergen–Paap rk Wald F -statistic exceeds 180 in the IV specifications reported in Tables V and VI.

Because S_i , H_i and R_i^A are nested within P_i , the coefficient β_1 in Equation 4 is the effect of working for Firm A without a noncompete relative to not working for Firm A at the low wage. The effects of signing a salient or hidden noncompete (at the low wage) relative to not working for Firm A (at the low wage) are $\beta_1 + \beta_2$ and $\beta_1 + \beta_3$, respectively, so β_2 and β_3 are incremental relative to taking Firm A’s job without a noncompete. As with our Phase 1 regressions (2), Equation 4 imposes a linearity assumption. We motivate this based on the results of Table II and the lack of interaction between noncompete use and wage effects. We show a dichotomous version of our IV in Appendix H.

Because invitation status and contract assignment are randomized, the excluded instruments are independent of potential Phase 2 outcomes, conditional on the included fixed effects. Identification therefore requires that these randomizations affect Phase 2 outcomes only through whether the worker enters Firm A and, conditional on entry, which contract is signed there at which wage. Put differently, the exclusion restriction is satisfied here since a worker cannot interact with Firm B absent signing a contract with Firm A. Had we invited all 14K (or 29K) workers to Firm B, this exclusion restriction might have been violated. It might be possible that a job offer containing a noncompete could have affected mobility to Firm B — *even if the worker did not sign the noncompete*. The offer itself might have had an effect through learning or encouragement even if the offer was not signed.

Appendix K.3 discusses the corresponding monotonicity conditions for this multi-endogenous IV design following Bhuller and Sigstad (2024) and reports the empirical test.

Instrumented selection correction. Following DiNardo et al. (2021), we estimate a two-step Heckman (1979) selection model using randomly assigned invite-date indicators as excluded instruments. Phase 2 outcomes are observed only when $P_i = 1$. Completion rates vary sharply by randomly assigned invite date (Figure J.7). Because invite date is collinear with batch fixed effects, we omit batch fixed effects in this specification. The identifying assumption is that, conditional on controls, invite date affects later outcomes with Firm B only through its effect on completion of Firm A’s task.

We model selection into working for Firm A with the latent-index equation

$$P_i^* = \sum_d \pi_d \mathbb{1}\{\text{Invite Date}_i = d\} + \pi_2 O_i^S + \pi_3 O_i^H + \pi_4 w_i^A + \gamma_{s(i)} + u_i, \quad \text{for } I_i = 1, \quad (23)$$

$$P_i = \mathbb{1}\{P_i^* > 0\}, \quad \text{for } I_i = 1. \quad (24)$$

Conditional on $P_i = 1$ (about 1.6K workers), we then estimate

$$y_i = \beta_1 S_i + \beta_2 H_i + \beta_3 R_i^A + \beta_4 w_i^B + \gamma_{s(i)} + \varepsilon_i, \quad \text{observed only if } P_i = 1. \quad (25)$$

Because Firm B’s wage w_i^B is realized only after a worker completes Firm A’s task, it enters the outcome equation but not the selection equation. Under the standard Heckman assumptions, we estimate Equation (23) by probit, construct the inverse Mills ratio $\hat{\lambda}_i$, and then estimate

$$y_i = \beta_1 S_i + \beta_2 H_i + \beta_3 R_i^A + \beta_4 w_i^B + \gamma_{s(i)} + \rho \sigma_\varepsilon \hat{\lambda}_i + v_i, \quad \text{for } P_i = 1, \quad (26)$$

without weights and with [White-Huber](#) heteroskedasticity-consistent standard errors (incorporating the uncertainty of the inverse Mills term). After correcting for selection into Phase 2, β_1 and β_2 compare outcomes for workers who signed a salient or hidden noncompete at Firm A relative to workers who completed Firm A without a noncompete. Per [DiNardo et al. \(2021\)](#), we interpret this specification as a “gettable” average treatment effect.

Skipper/reminder specification. In our final approach, 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. These workers may still be selected in other ways, but they could not have selected into the hidden noncompete by observing page 7 itself.

Skippers learn about their noncompete status only when Firm A sends the reminder. We therefore restrict attention to workers in the hidden or no-noncompete conditions who signed the contract and skipped page 7 entirely, and estimate

$$y_i = \beta_1 H_i + \beta_2 R_i^A + \beta_3 w_i^B + \gamma_{s(i)} + \psi_{t(i)} + \varepsilon_i, \quad \text{for } P_i = 1, S_i = 0, \text{ and } D_i = 1, \quad (27)$$

where $D_i = \mathbb{1}\{\text{Skipped Page 7}\}$. For outcomes realized after the reminder, this specification estimates a skipper-specific treatment effect of learning one had signed a hidden noncompete. For outcomes realized before the reminder, it yields a placebo test: absent advance knowledge of hidden noncompete status, behavior should be indistinguishable across the hidden and no-noncompete groups.

K.3 Monotonicity Conditions for 2SLS with Multiple Endogenous Margins

In a just-identified multivariate 2SLS model, [Bhuller and Sigstad \(2024\)](#) show that, in addition to the usual exclusion, independence, and rank conditions, two further restrictions are necessary and sufficient for 2SLS to assign proper nonnegative weights to margin-specific causal effects: *average conditional monotonicity* and *no cross effects*. We first define these conditions. We then show how an equivalent reparameterization of our experimental

design lines up with them. Finally, we implement the empirical diagnostic implied by [Bhuller and Sigstad \(2024\)](#) for the baseline nested specification in Equation (4).

In this subsection, an *endogenous margin* means one of the endogenous variables in (4): working for Firm A at all (P_i), working for Firm A under a salient noncompete (S_i), working for Firm A under a hidden noncompete (H_i), or the realized Firm A wage margin ($R_i^A = P_i w_i^A$). A latent response type is a worker's complete set of potential choices across the possible offer arms and Firm A wage offers. The issue is not whether the randomized instruments are valid. Rather, the issue is whether the 2SLS coefficient on a given margin can be interpreted as a positively weighted average of causal effects for that same margin, rather than as an object that puts negative weight on some response types or also loads on causal effects of other margins.

Average conditional monotonicity. For a given endogenous margin k , average conditional monotonicity is the *own-margin* condition. After removing the linear association with the fitted values for the other endogenous margins, the fitted value for margin k should not move any latent response type away from margin k on average. Equivalently, within each latent response type, the partial first-stage relationship between predicted take-up of margin k and actual take-up of margin k should be weakly positive. In our setting, for example, the fitted salient margin should not, after netting out the fitted P_i , H_i , and R_i^A margins, be negatively related to signing a salient noncompete. This condition is weaker than pointwise monotonicity: it allows local reversals across instrument values, as long as the average partial relationship for that margin is not negative for any latent response type.

No cross effects. No cross effects is the *other-margin* condition. After isolating the first-stage variation used for margin k , that variation should not also change any other margin $\ell \neq k$ on average. Equivalently, the variation used to identify the coefficient on S_i should not also identify effects of P_i , H_i , or R_i^A , and similarly for the other margins. When this condition fails, the coefficient on one endogenous margin can be contaminated by causal effects of another endogenous margin. When it holds, each multivariate 2SLS coefficient loads only on its own endogenous margin.

Thus, average conditional monotonicity asks whether the own partial first stage has the right sign; no cross effects asks whether the cross partial first stages are zero.

Equivalent reparameterization. The design-based argument is clearest after an equivalent reparameterization. Instead of the nested contract-status variables (P_i, S_i, H_i), define the three mutually exclusive Firm A contract states

$$D_i^\emptyset = P_i(1 - O_i^S - O_i^H), \quad D_i^S = P_i O_i^S = S_i, \quad D_i^H = P_i O_i^H = H_i.$$

The fourth endogenous variable is the realized Firm A wage margin,

$$R_i^A = w_i^A(D_i^\emptyset + D_i^S + D_i^H) = P_i w_i^A.$$

The omitted state is not working for Firm A. Similarly, define mutually exclusive offer-arm instruments

$$Z_i^\emptyset = I_i(1 - O_i^S - O_i^H), \quad Z_i^S = O_i^S, \quad Z_i^H = O_i^H,$$

with non-invitation as the omitted offer arm, and let $Z_i^R = w_i^A$ denote the wage-offer instrument. Together, these variables are algebraically equivalent to the baseline endogenous and excluded-instrument vectors. Under this representation, each non-wage offer arm can generate only its corresponding contract state or nonparticipation. For example, a salient offer can generate $D_i^S = 1$ or no Firm A work, but it cannot generate $D_i^\emptyset = 1$ or $D_i^H = 1$. The wage-offer instrument shifts the realized wage margin R_i^A only through workers who work for Firm A, since $R_i^A = 0$ when $P_i = 0$.

Relation to the baseline specification. Table K.1 implements this reparameterization and verifies that it is algebraically equivalent to the baseline IV after including the realized wage margin R_i^A . The reparameterized specification estimates the three mutually exclusive Firm A contract states, along with R_i^A , relative to not working for Firm A. The baseline specification in (4) instead uses the nested variables (P_i, S_i, H_i, R_i^A) : β_1 captures working for Firm A without a noncompete relative to not working for Firm A at the low wage, while β_2 , β_3 , and β_4 are increments for salient noncompetes, hidden noncompetes, and the realized Firm A wage margin relative to that benchmark. The economic contrasts are therefore the same across the two parameterizations. In Table K.1, the salient-versus-no-noncompete and hidden-versus-no-noncompete effects are obtained by subtracting the coefficient on the no-noncompete Firm A state from the coefficients on the salient and hidden states, while the coefficient on R_i^A is unchanged. The bottom rows report these implied contrasts and match the corresponding estimates in Tables V and VI.

The reparameterized model gives the clean design-based intuition. Our main specification, however, uses the nested variables (P_i, S_i, H_i, R_i^A) . For those variables, the conditions are not automatic because the same randomized offer or wage offer can affect more than one nested margin. For example, a salient noncompete offer affects the salient signing margin S_i , but it can also reduce the extensive margin P_i by causing some workers not to work for Firm A at all. Similarly, a higher Firm A wage offer mechanically increases R_i^A for workers who join Firm A and may also in theory affect P_i , S_i , and H_i by changing acceptance. We therefore complement the reparameterization with the empirical diagnostic proposed by Bhuller and Sigstad (2024) for the nested first stage used in (4).

Empirical diagnostic in our setting. We assess average conditional monotonicity and no cross effects using the first-stage diagnostic implied by the multivariate 2SLS framework.

Let $(\hat{P}_i, \hat{S}_i, \hat{H}_i, \hat{R}_i^A)$ denote the fitted values from the baseline first-stage system, estimated on the full sample using the same excluded instruments and included controls as in the main IV specification. We then form subsamples using worker characteristics measured

Table K.1: IV Estimates with Mutually Exclusive Firm A Contract States and Realized Wage Margin for Phase 2 Outcomes

	(1)	(2)	(3)	(4)
	Joined Firm B (Pre)	Joined Firm B (Post)	Shared Firm A's Résumés (Pre)	Shared Firm A's Résumés (Post)
Signed No-Noncompete	.7*** (.048)	.42*** (.044)	.043*** (.016)	.016** (.008)
Signed Salient	.76*** (.049)	.3*** (.044)	.051*** (.017)	.02** (.0098)
Signed Hidden	.74*** (.05)	.22*** (.041)	.059*** (.018)	.018** (.0078)
Realized Firm A Wage	-.00037 (.00081)	-.00052 (.00072)	-.00019 (.00029)	-.00022 (.00014)
Sample	Pop.	Pop.	Pop.	Pop.
Estimator	2SLS	2SLS	2SLS	2SLS
Weighted	N	N	N	N
No-Noncompete Mean	0.64	0.39	0.03	0.01
KP RK Wald F	183.58	183.58	183.58	183.58
Cragg-Donald Wald F	257.32	257.32	257.32	257.32
Salient - No Noncompete	0.06* (0.03)	-0.12*** (0.03)	0.008 (0.01)	0.004 (0.006)
Hidden - No Noncompete	0.05 (0.03)	-0.20*** (0.03)	0.02 (0.01)	0.002 (0.005)
Salient - Hidden	0.01 (0.03)	0.08*** (0.03)	-0.008 (0.01)	0.002 (0.006)
Observations	29,005	29,005	29,005	29,005
R ²	.57	.25	.041	.01

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. The estimator is 2SLS: mutually exclusive Firm A contract states and the realized Firm A wage margin R_i^A are instrumented with the corresponding randomized offer arms and the Firm A wage offer w_i^A ; the omitted state is not working for Firm A. The Salient - No Noncompete and Hidden - No Noncompete rows subtract the no-noncompete state coefficient. Specifications match the baseline IV controls. Heteroskedasticity-robust standard errors are in parentheses. Indicator coefficients are percentage points.

before randomization and, within each subsample, estimate

$$\begin{aligned}
 P_i &= \alpha_P + \pi_{PP}\hat{P}_i + \pi_{PS}\hat{S}_i + \pi_{PH}\hat{H}_i + \pi_{PR}\hat{R}_i^A + u_{Pi}, \\
 S_i &= \alpha_S + \pi_{SP}\hat{P}_i + \pi_{SS}\hat{S}_i + \pi_{SH}\hat{H}_i + \pi_{SR}\hat{R}_i^A + u_{Si}, \\
 H_i &= \alpha_H + \pi_{HP}\hat{P}_i + \pi_{HS}\hat{S}_i + \pi_{HH}\hat{H}_i + \pi_{HR}\hat{R}_i^A + u_{Hi}, \\
 R_i^A &= \alpha_R + \pi_{RP}\hat{P}_i + \pi_{RS}\hat{S}_i + \pi_{RH}\hat{H}_i + \pi_{RR}\hat{R}_i^A + u_{Ri},
 \end{aligned}$$

using the same included controls as in the baseline first stage. The diagonal coefficients are the empirical analogue of average conditional monotonicity: each fitted margin should predict its own realized margin with a weakly positive sign. The off-diagonal coefficients are the empirical analogue of no cross effects: a fitted margin for one endogenous margin should not predict another realized margin after the other fitted margins are included. Equivalently, the 4×4 coefficient matrix should be diagonal with weakly nonnegative diagonal entries. Rejection of the sign restrictions on the diagonal entries is evidence against average conditional monotonicity. Rejection of the zero restrictions on the off-diagonal entries is evidence against no cross effects. Failure to reject should be interpreted more modestly as evidence that the observed subsamples are consistent with the assumptions. The full-sample analogue is mechanically satisfied by construction of the fitted values, so the informative evidence comes from predetermined subsamples rather than from the pooled sample.

Our results are in Tables K.2 and K.3, which split the four realized margins across two tables for readability. Across predetermined subsamples, all own coefficients for the four margins are positive. Most are near one, although the extensive-margin coefficients for P_i are somewhat more variable. The one-sided tests therefore provide no evidence of violations of the nonnegative own-coefficient restrictions. The row-level off-diagonal tests also provide little evidence of cross-margin prediction after multiple-testing adjustment: while a few unadjusted off-diagonal tests are small in hourly-rate quartile subsamples, none of the Holm-adjusted tests rejects the zero restrictions on the off-diagonal coefficients. This evidence is diagnostic rather than dispositive: it cannot prove assumptions that are stated over latent response types. It does, however, show that the observed first-stage behavior has the pattern the assumptions require: each predicted margin, including the realized wage margin, predicts its own realized margin in the expected direction, with no Holm-adjusted evidence that it also predicts other realized margins.

The tables should therefore be read as diagnostics of the first stage. The diagonal entries ask whether each predicted margin, including R_i^A , moves its own realized margin in the expected direction. The off-diagonal tests ask whether the predicted movement in the other three margins jointly predicts the realized margin in that row, after the row's own fitted margin and the included controls are held fixed.

Table K.2: First-Stage Diagnostic for Four Endogenous Margins: Joining and Salient Noncompete Margins

Panel A. P_i : joined Firm A

Subsample	Own coef.	s.e.	Own viol. p	Holm p	Off-diag. p	Holm p
Male = 0	1.154	(0.108)	> .999	> .999	0.4662	> .999
Male = 1	0.700	(0.129)	> .999	> .999	0.1994	> .999
Minority recruiter = 0	0.634	(0.138)	> .999	> .999	0.8240	> .999
Minority recruiter = 1	1.162	(0.102)	> .999	> .999	0.9290	> .999
Non-enforce state = 0	1.010	(0.107)	> .999	> .999	0.9989	> .999
Non-enforce state = 1	0.980	(0.134)	> .999	> .999	0.9981	> .999
Hourly-rate quartile = 1	1.095	(0.169)	> .999	> .999	0.0543	> .999
Hourly-rate quartile = 2	1.289	(0.186)	> .999	> .999	0.5803	> .999
Hourly-rate quartile = 3	1.169	(0.169)	> .999	> .999	0.6648	> .999
Hourly-rate quartile = 4	0.501	(0.146)	> .999	> .999	0.0182	> .999
Qualified profile = 2	1.020	(0.096)	> .999	> .999	0.9317	> .999
Qualified profile = 3	1.100	(0.188)	> .999	> .999	0.8654	> .999
Active on platform = 0	0.921	(0.107)	> .999	> .999	0.4768	> .999
Active on platform = 1	1.137	(0.134)	> .999	> .999	0.3644	> .999

Panel B. S_i : signed salient

Subsample	Own coef.	s.e.	Own viol. p	Holm p	Off-diag. p	Holm p
Male = 0	0.977	(0.052)	> .999	> .999	0.5419	> .999
Male = 1	1.035	(0.065)	> .999	> .999	0.3558	> .999
Minority recruiter = 0	0.743	(0.074)	> .999	> .999	0.9631	> .999
Minority recruiter = 1	1.082	(0.048)	> .999	> .999	0.9070	> .999
Non-enforce state = 0	1.003	(0.054)	> .999	> .999	0.9368	> .999
Non-enforce state = 1	0.996	(0.062)	> .999	> .999	0.9024	> .999
Hourly-rate quartile = 1	1.174	(0.086)	> .999	> .999	0.9432	> .999
Hourly-rate quartile = 2	1.006	(0.089)	> .999	> .999	0.8321	> .999
Hourly-rate quartile = 3	0.980	(0.079)	> .999	> .999	0.9847	> .999
Hourly-rate quartile = 4	0.844	(0.074)	> .999	> .999	0.6618	> .999
Qualified profile = 2	0.920	(0.048)	> .999	> .999	0.4168	> .999
Qualified profile = 3	1.160	(0.075)	> .999	> .999	0.9979	> .999
Active on platform = 0	0.953	(0.054)	> .999	> .999	0.9753	> .999
Active on platform = 1	1.060	(0.063)	> .999	> .999	0.9833	> .999

Notes: Tables K.2 and K.3 jointly report the Bhuller–Sigstad first-stage diagnostic for the updated four-margin IV specification. The endogenous margins are joining Firm A (P_i), signing a salient noncompete (S_i), signing a hidden noncompete (H_i), and the realized Firm A wage margin ($R_i^A = P_i w_i^A$). The tables are organized by realized margin. Rows report predetermined subsamples defined by gender, minority-recruiter status, noncompete-enforcement state, hourly-rate quartile, qualification profile, and platform-activity status; all nonmissing values of each listed subgroup variable are included. Full-sample fitted values are constructed from first-stage regressions on the excluded instruments (I_i, O_i^S, O_i^H, w_i^A) and the included controls used in the baseline IV. Within each predetermined subsample, each realized margin is regressed on all four fitted margins and the same included controls. Own viol. p is the one-sided p-value for a violation of the nonnegative own-coefficient restriction; values that round to one are displayed as > .999. Off-diag. p is the joint test p-value that all off-diagonal fitted-margin coefficients equal zero in that row. Holm p reports Holm-adjusted p-values computed within the own-coefficient and off-diagonal test families across the rows in both tables. Heteroskedasticity-robust standard errors are used.

Table K.3: First-Stage Diagnostic for Four Endogenous Margins: Hidden Noncompete and Realized Wage Margins

Panel C. H_i : signed hidden

Subsample	Own coef.	s.e.	Own viol. p	Holm p	Off-diag. p	Holm p
Male = 0	1.039	(0.053)	> .999	> .999	0.8216	> .999
Male = 1	0.940	(0.062)	> .999	> .999	0.8496	> .999
Minority recruiter = 0	0.771	(0.072)	> .999	> .999	0.8692	> .999
Minority recruiter = 1	1.078	(0.048)	> .999	> .999	0.9046	> .999
Non-enforce state = 0	0.994	(0.053)	> .999	> .999	0.9807	> .999
Non-enforce state = 1	1.008	(0.061)	> .999	> .999	0.9708	> .999
Hourly-rate quartile = 1	0.960	(0.078)	> .999	> .999	0.5302	> .999
Hourly-rate quartile = 2	1.055	(0.089)	> .999	> .999	0.8170	> .999
Hourly-rate quartile = 3	1.088	(0.081)	> .999	> .999	> .999	> .999
Hourly-rate quartile = 4	0.907	(0.075)	> .999	> .999	0.1987	> .999
Qualified profile = 2	0.953	(0.048)	> .999	> .999	0.6040	> .999
Qualified profile = 3	1.092	(0.072)	> .999	> .999	0.9737	> .999
Active on platform = 0	1.043	(0.055)	> .999	> .999	0.6239	> .999
Active on platform = 1	0.945	(0.059)	> .999	> .999	0.6416	> .999

Panel D. R_i^A : realized wage

Subsample	Own coef.	s.e.	Own viol. p	Holm p	Off-diag. p	Holm p
Male = 0	0.925	(0.075)	> .999	> .999	0.3180	> .999
Male = 1	1.131	(0.094)	> .999	> .999	0.1229	> .999
Minority recruiter = 0	0.823	(0.103)	> .999	> .999	0.9383	> .999
Minority recruiter = 1	1.037	(0.070)	> .999	> .999	0.8638	> .999
Non-enforce state = 0	0.995	(0.076)	> .999	> .999	0.9940	> .999
Non-enforce state = 1	1.013	(0.091)	> .999	> .999	0.9828	> .999
Hourly-rate quartile = 1	0.877	(0.115)	> .999	> .999	0.0446	> .999
Hourly-rate quartile = 2	0.914	(0.127)	> .999	> .999	0.3921	> .999
Hourly-rate quartile = 3	0.981	(0.115)	> .999	> .999	0.7970	> .999
Hourly-rate quartile = 4	1.208	(0.112)	> .999	> .999	0.0157	0.8786
Qualified profile = 2	0.922	(0.068)	> .999	> .999	0.6633	> .999
Qualified profile = 3	1.112	(0.116)	> .999	> .999	0.8775	> .999
Active on platform = 0	0.982	(0.077)	> .999	> .999	0.7240	> .999
Active on platform = 1	1.007	(0.090)	> .999	> .999	0.5699	> .999

Notes: Tables K.2 and K.3 jointly report the Bhuller–Sigstad first-stage diagnostic for the updated four-margin IV specification. The endogenous margins are joining Firm A (P_i), signing a salient noncompete (S_i), signing a hidden noncompete (H_i), and the realized Firm A wage margin ($R_i^A = P_i w_i^A$). The tables are organized by realized margin. Rows report predetermined subsamples defined by gender, minority-recruiter status, noncompete-enforcement state, hourly-rate quartile, qualification profile, and platform-activity status; all nonmissing values of each listed subgroup variable are included. Full-sample fitted values are constructed from first-stage regressions on the excluded instruments (I_i, O_i^S, O_i^H, w_i^A) and the included controls used in the baseline IV. Within each predetermined subsample, each realized margin is regressed on all four fitted margins and the same included controls. Own viol. p is the one-sided p-value for a violation of the nonnegative own-coefficient restriction; values that round to one are displayed as > .999. Off-diag. p is the joint test p-value that all off-diagonal fitted-margin coefficients equal zero in that row. Holm p reports Holm-adjusted p-values computed within the own-coefficient and off-diagonal test families across the rows in both tables. Heteroskedasticity-robust standard errors are used.

L Job Performance

In this section, we examine how randomly requiring noncompetes in a contract affects performance on Firm A's work. Although not formally part of our theoretical framework, noncompetes could affect the quantity or quality of work by selecting the workers willing to accept the job. There may also be a treatment effect of 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, or submitting implausibly low salary offers. Workers who did not complete Firm A's job were coded as a zero for these variables.

We estimate Equation (2) to understand the effect of requiring a noncompete on subsequent performance, coding workers with zeros if they did not work for Firm A.²⁶ We also use the 2SLS and Heckman models designed to estimate the effect of signing a noncompete, described in detail in Section V.C.

The results, shown in Table L.1, suggest little impact of noncompetes on task performance relative to no noncompete, with the exception that requiring a salient noncompete reduces the length of the notes by 19.6%. 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 for a difference relative to the no-noncompete condition, it is not clear how to interpret these differentials.

²⁶Note that for the 'mistakes' variable, this coding choice makes it difficult to interpret the estimates from Equation (2), because zero mistakes coincides with not completing the review. Thus, estimates from this specification appear in column (4) of Table L.1, which we show for completeness, but are not directly interpretable.

Table L.1: Phase 1 Noncompete Effects on Firm A Task Performance

	(1) Num Characters in Notes	(2) Num Characters in Notes	(3) Num Characters in Notes	(4) Mistakes	(5) Mistakes	(6) Mistakes
Offered Salient Noncompete	-25* (13)			-.04 (.079)		
Offered Hidden Noncompete	7.9 (15)			-.1 (.069)		
Firm A Wage Offer	2.2*** (.33)			.0038** (.0016)		
Signed Salient		-151 (94)	-140 (96)		.43 (.39)	.42 (.38)
Signed Hidden		35 (98)	42 (92)		-.13 (.34)	-.12 (.37)
Firm A Realized Wage		5.5** (2.2)	2.6 (4.5)		-.012 (.0094)	-.02 (.018)
Inverse Mills Ratio (λ)			-67 (577)			-1.8 (2.3)
Sample	Invited	Pop.	Paid by Firm A	Invited	Pop.	Paid by Firm A
Estimator	OLS	2SLS	Heck- man	OLS	2SLS	Heck- man
Weighted	Y	N	N	Y	N	N
No-Noncompete Mean	127.30	1135.26	1135.26	0.50	3.75	3.75
KP RK Wald F		183.58			183.58	
Cragg-Donald Wald F		257.32			257.32	
Heckman: χ^2 (Excl. Inst.)			33.88***			33.88***
Salient - Hidden	-33.34** (14.48)	-185.73** (87.07)	-181.87* (94.11)	0.06 (0.06)	0.57 (0.38)	0.54 (0.38)
Salient Wage Equiv.	11.79* (6.27)	27.55 (20.05)	54.56 (95.44)	10.48 (19.94)	36.02 (42.93)	20.82 (23.45)
Hidden Wage Equiv.	-3.69 (6.89)	-6.39 (18.34)	-16.27 (48.68)	27.28 (19.66)	-11.23 (29.30)	-5.80 (19.48)
Observations	13,851	29,005	13,851	13,851	29,005	13,851
R^2	.015	.31		.011	.27	

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcomes are the note-character count and mistake count from Firm A's résumé-screening task; non-completers are coded zero in invited-sample and full-population specifications. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. The omitted group is no noncompete. Models include strata and batch fixed effects except for the Heckman specification; heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Weighted uses inverse-probability sampling weights. Wage offers and wage equivalents are dollars per hour. Note, the no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample.

M 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 III. Prior research has found, for example, that banning noncompetes was especially beneficial for women and racial minorities (Lipsitz and Starr, 2022; Johnson, Lavetti, and Lipsitz, 2023), suggesting that we might expect that such groups may be particularly averse to accepting jobs with noncompetes *ex ante*. Other research has questioned how workers might see noncompetes when their states do not enforce noncompetes (Starr, Prescott, and Bishara, 2020). To examine these ideas, we limit our sample based on various *ex ante* characteristics and then re-estimate Equation (2).

Figure M.1 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 exhibit no aversion to the salient noncompete, while women are 2.5 percentage points less likely to complete Firm A's task under the salient-noncompete condition. Similarly, workers with high asking rates are 2.6 percentage points less likely to accept Firm A's job with a salient noncompete, whereas those with low asking rates are only 1 percentage point less likely to accept.

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 M.2 shows the 2SLS model corresponding to column (2) of Table V. Similarly, Figure M.3 examines the 2SLS model from the same table for post-reminder mobility.

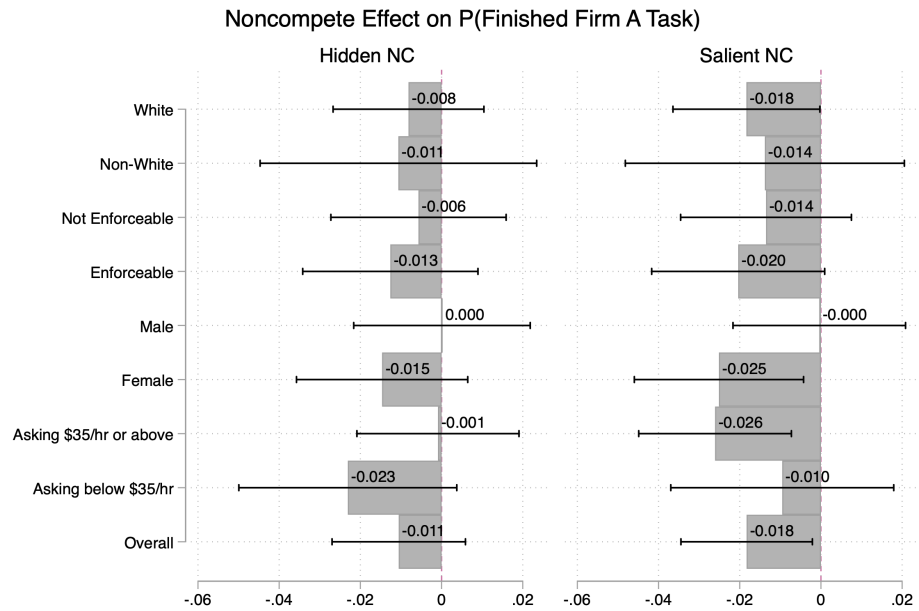
The results in Figure M.2 suggest some interesting heterogeneity. Pre-reminder, we see no evidence that signing a noncompete (either hidden or salient) reduces mobility. In fact, we find that the causal effect of signing a salient noncompete increases mobility in several subgroups, including states where the noncompete is *per se* unenforceable, for women, and those with high asking rates. In contrast, post-reminder, in Figure M.3, 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 M.4 and M.5 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 M.6 to M.8 show subsample analyses corresponding to the causal effect of signing a noncompete on earnings, both cumulative from Firm A and B and separately. In no subsample do we find that noncompetes, either salient or hidden, positively and statistically significantly increase earnings. The cumulative results for both the hidden and salient noncompetes suggest that both reduce earnings similarly for most groups. Looking

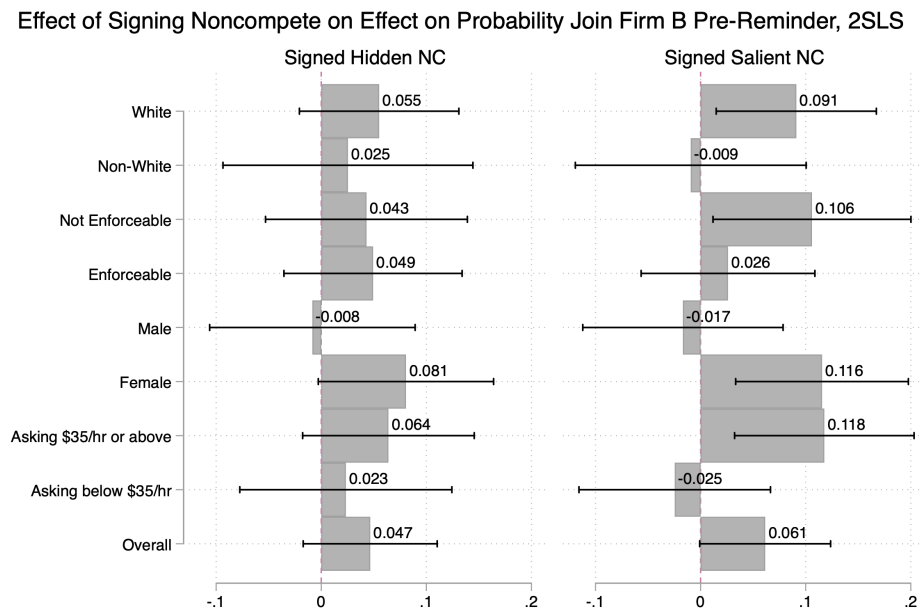
at the breakdown by Firm A and B earnings separately, the earnings losses from signing the noncompete appear to come from earning less from Firm B (Figure M.8).

Figure M.1: **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 (2) is estimated on that subsample and the coefficients and 95% confidence intervals on the hidden and salient noncompete variables are shown in that row.

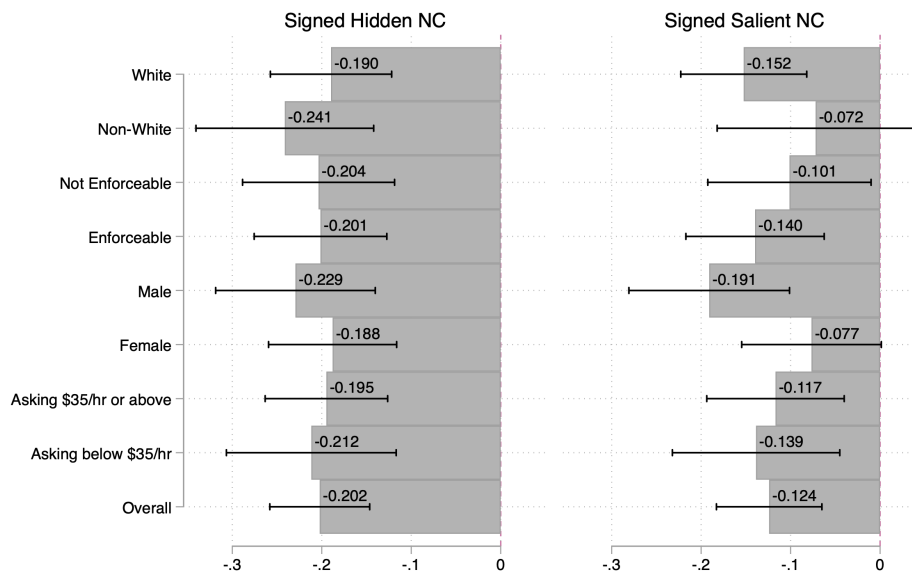
Figure M.2: Heterogeneity in Causal Effect of Signing a Noncompete on Joining Firm B Pre-Reminder, 2SLS Model



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

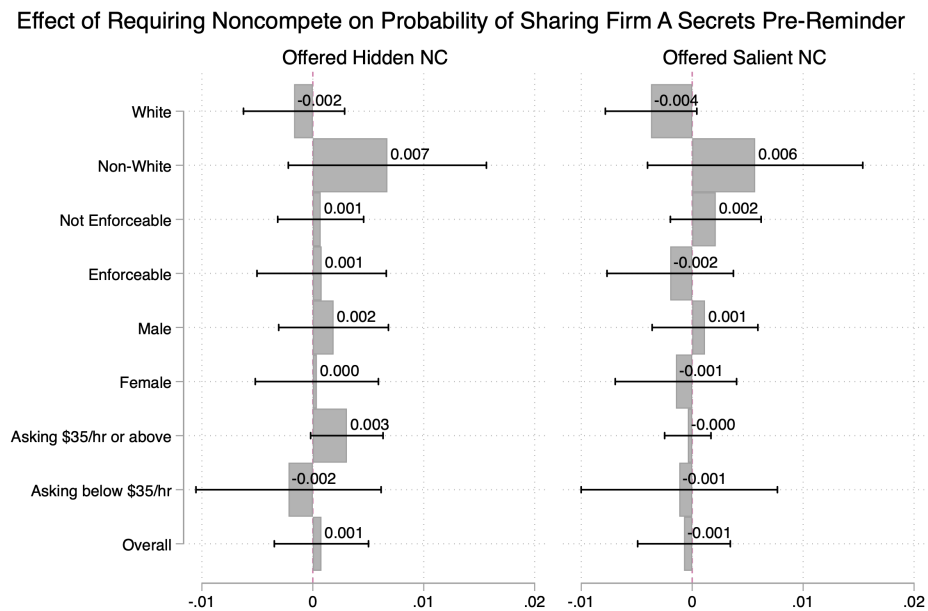
Figure M.3: **Heterogeneity in Causal Effect of Signing Noncompete on Joining Firm B Post-Reminder, 2SLS Model**

Effect of Signing Noncompete on Effect on Probability Join Firm B Post-Reminder, 2SLS



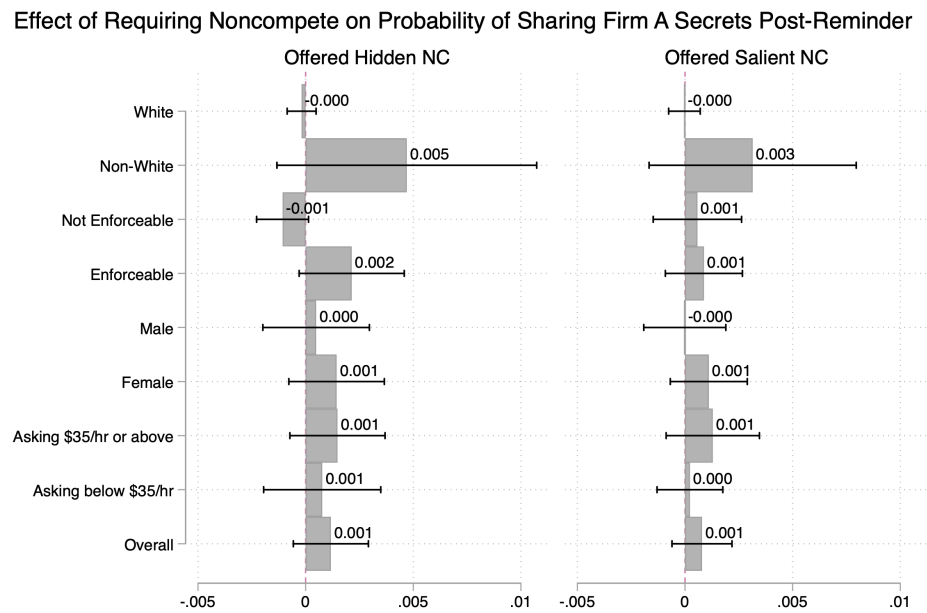
Notes: This figure shows subsample results from column (6) of Table V.

Figure M.4: **Heterogeneity in Causal Effect of Firm A Requiring Noncompetes on Sharing Firm A's Résumés Pre-Reminder**



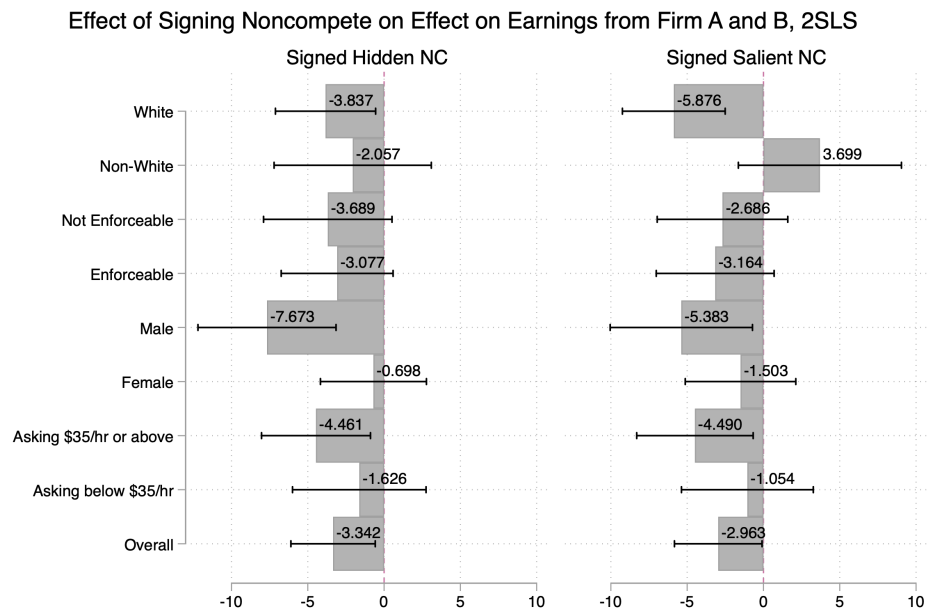
Notes: This figure shows subsample results from column (1) of Table VI.

Figure M.5: **Heterogeneity in Causal Effect of Firm A Requiring Noncompetes on Sharing Firm A's Résumés Post-Reminder**



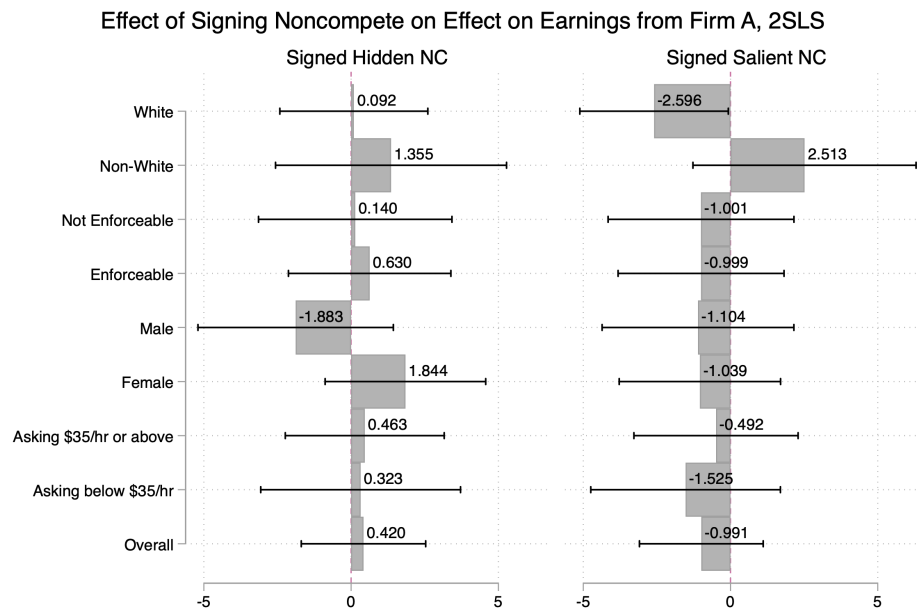
Notes: This figure shows subsample results from column (6) of Table VI.

Figure M.6: **Heterogeneity in Causal Effect of Signing a Noncompete on Firm A and B Earnings, 2SLS model**



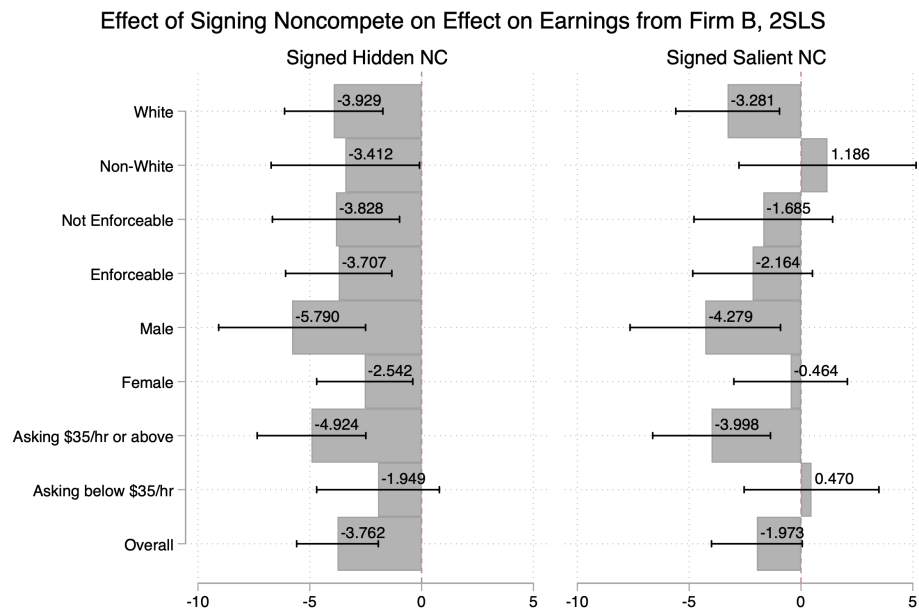
Notes: This figure shows subsample analyses of column (3) of Table VII.

Figure M.7: Heterogeneity in Causal Effect of Signing a Noncompete on Firm A Earnings, 2SLS model



Notes: This figure shows subsample analyses of column (3) of Table P.1.

Figure M.8: Heterogeneity in Causal Effect of Signing a Noncompete on Firm B Earnings, 2SLS model



Notes: This figure shows subsample analyses of column (3) of Table P.2.

N Résumé-Sharing Details and Additional Results

To measure résumé sharing pre-and post, we recorded the cumulative number of Firm A résumés shared as of the pre-reminder and post-reminder measurement dates. The pre-reminder measurement date was six days before the reminder. The post-reminder resume date was 32 days after the reminder.

We define post-reminder sharing using the difference between those cumulative counts. This number can be negative in instances where (for example) a recruiter deleted a previously-shared résumé from the messaging thread with Firm A. The main-text outcome in Table VI is binary: it equals one when the pre-reminder count is positive or, for the post-reminder measure, when the difference is positive. Appendix Tables N.1 and N.2 report the corresponding count outcomes using, respectively, a nonnegative difference and the unadjusted raw difference. The latter allows for negative numbers, and tests for the possibility that after the reminder, recruiter deleted previously-shared résumé from the messaging thread with Firm A.

We also made slight textual alterations to résumés shared by Firm A so that, if a recruiter later identified a candidate independently, we could distinguish such cases from sharing Firm A's résumés. There were no instances of a recruiter finding the same candidate independently; all instances of Firm A's shared résumés used the same version that was given to the recruiter by Firm A. As such, we have not reported this outcome separately in our tables.

Table N.1: Firm A Noncompetes and the Number of Firm A Résumés Shared with Firm B Before and After Reminder: Nonnegative Difference

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Number of Firm A Resumes Shared with Firm B							
	Before the Reminder				After Reminder: Nonnegative Difference			
Offered Salient Noncompete	-.0034 (.0051)				.00065 (.0017)			
Offered Hidden Noncompete	.00039 (.0056)				.0018 (.0024)			
Firm A Wage Offer	.00018 (.00012)				-5.1e-06 (.000051)			
Signed Salient		-.0019 (.036)	-.013 (.036)			-.0027 (.022)	-.01 (.023)	
Signed Hidden		.009 (.036)	.0047 (.035)	.045 (.081)		-.0027 (.024)	-.0034 (.022)	-.047 (.052)
Firm A Realized Wage		-.00041 (.00087)	.00018 (.0017)	.00048 (.0022)		-.00071 (.00057)	-.00078 (.0011)	-.0018 (.0015)
Firm B Wage Offer			.0028*** (.00082)	.0046* (.0025)			.000022 (.00052)	-.00067 (.0012)
Inverse Mills Ratio (λ)			.066 (.22)				.014 (.14)	
Sample	Invited	Pop.	Paid by Firm A	Skippers	Invited	Pop.	Paid by Firm A	Skippers
Estimator	OLS	2SLS	Heckman	OLS	OLS	2SLS	Heckman	OLS
Weighted	Y	N	N	Y	Y	N	N	Y
No-Noncompete Mean	0.01	0.10	0.10	0.11	0.00	0.03	0.03	0.04
KP RK Wald F		183.58				183.58		
Cragg-Donald Wald F		257.32				257.32		
Heckman: χ^2 (Excl. Inst.)			33.88***				33.88***	
Salient - Hidden	-0.004 (0.005)	-0.01 (0.03)	-0.02 (0.04)		-0.001 (0.002)	0.00005 (0.02)	-0.007 (0.02)	
Salient Wage Equiv.			4.55 (13.24)				444.71 (10369.23)	
Hidden Wage Equiv.			-1.71 (12.69)	-9.71 (18.89)			153.06 (3688.34)	-69.59 (127.89)
Observations	13,851	29,005	13,851	314	13,851	29,005	13,851	314
R ²	.012	.026		.37	.0075	.0058		.17

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. The outcome in columns 1–4 is the number of Firm A résumés shared with Firm B before the reminder. In columns 5–8, the outcome is the cumulative post-reminder total minus the pre-reminder count, with negative raw differences replaced by zero. The raw difference can be negative in instances where (for example) a recruiter deleted a previously-shared résumé from the messaging thread with Firm A. The outcome therefore retains both zero and positive counts and is not conditional on sharing at least one résumé or on joining Firm B. The omitted group is no noncompete. Columns 1–4 are pre-reminder and columns 5–8 are post-reminder. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. Column samples and designs are listed in the headings; skippers signed Firm A's contract without reading page 7. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Robust standard errors are in parentheses. Coefficients on count outcomes are measured in numbers of résumés; wage offers and wage equivalents are measured in dollars per hour. The no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample.

Table N.2: Firm A Noncompetes and the Number of Firm A Résumés Shared with Firm B Before and After Reminder: Raw Difference

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Number of Firm A Resumes Shared with Firm B							
	Before the Reminder				After Reminder: Raw Difference			
Offered Salient Noncompete	-.0034 (.0051)				.0034 (.003)			
Offered Hidden Noncompete	.00039 (.0056)				.0052 (.0035)			
Firm A Wage Offer	.00018 (.00012)				-.00011 (.000072)			
Signed Salient		-.0019 (.036)	-.013 (.036)			.016 (.031)	.0045 (.031)	
Signed Hidden		.009 (.036)	.0047 (.035)	.045 (.081)		.028 (.032)	.027 (.03)	.0049 (.078)
Firm A Realized Wage		-.00041 (.00087)	.00018 (.0017)	.00048 (.0022)		-.0012* (.00069)	-.000019 (.0015)	-.0036* (.0021)
Firm B Wage Offer			.0028*** (.00082)	.0046* (.0025)			-.00016 (.00069)	-.00088 (.0021)
Inverse Mills Ratio (λ)			.066 (.22)				.2 (.19)	
Sample	Invited	Pop.	Paid by Firm A	Skippers	Invited	Pop.	Paid by Firm A	Skippers
Estimator	OLS	2SLS	Heckman	OLS	OLS	2SLS	Heckman	OLS
Weighted	Y	N	N	Y	Y	N	N	Y
No-Noncompete Mean	0.01	0.10	0.10	0.11	-0.00	-0.01	-0.01	-0.02
KP RK Wald F		183.58				183.58		
Cragg-Donald Wald F		257.32				257.32		
Heckman: χ^2 (Excl. Inst.)			33.88***				33.88***	
Salient - Hidden	-0.004 (0.005)	-0.01 (0.03)	-0.02 (0.04)		-0.002 (0.003)	-0.01 (0.02)	-0.02 (0.03)	
Salient Wage Equiv.			4.55 (13.24)				28.61 (235.74)	
Hidden Wage Equiv.			-1.71 (12.69)	-9.71 (18.89)			171.04 (777.23)	5.57 (88.05)
Observations	13,851	29,005	13,851	314	13,851	29,005	13,851	314
R ²	.012	.026		.37	.009	.0027		.18

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. The outcome in columns 1–4 is the number of Firm A résumés shared with Firm B before the reminder. In columns 5–8, the outcome is the cumulative post-reminder total minus the pre-reminder count, without replacing negative differences by zero. This number can be negative in instances where (for example) a recruiter deleted a previously-shared résumé from the messaging thread with Firm A. The 15 subjects whose recorded cumulative post-reminder total is below their pre-reminder count retain negative outcome values. The outcome retains zero-count subjects and is not conditional on sharing at least one résumé or on joining Firm B. The omitted group is no noncompete. Columns 1–4 are pre-reminder and columns 5–8 are post-reminder. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. Column samples and designs are listed in the headings; skippers signed Firm A’s contract without reading page 7. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Robust standard errors are in parentheses. Coefficients on count outcomes are measured in numbers of résumés; wage offers and wage equivalents are measured in dollars per hour. The no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample.

N.1 Résumé Sharing, Conditional on Accepting Firm B

Our main results on résumé sharing in Section V.E (Table VI) are *unconditional* results. If a subject never works for Firm B, we code the résumé sharing as zero in Table VI. However, readers may also wonder about sharing rates conditional on joining Firm B.

In Table N.3, we address this section by modifying the specifications in Section V.C in the following ways. Let $P_i^B = \mathbb{1}(\text{Joined Firm B}_i)$, which can also be measured both pre- and post-reminder.

- Columns 1 and 5 present updated versions of Equation 3. This specification is now conditional on $P_i^B = 1$, i.e., joining Firm B (pre-reminder in Column 1, and post-reminder in Column 5).
 - When conditioning on joining Firm B, all remaining subjects who had been offered a noncompete are those who signed them. For clarity and parsimony, Table N.3 thus reports S_i and H_i (signed salient/hidden noncompetes) instead of O_i^S and O_i^H (offered salient/hidden noncompetes, as in the original Equation 3).
 - We do nothing in these columns to address the endogeneity of joining Firm B. As such, Columns 1 and 5 should be interpreted descriptively.
- Columns 2 and 6 update our IV specification (Equation 4). We use the same instruments, but use a new set of endogenous variables.
 - We now instrument for (P_i^B, S_i, H_i) . The last two variables are the same as Equation (4). For the first endogenous variable, we instrument for joining Firm B instead of Firm A.
 - Column 2 instruments for joining Firm B pre-reminder, and Column 6 instruments for joining Firm B post-reminder.
- Columns 3 and 7 update our Heckman specifications (Equation 26). We use the same excluded instruments for the selection equation, but have a different selection variable. In Column 3 we use joining Firm B (pre-reminder) as the selection binary, and in Column 7 we use joining Firm B (post-reminder) as the selection binary.
- Finally, Columns 4 and 8 update our “skipper/reminder” design (Equation 27). Both columns are limited to recruiters who join Firm B pre-reminder, so that the reminder can serve as a shock. Column 4 is a placebo test, and Column 8 shows the effect of our reminder intervention.

Our results in Table N.3 broadly find no evidence of noncompete effects. For most of the results, we can reject even small effects. In Column (6) we find a marginally statistically significant result, but they suggest that hidden noncompetes *increase* information leakage rather than decreasing them (as intended).

Comparing Results. Our results suggest signing noncompetes reduce mobility to Firm B after the reminder (Table V), but has no detectable effect on résumé sharing conditional on joining Firm B (Table N.3).

If résumé sharing behavior among movers is unchanged and fewer workers move, one might expect the unconditional sharing rate to decline. However, we find no detectable change in the unconditional sharing rate (Table VI).

This apparent contradiction arises because the conditional and unconditional specifications study different populations. Tables V and VI report unconditional results for the broader sample of Firm A workers, using several research designs with different identifying assumptions. By contrast, Table N.3 restricts attention to workers who join Firm B at the relevant measurement point. As a result, the conditional specification describes résumé sharing behavior among movers to Firm B, while the unconditional specifications capture the net effect of treatment on résumé sharing in the broader population. These estimates therefore need not line up mechanically, even when treatment reduces mobility to Firm B.

More informally, the unconditional specifications place weight on workers whose realized noncompete status is shifted by the relevant sources of variation, whereas the conditional specification places weight on workers who join Firm B under those designs. Because these are different margins of adjustment, the resulting estimates need not line up.

Table N.3: Firm A Noncompetes and Sharing Firm A Résumés with Firm B, Conditional on Joining Firm B

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1(Shared Firm A Resumes with Firm B)							
	Before the Reminder				After the Reminder			
Signed Salient	.019 (.021)	.0044 (.012)	.016 (.017)		.0078 (.011)	.0088 (.0062)	-.0011 (.02)	
Signed Hidden	.03 (.024)	.014 (.012)	.027 (.017)	.026 (.069)	.024 (.018)	.0097* (.0053)	.0079 (.028)	-.0067 (.016)
Firm A Wage Offer	-.00002 (.00056)				-.0004 (.0003)			
Firm A Realized Wage		-.00017 (.00028)	.00042 (.00044)	-.00089 (.0017)		-.0002 (.00013)	-.00015 (.00044)	-.00055 (.00046)
Firm B Wage Offer			.0011*** (.00039)	.0025 (.0022)			-.000083 (.00037)	-.00045 (.00049)
Inverse Mills Ratio (λ)			.073 (.074)				.034 (.073)	
Sample	Accept Firm B (Pre)	Pop.	Accept Firm B (Pre)	Skippers who Accept Firm B (Pre)	Accept Firm B (Post)	Pop.	Accept Firm B (Post)	Skippers who Accept Firm B (Pre)
Estimator	OLS	2SLS	Heck- man	OLS	OLS	2SLS	Heck- man	OLS
Weighted	Y	N	N	Y	Y	N	N	Y
No-Noncompete Mean	0.05	0.03	0.00	0.06	0.01	0.01	0.00	0.01
KP RK Wald F		43.43				21.82		
Cragg-Donald Wald F		67.04				32.71		
Heckman: χ^2 (Excl. Inst.)			25.27**				17.71	
Salient - Hidden	-0.01 (0.02)	-0.01 (0.01)	-0.01 (0.02)		-0.02 (0.02)	-0.001 (0.006)	-0.009 (0.02)	
Salient Wage Equiv.			-14.02 (15.84)				-13.44 (238.80)	
Hidden Wage Equiv.			-23.85 (17.14)	-10.38 (29.51)			94.27 (551.12)	-14.93 (34.54)
Observations	1,186	29,005	13,851	198	518	29,005	13,851	198
R^2	.14	.057		.51	.25	.0087		.31

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The outcome equals one if the worker shared at least one Firm A résumé; the sample is restricted to workers who joined Firm B at the relevant time. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. Columns 1-4 are pre-reminder and columns 5-8 are post-reminder. Columns 1 and 5 are descriptive conditional OLS; other columns use the design named in the heading. The omitted group is no noncompete. Heteroskedasticity-robust standard errors are in parentheses. Signed Salient and Signed Hidden indicate workers who signed those Firm A noncompetes. Indicator coefficients are percentage points. Note, the no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample.

O Phase 3: Job Search after (Randomized) Release from Noncompete

As a robustness check on the mobility results from Phase 2, the third phase of the experiment involved randomly releasing the workers from their noncompetes, and measuring their applications to another job. Figure O.1 illustrates the Phase 3 experimental design, while Figure F.1 shows the timeline. As workers were 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 was posted, two cross-randomized treatment schedules began. On each day, a few workers were randomly invited to apply to the job ad. Meanwhile, another set of randomly selected workers are sent a reminder/release message from Firm A. By this point in the experiment, all workers had been reminded about their NDA and their noncompete, if they had one (in Phase 2). The new message from Firm A reminds all workers again of their nondisclosure obligations, and releases them from their noncompete if they had one. The message can be found in Appendix F.7.

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

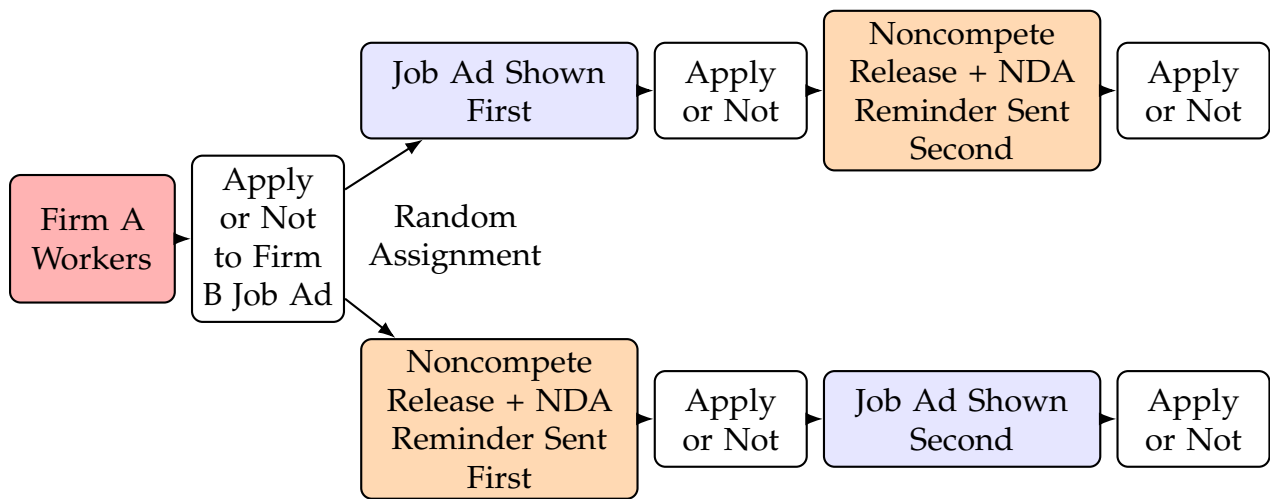
O.1 Empirical Approach

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:

$$\begin{aligned} y_{it} = & \beta_1 \mathbb{1}(\text{Noncompete}_i) + \beta_2 \mathbb{1}(\text{SentJobAd}_{it}) + \beta_3 \mathbb{1}(\text{Released}_{it}) \\ & + \beta_4 \mathbb{1}(\text{Noncompete}_i) \mathbb{1}(\text{SentJobAd}_{it}) + \beta_5 \mathbb{1}(\text{Noncompete}_i) \mathbb{1}(\text{Released}_{it}) \\ & + \beta_6 \mathbb{1}(\text{SentJobAd}_{it}) \mathbb{1}(\text{Released}_{it}) + \beta_7 \mathbb{1}(\text{Released}_{it}) \mathbb{1}(\text{SentJobAd}_{it}) \mathbb{1}(\text{Noncompete}_i) \\ & + \beta_8 w_i^B + \beta_9 w_i^A + \gamma_{s(i)} + \lambda_{t(i)} + \alpha_t + \epsilon_i, \end{aligned} \quad (28)$$

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).

Figure O.1: Phase 3 Experimental Design Summary

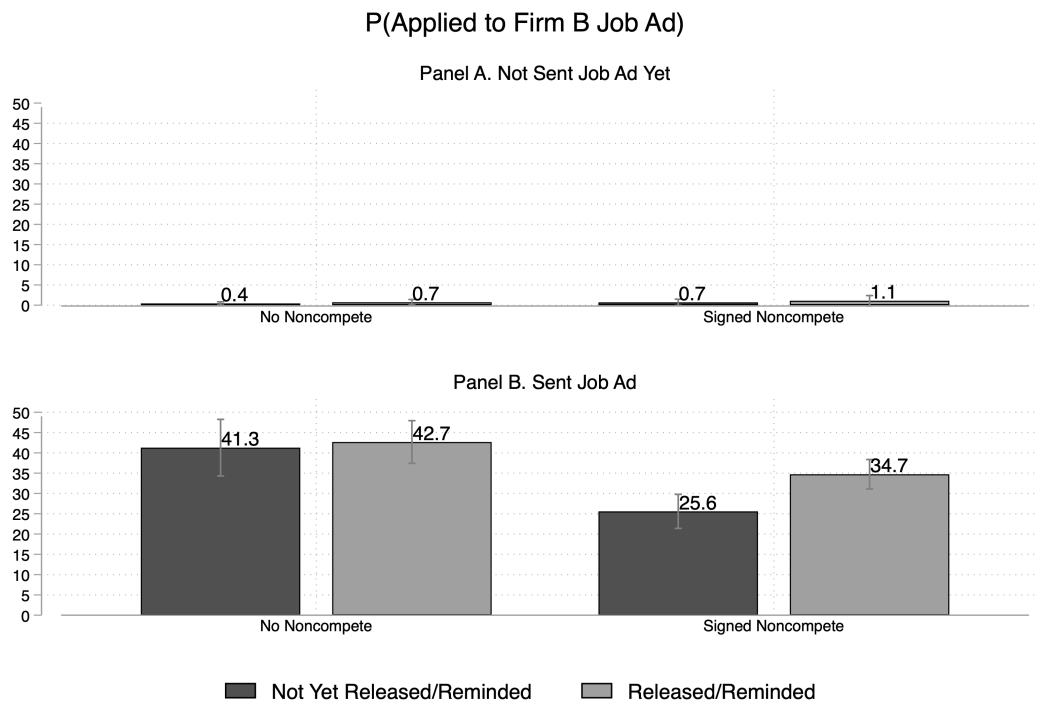


Notes: This figure describes Phase 3 of our experiment. Prior to this part of our experiment, a job opening was posted on a job search platform. We randomized both when workers were alerted to the job ad and when they were released from their noncompetes. The release of the noncompete came with a reminder of the ongoing NDA obligations. Workers in the no-noncompete 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.

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 being released from a noncompete. The randomized release did not induce changes in selection into noncompetes: Although workers may have selected into having a noncompete (or not), the randomized release occurs conditional on this selection. This approach allows workers to select into noncompetes and estimates the treatment effect of release. Because Equation (28) conditions on having signed a noncompete, selection differences are controlled for.

O.2 Phase 3 Results

Figure O.2: 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 partitions the data based on whether the worker had been sent the job ad 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).

Figure O.2 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 had 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 O.1: Phase 3 Randomized Noncompete Release and Applications to Firm B's Job Posting

Sample	(1) Noncompete Sample	(2) No Noncompete	(3) Full Sample
Firm A Wage Offer	-.0003 (.00073)	.0016 (.001)	.00043 (.00061)
Firm B Wage Offer	.0018** (.00069)	.00048 (.001)	.0012** (.0006)
Released/Reminded	.00049 (.014)	.00075 (.025)	.02 (.014)
Sent Job Ad	.26*** (.022)	.37*** (.034)	.37*** (.034)
Released/Reminded × Sent Job Ad	.067*** (.017)	.013 (.027)	.007 (.027)
Signed Noncompete			-.0014 (.011)
Released/Reminded × Signed Noncompete			-.018* (.01)
Sent Job Ad × Signed Noncompete			-.12*** (.039)
Released/Reminded × Sent Job Ad × Signed Noncompete			.065** (.033)
Constant	-.05 (.05)	-.062 (.072)	-.057 (.042)
Weighted No-Noncompete Mean	Y .	Y 0.31	Y 0.31
Observations	36,855	20,090	56,945
R ²	.22	.3	.2

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. Outcome is applying to Firm B's Phase 3 job posting. The sample is Firm A task completers reachable for the release/reminder message. Columns split the noncompete, no-noncompete, and full samples as shown. Standard errors are clustered by worker. Controls are strata, initial-outreach-date, Firm A wage, Firm B wage, and day fixed effects. Released/Reminded and Sent Job Ad indicate those messages had been sent by that date. The triple interaction is the release-by-job-ad effect for Firm A noncompete workers. No-Noncompete Mean is the omitted-group mean; Weighted denotes inverse-probability sampling weights; wage offers are dollars per hour; indicator coefficients are probability changes.

Table O.1 formally tests these differences using Equation (28). It also shows models only for the noncompete sample and the no-noncompete sample, which underlie the triple-differences model in column (3). Based on column (3), these models suggest that being released from a noncompete increased application rates to Firm B's job posting by 20% (0.065/0.31). They also suggest that not being released from a noncompete reduces willingness to apply by 39% (-0.12/0.31). Table O.2 reports similar effects with the Heckman correction approach, and Tables O.4 and O.3 report similar results for those with hidden and salient noncompetes.

Table O.2: Phase 3 Randomized Noncompete Release and Applications to Firm B's Job Posting, Heckman Correction

Sample	(1) Noncompete Sample	(2) No Noncompete	(3) Full Sample
Firm A Wage Offer	-.0001 (.00013)	.00066*** (.00018)	.00034*** (.0001)
Firm B Wage Offer	.00088*** (.00012)	.00077*** (.00018)	.00063*** (.000099)
Released/Reminded	.016 (.01)	.036*** (.014)	.032*** (.012)
Sent Job Ad	.3*** (.0093)	.41*** (.013)	.4*** (.012)
Released/Reminded × Sent Job Ad	.063*** (.012)	-.00062 (.016)	.011 (.015)
Signed Noncompete			-.01 (.0095)
Released/Reminded × Signed Noncompete			-.012 (.014)
Sent Job Ad × Signed Noncompete			-.1*** (.015)
Released/Reminded × Sent Job Ad × Signed Noncompete			.048*** (.018)
Constant	-.031 (.027)	-.099*** (.032)	-.099*** (.027)
Inverse Mills Ratio (λ)	-.15*** (.035)	.096** (.039)	.063 (.047)
Weighted	N	N	N
No-Noncompete Mean	.	0.31	0.31
Heckman: χ^2 (Excl. Inst.)	427.36***	391.82***	246.92***
Observations	44,240	24,115	68,355

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcome is applying to Firm B's Phase 3 job posting. The sample is Firm A task completers reachable for the release/reminder message. Columns split the noncompete, no-noncompete, and full samples as shown. Standard errors are clustered by worker. Models include strata and date fixed effects plus an inverse Mills ratio from a first-stage Heckman model using invite date as the excluded instrument. Released/Reminded and Sent Job Ad indicate those messages had been sent by that date. The triple interaction is the release-by-job-ad effect for Firm A noncompete workers. No-Noncompete Mean is the omitted-group mean; Weighted denotes inverse-probability sampling weights; wage offers are dollars per hour; indicator coefficients are probability changes. Note that the sample size is larger than in Table O.1 because it includes the first-stage observations from the Heckman model.

Table O.3: Phase 3 Randomized Noncompete Release and Applications by Hidden and Salient Noncompete Status

Sample	(1) Hidden	(2) Salient	(3) No Noncompete	(4) Completed Firm A Task
Firm A Wage Offer	-.00052 (.00098)	-.00083 (.00097)	.0016 (.001)	.00044 (.00061)
Firm B Wage Offer	.0015* (.00091)	.0014 (.00097)	.00048 (.001)	.0012** (.0006)
Released/Reminded	-.0065 (.021)	-.0039 (.022)	.00075 (.025)	.02 (.014)
Sent Job Ad	.27*** (.027)	.28*** (.032)	.37*** (.034)	.37*** (.034)
Released/Reminded × Sent Job Ad	.045* (.024)	.086*** (.023)	.013 (.027)	.0064 (.027)
Hidden				-.0072 (.012)
Salient				.0059 (.015)
Released/Reminded × Hidden				-.019* (.011)
Released/Reminded × Salient				-.016 (.013)
Sent Job Ad × Hidden				-.11** (.044)
Sent Job Ad × Salient				-.13*** (.044)
Released/Reminded × Sent Job Ad × Hidden				.048 (.036)
Released/Reminded × Sent Job Ad × Salient				.085** (.037)
Constant	-.032 (.07)	-.014 (.066)	-.062 (.072)	-.058 (.042)
Weighted No-Noncompete Mean	Y .	Y .	Y 0.31	Y 0.31
Observations	18,865	17,990	20,090	56,945
R ²	.3	.3	.3	.21

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. Outcome is applying to Firm B's Phase 3 job posting. The sample is Firm A task completers reachable for the release/reminder message. Columns split the hidden, salient, no-noncompete, and full samples as shown. Standard errors are clustered by worker. Controls are strata, initial-outreach-date, Firm A wage, Firm B wage, and day fixed effects. Released/Reminded and Sent Job Ad indicate those messages had been sent by that date. Triple-interaction rows capture the differential release-by-job-ad effect for the listed Firm A noncompete status. No-Noncompete Mean is the omitted-group mean; Weighted denotes inverse-probability sampling weights; wage offers are dollars per hour; indicator coefficients are probability changes.

Table O.4: Phase 3 Randomized Noncompete Release and Applications by Hidden and Salient Status, Heckman Correction

Sample	(1) Hidden	(2) Salient	(3) No Noncompete	(4) Completed Firm A Task
Firm A Wage Offer	.00059*** (.00017)	-.0008*** (.00018)	.00066*** (.00018)	.00034*** (.0001)
Firm B Wage Offer	.00053*** (.00017)	.0009*** (.00018)	.00077*** (.00018)	.00063*** (.000099)
Released/Reminded	.021 (.013)	-.0023 (.014)	.036*** (.014)	.033*** (.012)
Sent Job Ad	.32*** (.013)	.28*** (.013)	.41*** (.013)	.4*** (.012)
Released/Reminded × Sent Job Ad	.035** (.016)	.08*** (.017)	-.00062 (.016)	.01 (.015)
Hidden				-.017 (.011)
Salient				-.0018 (.011)
Released/Reminded × Hidden				-.014 (.016)
Released/Reminded × Salient				-.011 (.016)
Sent Job Ad × Hidden				-.083*** (.017)
Sent Job Ad × Salient				-.12*** (.017)
Released/Reminded × Sent Job Ad × Hidden				.026 (.021)
Released/Reminded × Sent Job Ad × Salient				.071*** (.021)
Constant	-.14*** (.03)	.19*** (.04)	-.099*** (.032)	-.13*** (.028)
Inverse Mills Ratio (λ)	-.099*** (.033)	-.37*** (.046)	.096** (.039)	.12** (.049)
Weighted	N	N	N	N
No-Noncompete Mean	.	.	0.31	0.31
Heckman: χ^2 (Excl. Inst.)	476.14***	312.28***	391.82***	246.92***
Observations	22,855	21,385	24,115	68,355

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcome is applying to Firm B's Phase 3 job posting. The sample is Firm A task completers reachable for the release/reminder message. Columns split the hidden, salient, no-noncompete, and full samples as shown. Standard errors are clustered by worker. Models include strata and day fixed effects plus an inverse Mills ratio from a first-stage Heckman model using invite date as the excluded instrument. Released/Reminded and Sent Job Ad indicate those messages had been sent by that date. Triple-interaction rows capture the differential release-by-job-ad effect for the listed Firm A noncompete status. No-Noncompete Mean is the omitted-group mean; Weighted denotes inverse-probability sampling weights; wage offers are dollars per hour; indicator coefficients are probability changes. Note that the sample size is larger than in Table O.1 because it includes the first-stage observations from the Heckman model.

P Earnings Results: Firms A and B Separately

In this appendix, we separate out our earnings results from Section VI (Table VII). Table P.1 shows that the effect of requiring a noncompete reduces Firm A compensation only in the salient group, because some workers chose not to work for Firm A and therefore were not paid by Firm A. For skippers, this is a placebo analysis. In contrast, Table P.2 shows that signing a salient or hidden noncompete reduces Firm B compensation—as expected given the mobility results.

Table P.1: Firm A Noncompetes and Earnings from Firm A

	(1)	(2)	(3)	(4)	(5)	(6)
	Firm A Compensation					
Offered Salient Noncompete	-.18** (.077)	-.92** (.4)				
Offered Hidden Noncompete	-.076 (.076)	-.41 (.41)				
Signed Salient			-.99 (1.1)	-.86 (1.1)		
Signed Hidden			.43 (1.1)	.56 (1.1)	-.078 (.055)	-3.5 (2.5)
Inverse Mills Ratio (λ)				.98 (6.8)		
Sample	Invited	Invited	Pop.	Paid by Firm A	Skippers	Skippers
Estimator	PPML	OLS	2SLS	Heck- man	PPML	OLS
Weighted	Y	Y	N	N	Y	Y
No-Noncompete Mean	5.60	5.60	45.57	45.57	46.61	46.61
KP RK Wald F			301.54			
Cragg-Donald Wald F			370.34			
Heckman: χ^2 (Excl. Inst.)				33.51***		
Salient - Hidden	-0.10 (0.08)	-0.51 (0.39)	-1.42 (1.11)	-1.43 (1.10)		
Observations	13,851	13,851	29,005	13,851	321	321
R^2		.012	.85			.048

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Earnings are experimental payments from Firm A only. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. The omitted group is no noncompete. All models include strata fixed effects; columns 1-2 also include outreach-date fixed effects. Skipper specifications use simplified strata-component fixed effects. Heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Weighted uses inverse-probability sampling weights. PPML coefficients are log points; other coefficients are dollars. Offered rows use randomized assignment; Signed rows use realized Firm A contract status. Note, the no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample.

Table P.2: Firm A Noncompetes and Earnings from Firm B

	(1)	(2)	(3)	(4)	(5)	(6)
	Firm B Compensation					
Offered Salient Noncompete	-.24 (.19)	-.19 (.15)				
Offered Hidden Noncompete	-.58*** (.19)	-.4*** (.12)				
Signed Salient			-2* (1)	-2.8*** (.96)		
Signed Hidden			-3.8*** (.93)	-3.8*** (.92)	-.62** (.27)	-4.1** (1.8)
Inverse Mills Ratio (λ)				.77 (5.8)		
Sample	Invited	Invited	Pop.	Paid by Firm A	Skippers	Skippers
Estimator	PPML	OLS	2SLS	Heck- man	PPML	OLS
Weighted	Y	Y	N	N	Y	Y
No-Noncompete Mean	0.92	0.91	8.34	8.34	8.68	8.68
KP RK Wald F			301.54			
Cragg-Donald Wald F			370.34			
Heckman: χ^2 (Excl. Inst.)				33.51***		
Salient - Hidden	0.35 (0.22)	0.21 (0.14)	1.79* (0.96)	0.96 (0.94)		
Observations	13,621	13,851	29,005	13,851	321	321
R^2		.012	.13			.032

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Earnings are experimental payments from Firm B only. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. The omitted group is no noncompete. All models include strata fixed effects; columns 1-2 also include outreach-date fixed effects. Skipper specifications use simplified strata-component fixed effects. Heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Weighted uses inverse-probability sampling weights. PPML coefficients are log points; other coefficients are dollars. Offered rows use randomized assignment; Signed rows use realized Firm A contract status. Note, the no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample.

Q Work beyond Firms A and B

Section VI of the main text describes how our experimental treatments lowered aggregate earnings from Firms A and B. It is possible that recruiters could have made up this loss by working for other firms we do not observe (including potentially jobs that would not be covered by the noncompete).

The goal of this section is to measure this possibility by collecting data about the subjects' other employment and earnings *beyond Firms A and B* — using the platform data we described in Appendix G.

To summarize our main findings: We find modest evidence that the noncompete inhibits workers from taking new (non-Firm A/B) work, and no evidence that workers with noncompetes substitute toward other non-Firm A/B work. Our results on this outcome are visible in unconditional plots of trends (Figure Q.1), and in our regression tests (Section Q.4). The clearest evidence comes from the skipper/reminder design. By contrast, our estimates of the exact dollar loss of this foregone work are noisy.

Background and Overview. Studying the effects of noncompetes on earnings and engagements with other firms was a primary goal of this experiment. Our initial plan was to seek an employer, randomize noncompetes in job offers, and measure mobility differences on LinkedIn or other platforms where workers post their experiences and accomplishments. However, we foresaw key obstacles with this approach.

- Many workers do not report all of their work experiences; they might especially under-report experiences when covered by a noncompete.
- Even with uncensored payroll data from a platform (as we have collected in Appendix G), power calculations could be challenging. If a few workers use a particular platform a lot, and others only occasionally — then outcome variables could be very sparse, skewed, and high variance.

For this reason, we recognized that we would need to *generate an offer*. This is a major reason our experimental design planned for “a Firm B,” even before we found a partner who could take this role (Section III.C). The other reason was that working with a second firm might allow us to measure information leakage (Section V).

Despite these complications — which appear in our analysis of the platform data — we find several consistent results below. Our results are stronger on the extensive margin of having other employment on this platform (at all), and noisier on the question of exactly how many dollars were sacrificed.

Q.1 Outcome Variables

Open Contracts. Our main outcome variable is whether a worker i has an open contract with any employer at month m . This is an *extensive margin* variable representing whether the worker is engaged in a task on the platform with another employer. Approximately 24% of our freelancers had an open contract on the platform in any given month during the experimental period.²⁷ We separately measure i) if a worker has any open contract, and ii) whether the worker has any open contract *excluding Firms A and B*. Aggregate trends for these variables can be seen by treatment group in Figure Q.1.

Covered/Uncovered Contracts. We also measure whether workers are performing jobs covered by Firm A's noncompete. Although Firm A's noncompete was written broadly, in practice it would likely be interpreted as (at best) covering other HR/recruiting jobs. To classify this, we used a brief description of each engagement included in the platform data. We developed a classification approach using LLMs following the guidance of [Ziems et al. \(2024\)](#); our specific procedure is described in detail in Appendix Q.7.²⁸ This produced a classification of all engagements in our dataset as being related to HR/recruiting ("covered") or not.

Unconditionally, 5.7% of the invited sample had a job covered by Firm A's noncompete during the experimental period (not including Firm B). That number jumps to 9.8% including Firm A and B. Using this classification, we construct three distinct dependent variables to identify covered jobs (including or excluding Firm A and B), and uncovered jobs:

1. An indicator for whether if recruiter i has at least one open contract (i.e., is working), that is NOT covered by Firm A's noncompete.
2. An indicator that the individual has at least one open contract that *is* covered by Firm A's noncompete, excluding both Firm A and B.
3. The third is the same as the second, except it includes Firm A and B. We include this to show the impact of our intervention.

Earnings. Finally, our data also includes earnings amounts from all engagements. We verified that the earnings amounts recorded in the platform data for Firms A and B were correct. When we used this data for outcomes, we found that (as anticipated) monthly earnings are both sparse and highly skewed. Approximately 2-4% of the invited sample received a payment on the platform in any given month. Average monthly earnings during the experimental period are approximately \$108, with a standard deviation of approximately \$2,400, driven by several individuals earning over \$150k in a given month.

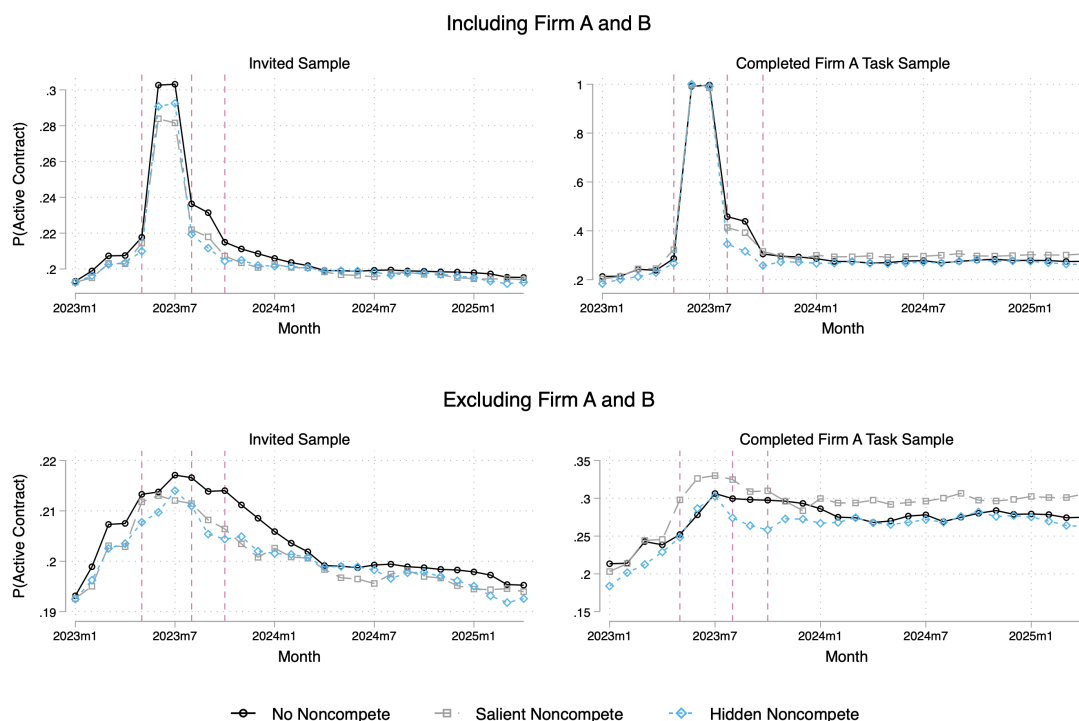
²⁷24% is higher than the average payment rate because contracts can be open for several months while only receiving payment after the work is finished.

²⁸Note that one limitation is that we can only observe the job title of the contract, not necessarily details about the employer.

As anticipated, our statistical power is low for the earnings outcome variable. While our results generally have the same sign of effects, standard errors are relatively wide and uninformative. Although we cannot reject zero effects, we also cannot reject large effects on either side (i.e., no “tight zero”). Despite this, we report our results in Section Q.6, and spend most of our discussion below on the extensive margin effects described above.

Q.2 Visualization of Unconditional Trends

Figure Q.1: Probability of Having an Open Contract



Notes: This figure shows the probability that workers have an open contract. Sample weights applied. The three, red, vertical lines, from left to right, reflect the month in which the first job offer went out, the month in which the reminder was sent, and the month in which individuals were released from their noncompetes.

Before proceeding with a regression analysis, Figure Q.1 shows unconditional average trends in the probability that an individual has an open contract for a job. The top panel includes contracts with Firm A and B, while the bottom panel excludes contracts with Firm A and B. The left panel reflects the weighted, invited sample, while the right panel conditions on those who completed Firm A’s task. The left-most red-dotted line is the month of the first invitation, the middle-line is the reminder, and the right-most red-dotted line is the month in which individuals were released from their noncompete.

The top panel, which includes contracts other than those with Firm A and B, provides a visualization of our experimental results from Sections IV and V:

- Among those invited, those offered a salient noncompete were the least likely to have an open contract after the initial offer from Firm A (Table III). We can see these differences in the plot: No-noncompete spikes are the highest, followed by hidden and salient. This is visible in the top left.
- Among those who completed Firm A's task, the hidden were most negatively affected post-reminder. We can see this in the top right, where open contracts on the platform is lower after the reminder for the hidden group. We can also see this trend in the top left, although post-reminder differences in the top left compound the initial selection into Firm A with the post-reminder drop.

The bottom panel repeats the same analysis but drops contracts with Firm A or B. Here, we see that in the invited sample the noncompete lines (blue and gray) dip below the black line (no noncompete), particularly during the period between the reminder and the release. Among those who worked for Firm A, we see a similar post-reminder drop for those with a hidden noncompete. This provides some visual evidence that the noncompetes dissuaded individuals from working with other employers on the platform.

Q.3 Data and Specifications

The rest of the empirical analysis in this section tests these ideas directly using the same empirical strategies developed in the manuscript. The sources of variation in the main paper are cross-sectional, with the exception of the within-person reminder effect (as applied to the skippers). As such, we transform the platform data from Sections G into an individual×monthly panel over the experimental period (i.e., between the first job offer and the noncompete release).

For completeness and consistency with our main text specifications, we mainly use the cross sectional source of variation in our specifications (with time-period fixed effects). However, the skipper diff-in-diff provides a sharp test of whether learning about the noncompete reduced work on this platform. This design compares hidden-noncompete skippers to no-noncompete skippers before and after the reminder. It is therefore less dependent on cross-sectional comparisons, permits individual fixed-effects, and allows us to check directly for both pre-reminder parallel trends and differential post-reminder changes.

We present all three of our empirical designs below in the same order as the main text. We omit the Heckman selection approach because the platform outcomes are observed for the full population of workers, including those who never joined Firm A, so there is no missing-outcome selection problem analogous to the Firm B analyses. The relevant concern here is instead selection into signing a noncompete, which we address through the IV/2SLS and skipper/reminder designs.

Specification #1: Reduced Form Effect of Requiring a Noncompete. To estimate the effect of Firm A requiring a noncompete, we follow our phase 1 approach, updating Equation 2 as follows:

$$y_{im} = \beta_1 \mathbb{1}(\text{Offered Salient NC}_i) + \beta_2 \mathbb{1}(\text{Offered Hidden NC}_i) + \gamma_{s(i)} + \psi_{t(i)} + \tau_m + \epsilon_{im}, \quad (29)$$

This is an update of Equation 2 to include month fixed effects τ_m . Strata fixed effects $\gamma_{s(i)}$ and fixed effects for the invitation order $\psi_{t(i)}$ are included. For consistency with Section VI, we omit the randomized Firm A and Firm B wage-offer controls from these regressions.²⁹ As in phase 2, we break out the results both before and after the reminder, and limit the data to the experimental period (e.g., between the first job offer and the noncompete release).

Specification #2: IV/2SLS Effect of Signing a Noncompete. To estimate the effect of signing a noncompete on the probability of having an open contract on the platform, we apply our Phase 2 2SLS specification (Sections V.C and K.2, Equation 4), adding month fixed effects.

We include the IV specification for consistency with our Phase 2 designs, but interpret it more cautiously than our main Firm B IV estimates. In the Firm B design, the experimental Firm B offer was generated only for workers who first worked for Firm A. This design feature reduces concerns that Firm A's invitation directly affects the Firm B outcome outside the Firm A contracting channel.

By contrast, in the present analysis of other platform employers, the dependent variable is other platform activity. A randomized Firm A offer could affect this outcome even if a worker declines Firm A's contract: for example, Firm A's invitation might make a worker more likely to keep the platform account active or search for other platform work. Such a channel would violate the exclusion restriction and result in biased estimates.

The direction of the resulting bias depends on how direct offer effects vary across offer arms. Appendix Q.8 discusses the sign of this bias. We study the benchmark case in which a positive direct activation effect is common across Firm A offer arms, and Firm A take-up rates in the noncompete arms are lower than in the no-noncompete arm (as shown in Section IV, Table III). In this case, a noncompete effect whose true value is negative would be either attenuated towards zero or could even become positive. We nonetheless include these results for consistency with our main-text approaches in Section V.

Specification #3: Skipper difference-in-differences. Our final specification uses the workers who signed Firm A's contract without reading page 7 at all ("skippers"). We use a

²⁹For outcomes based on work beyond Firms A and B, those wage offers are not themselves components of the dependent variable. Because wage offers were cross-randomized independently of noncompete assignment, omitting them does not affect identification of the noncompete effects; the estimates should be interpreted as effects averaged over the experiment's wage-offer distribution.

difference-in-differences adaptation of our main-text skipper specification in Sections V and K.2 (Equation 27). Before the reminder, hidden skippers and no-noncompete skippers had the same observed contracting experience: both signed the agreement without reading page 7. After the reminder, hidden skippers were informed that they were bound by Firm A's noncompete, while no-noncompete skippers were not.

We therefore compare the change in platform activity for hidden skippers before versus after the reminder to the corresponding change for no-noncompete skippers. Let $D_i = \mathbb{1}\{\text{Skipped Page 7}_i\}$ indicate that worker i signed Firm A's contract without viewing page 7. Let $H_i = \mathbb{1}\{\text{Hidden NC}_i\}$ indicate assignment to the hidden noncompete condition, and let $Post_m$ indicate that month m is in the post-reminder period. We restrict the sample to workers in the hidden or no-noncompete conditions who joined Firm A and skipped page 7. For each monthly platform outcome y_{im} , we estimate:

$$y_{im} = \theta (H_i \times Post_m) + \lambda H_i + \gamma_{s(i)} + \psi_{t(i)} + \tau_m + \varepsilon_{im}, \quad \text{for } P_i = 1, D_i = 1, O_i^S = 0. \quad (30)$$

Here $P_i = 1$ indicates that worker i joined Firm A, $O_i^S = 0$ excludes the salient noncompete condition, $\gamma_{s(i)}$ are randomization-stratum fixed effects, $\psi_{t(i)}$ are invite-order fixed effects, and τ_m are month fixed effects. The main post-reminder indicator $Post_m$ is absorbed by the month fixed effects. Standard errors are clustered at the individual level, and we use the same sample weights as in the manuscript. We also estimate a version with individual fixed effects a_i , where the individual fixed effects absorb H_i , the strata fixed effects, and the invite-order fixed effects.

The coefficient of interest is θ . It measures the differential post-reminder change in the outcome for hidden skippers relative to no-noncompete skippers. We interpret θ as a skipper-specific effect of learning, through the reminder, that one had signed a hidden noncompete. This is not an estimate of the average effect of signing a noncompete for all workers. Rather, it is a local reminder-specific estimate for the subgroup whose noncompete status was not revealed by their own contract reading.

The identifying assumption is that, absent the reminder, hidden skippers and no-noncompete skippers would have followed parallel trends in monthly platform activity. This assumption is plausible here because the hidden and no-noncompete conditions were randomly assigned, and because the skipper restriction ensures that treated workers did not actually observe the hidden noncompete page before signing. The pre-reminder period provides a placebo test: if hidden skippers did not know about the noncompete before the reminder, their platform activity should trend similarly to the no-noncompete skippers before the reminder.

To display an event-study visualization, we estimate the dynamic analogue of Equation (30) (with individual fixed effects):

$$y_{im} = \alpha_i + \tau_m + \sum_{\ell \neq -1} \theta_\ell \left(H_i \times \mathbb{1}\{m - M^R = \ell\} \right) + \varepsilon_{im}, \quad \text{for } P_i = 1, D_i = 1, O_i^S = 0, \quad (31)$$

where M^R is the reminder month and the omitted category is the month immediately before the reminder. The coefficients θ_ℓ trace the difference between hidden and non-competes in each month relative to the omitted pre-reminder month. Pre-reminder coefficients provide evidence on the parallel-trends assumption, while post-reminder coefficients show the timing and persistence of any reminder effect.

We estimate these models separately for the same platform outcomes used above: any open contract, open contracts covered by the noncompete including Firms A and B, open contracts covered by the noncompete excluding Firms A and B, and open contracts not covered by the noncompete.

Q.4 Regression Results

Table Q.1 displays the results for requiring a noncompete (Equation 29). The results are directionally consistent with our main experimental results in Section IV.C, though the results are weaker when looking at jobs excluding Firm A and B. Offering Firm A's job with a salient noncompete reduces the probability that an individual has an open contract before the reminder. After the reminder, we find that requiring a salient or hidden noncompete reduces the likelihood of having an open contract, though again the results weaken (though remain same-signed) when looking at jobs that do not include Firm A and B.

Table Q.1 contains two key results. First, columns 1, 2, 5, and 6 show that any potential substitution toward other jobs does not outweigh the negative effects on Firm A and B. Second, looking only at non-AB firms, we find a negative but underpowered effect. We therefore do not read Table Q.1 as providing stand-alone proof of a broad outside-employer effect. Its more precise implication is narrower but still important: any substitution toward other platform work does not offset the experimentally generated losses from Firms A and B. The outcomes including Firms A and B remain lower under noncompetes, especially after the reminder. Thus, the platform data do not support the concern that workers simply replaced the lost Firm A and B opportunities with other work on the same platform.

Table Q.1: Effect of Requiring Noncompete on Having an Open Contract

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	$\mathbb{1}(\text{Open Contract})$							
	Any	Covered Inc. A+B	Covered Excl. A+B	Not Covered	Any	Covered Inc. A+B	Covered Excl. A+B	Not Covered
	Before the Reminder				After the Reminder			
Offered Salient Noncompete	-.015* (.0078)	-.012* (.0068)	7.9e-06 (.0044)	7.0e-06 (.0065)	-.012* (.0072)	-.0063 (.005)	-.0011 (.0044)	-.0025 (.0069)
Offered Hidden Noncompete	-.011 (.0078)	-.01 (.0069)	-.0045 (.0043)	-.0046 (.0065)	-.016** (.0071)	-.015*** (.0049)	-.0056 (.0043)	-.0065 (.0069)
Sample	Invited	Invited	Invited	Invited	Invited	Invited	Invited	Invited
Estimator	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Weighted	Y	Y	Y	Y	Y	Y	Y	Y
No-Noncompete Mean	0.27	0.14	0.06	0.19	0.23	0.07	0.06	0.19
Salient - Hidden	-0.004 (0.008)	-0.001 (0.007)	0.005 (0.004)	0.005 (0.006)	0.005 (0.007)	0.008* (0.005)	0.004 (0.004)	0.004 (0.007)
Observations	41,553	41,553	41,553	41,553	41,553	41,553	41,553	41,553
R ²	.36	.086	.15	.39	.42	.12	.14	.37

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the individual level. The dependent variable is an indicator for having an open contract with the firms described in the dependent variable. Columns (1)-(4) refer to the pre-reminder period, while columns (5)-(8) refer to the post-reminder period. The dependent variable in columns (1) and (5) refer to having any open contract; columns (2) and (6) include any open contract covered by the noncompete, including Firm A and B; columns (3) and (7) include any open contract covered by the noncompete, excluding Firm A and B; and columns (4) and (8) include any open contract not covered by the noncompete. Whether the contract is covered by the noncompete is based on an LLM analysis of the title of the job and the language in the noncompete itself. Control variables include strata FE, invite order FE, and month FE. Sample is limited to the experimental months, starting with the first invitation and ending with the last release from the noncompete.

Table Q.2: Effect of Signing Noncompete on Having an Open Contract, 2SLS Approach

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	1(Open Contract)							
	Any	Covered Inc. A+B	Covered Excl. A+B	Not Covered	Any	Covered Inc. A+B	Covered Excl. A+B	Not Covered
	Before the Reminder				After the Reminder			
Signed Salient	.075 (.071)	.052 (.051)	.046 (.053)	.071 (.074)	.05 (.075)	.013 (.054)	.044 (.053)	.067 (.076)
Signed Hidden	-.023 (.07)	-.015 (.049)	-.021 (.051)	-.051 (.073)	-.083 (.074)	-.089* (.052)	-.017 (.051)	-.045 (.075)
Sample	Pop.	Pop.	Pop.	Pop.	Pop.	Pop.	Pop.	Pop.
Estimator	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Weighted	N	N	N	N	N	N	N	N
No-Noncompete Mean	0.78	0.71	0.11	0.29	0.45	0.25	0.11	0.30
KP RK Wald F	301.99	301.99	301.99	301.99	301.99	301.99	301.99	301.99
Cragg-Donald Wald F	1112.72	1112.72	1112.72	1112.72	1112.72	1112.72	1112.72	1112.72
Salient - Hidden	0.10 (0.07)	0.07 (0.05)	0.07 (0.05)	0.12 (0.08)	0.13* (0.08)	0.10* (0.06)	0.06 (0.05)	0.11 (0.08)
Observations	87,015	87,015	87,015	87,015	87,015	87,015	87,015	87,015
R ²	.13	.28	.00028	-.000025	.01	.018	.00011	.0018

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the individual level. The dependent variable is an indicator for having an open contract with the firms described in the dependent variable. Columns (1)-(4) refer to the pre-reminder period, while columns (5)-(8) refer to the post-reminder period. The dependent variable in columns (1) and (5) refer to having any open contract; columns (2) and (6) include any open contract covered by the noncompete, including Firm A and B; columns (3) and (7) include any open contract covered by the noncompete, excluding Firm A and B; and columns (4) and (8) include any open contract not covered by the noncompete. Whether the contract is covered by the noncompete is based on an LLM analysis of the title of the job and the language in the noncompete itself. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. Control variables include strata FE and month FE. Sample is limited to the experimental months, starting with the first invitation and ending with the last release from the noncompete. Note, the no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample

IV/2SLS. Table Q.2 shows our IV/2SLS specification on the platform data. The first-stage diagnostics are strong: the Kleibergen–Paap rk Wald statistic is above 300. However, the IV/2SLS coefficients are generally imprecise, with the one exception that we find evidence that signing a hidden noncompete reduces take-up of jobs covered by the noncompete post-reminder when including Firm B’s job. Excluding Firm B’s job, the hidden effects are same-signed, but attenuate and become statistically insignificant.

We include the IV specification for consistency with the main text. For the hidden estimates it points in the same direction as the reduced-form estimates and the main Firm B IV estimates, although we cannot reject zero. However as stated earlier, there is a possible exclusion-restriction violation: a Firm A offer could activate general platform use, even for workers who do not sign Firm A’s contract. Appendix Q.8 shows how this would either attenuate the estimate towards zero or potentially reverse the sign to positive (particularly in the condition that noncompete take-up is lower (as shown in Table III for salient)).

Skipper Analysis. Figure Q.2 and Table Q.3 provide the cleanest evidence in this appendix that noncompetes reduced work with other platform employers. The skipper/DiD design isolates a local, reminder-specific treatment effect of learning about a previously unseen noncompete.

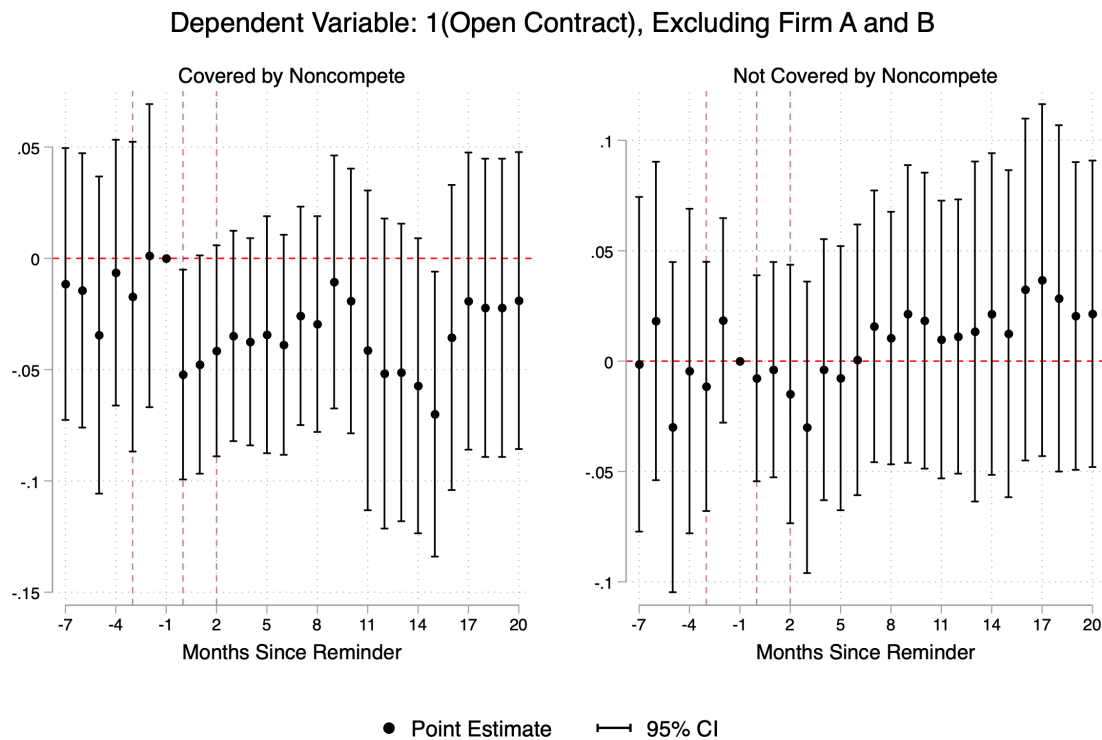
The event-study evidence in Figure Q.2 illustrates the effect. The pre-reminder coefficients for covered work excluding Firms A and B are the key placebo test: the coefficients are centered near zero, meaning that hidden skippers do not appear to have been differentially reducing covered platform work before they learned about the noncompete.

After the reminder, the covered-work coefficients turn negative and remain negative for much of the post-reminder period. By contrast, the uncovered-work panel shows no comparable post-reminder break. This covered-versus-uncovered contrast is important. If the reminder merely discouraged workers from using the platform, made them generally less responsive to employers, or coincided with an unrelated decline in demand for their services, we would expect similar effects on uncovered work. Instead, the post-reminder decline is concentrated in work plausibly within the scope of Firm A’s noncompete.

Table Q.3 formalizes the same comparison in a difference-in-differences specification, iterating between models that exclude versus include individual fixed effects. The main effect of “Signed Hidden”—which reflects the differential between hidden and no-noncompete in the pre-reminder period—is small and statistically insignificant in the specifications (this variable is subsumed in the specification with individual fixed effects). This is another placebo-style result: before the reminder, hidden skippers do not differ sharply from no-noncompete skippers in their platform work. The post-reminder interaction is the coefficient of interest. For any open contract, the interaction is -3.4 percentage points and statistically insignificant. This estimate is negative, but it combines covered and uncovered work and therefore averages across the margin where the noncompete should matter and the margin where it might not.

The covered-work estimates are stronger. When the dependent variable is an open

Figure Q.2: Skipper Event-Study Analysis of Having an Open Contract, Excluding Firm A and B



Notes: Sample includes skippers, weights applied. The three, red, vertical lines, from left to right, reflect the month in which the first job offer went out, the month in which the reminder was sent, and the month in which individuals were released from their noncompetes.

contract covered by the noncompete including Firms A and B, the post-reminder hidden interaction is -8.6 percentage points, significant at the 10 percent level. When Firms A and B are excluded, the estimate is -4.2 percentage points and significant at the 5 percent level.

Relative to the no-noncompete mean of 10 percent in the covered-excluding-Firms-A-and-B columns, this is a large effect: learning about the hidden noncompete reduces covered platform work (outside of Firm A and B) by approximately 42 percent among skippers. The estimate is unchanged when individual fixed effects are added, which is useful because the individual-fixed-effects specification absorbs all time-invariant differences between hidden and no-noncompete skippers.

The uncovered-work estimates provide evidence of a lack of substitution toward non-covered tasks. In columns 7 and 8, the post-reminder hidden interaction is -1.1 percentage points and statistically insignificant relative to a no-noncompete mean of 21 percent. The point estimate is small compared with the covered-work effect, and the absence of a significant uncovered-work effect reinforces the mechanism. The reminder did not simply

lower all platform activity. It lowered the kind of platform work most plausibly implicated by the noncompete, and we see little evidence of substitution towards uncovered jobs. Together, our results from the skipper design offer the strongest evidence in the referee appendix that the noncompete did more than shift work between Firms A and B; it also deterred covered outside opportunities on the platform.

Table Q.3: Effect of Signing Noncompete on Having an Open Contract, Skipper Difference-in-Differences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	1(Open Contract)							
	Any		Covered Inc. A+B		Covered Excl. A+B		Not Covered	
Signed Hidden	.026 (.028)		.031 (.026)		.03 (.03)		.069 (.044)	
Signed Hidden $\times$ Post-Reminder	-.034 (.055)	-.034 (.055)	-.086* (.048)	-.086* (.048)	-.042** (.02)	-.042** (.02)	-.011 (.026)	-.011 (.026)
Sample	Skipper	Skipper	Skipper	Skipper	Skipper	Skipper	Skipper	Skipper
Estimator	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Weighted	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE	N	Y	N	Y	N	Y	N	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y
No-Noncompete Mean	0.58	0.58	0.48	0.48	0.10	0.10	0.21	0.21
Observations	1,926	1,926	1,926	1,926	1,926	1,926	1,926	1,926
R ²	.57	.68	.68	.76	.41	.78	.54	.86

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the individual level. The dependent variable is an indicator for having an open contract with the firms described in the dependent variable. The dependent variable in columns (1) and (2) refer to having any open contract; columns (3) and (4) include any open contract covered by the noncompete, including Firm A and B; columns (5) and (6) include any open contract covered by the noncompete, excluding Firm A and B; and columns (7) and (8) include any open contract not covered by the noncompete. Whether the contract is covered by the noncompete is based on an LLM analysis of the title of the job and the language in the noncompete itself. Base control variables include strata FE, invite-order FE, and month FE. Individual FE are included in even numbered columns, subsuming the main effect of signing a hidden noncompete. The sample is limited to the experimental months, starting with the first invitation and ending with the last release from the noncompete.

Q.5 Discussion and Conclusion

The platform evidence in this section addresses the limitation noted in Section VI: the main earnings analysis observes payments from Firms A and B, but not all other work the recruiters may have performed. No dataset can fully solve this problem. Even tax records could miss under-reported work, and workers may use multiple platforms or offline relationships.

Nonetheless, the work-history data analyzed here are valuable because they measure a large set of other engagements on the same platform used for the experiment, for the full 29K-worker population described in Appendix G. This lets us test whether the losses from Firms A and B were offset by other platform work, and whether noncompetes deterred

covered outside work beyond the specific Firm B opportunity generated by the experiment.

The three designs tell a consistent but appropriately qualified story. The reduced-form estimates in Table Q.1 show that requiring a noncompete lowers aggregate platform work when Firms A and B are included, especially after the reminder. They also show no evidence of offsetting substitution into uncovered or other non-AB work. That is, in no specification do we observe a positive and statistically significant effect of requiring or signing a noncompete on working for others. The IV estimates in Table Q.2 are less believable in light of potential exclusion restrictions, but they nevertheless point in the same direction after the reminder for hidden noncompete signers. The skipper design in Figure Q.2 and Table Q.3 is the most informative design for other employers: among workers who skipped the hidden clause, the reminder reduces covered outside platform work but does not increase uncovered work.

Substantively, these results reinforce the main paper's interpretation of noncompetes as operating through attention and perceived enforcement risk. The hidden clause has limited effects before workers are reminded, consistent with the reading-time evidence in Section IV.C. Once workers receive a reminder, hidden noncompetes reduce mobility and covered work, consistent with the Firm B results in Section V. The salient-versus-hidden comparison also mirrors the main text. Salient noncompetes deter entry into Firm A, but those who knowingly sign them appear less deterred later, consistent with selection on beliefs about enforcement or willingness to comply. Hidden noncompetes, by contrast, are easier to enter but more consequential after a reminder.

The covered-versus-uncovered distinction is also important for interpretation. The strongest outside-employer evidence is not a generalized decline in platform activity. It is a decline in contracts classified as covered by Firm A's noncompete. This targeted pattern makes the noncompete mechanism more plausible. It also helps answer the concern that the main Firm B design might overstate the effect of noncompetes because Firm B generated an unusually salient competing offer. In the skipper analysis, the decline appears even after excluding Firms A and B, using other platform employers that were not part of the experimental design.

At the same time, we do not interpret these results as precise estimates of total earnings losses outside Firms A and B. The extensive-margin outcome is much more informative than monthly earnings. As discussed in Section Q.6, platform earnings are highly skewed, with a small number of very large monthly payments generating wide standard errors. The earnings tables therefore have the same general sign as the open-contract results but do not provide stable dollar estimates. The most defensible conclusion is that the main earnings losses in Section VI are not offset by observed substitution to other platform work, and that the best-powered outside-employer design shows a meaningful decline in covered outside contracts among hidden skippers after the reminder.

Q.6 Extra Tables: Earnings

The tables in this subsection repeat the same three research designs using monthly platform earnings rather than open-contract indicators. We report them for completeness, but we place less weight on these estimates than on the extensive-margin results above. The reason is statistical power. As discussed in the start of Appendix Q, monthly earnings are both sparse and highly skewed: most worker-months have little or no payment, while a small number of worker-months have very large payments. In this setting, an open-contract indicator is a more stable measure of whether the noncompete changed work opportunities, while monthly earnings are a noisier measure of the exact dollar value of those opportunities.

Table Q.4 reports the reduced-form earnings estimates. Overall, the results suggest that the effect of Firm A requiring a noncompete reduces monthly earnings during the experimental period, though the estimates are imprecise. Before the reminder, the salient and hidden estimates are negative, both including and excluding Firms A and B, but the standard errors are large relative to the point estimates. After the reminder, the estimates are close to zero. These results therefore do not provide precise evidence about dollar losses from other platform employers. More importantly, they do not show any positive offsetting earnings response in column (2) that would undo the Firm A and Firm B earnings losses documented in Section VI.

Table Q.5 reports the IV/2SLS earnings estimates. Over the whole experimental period (columns 1 and 2), the estimates of the effect of signing a noncompete are negative, but imprecise. The pre-reminder hidden-noncompete coefficient is large and negative, and statistically significant at the 5 percent level, but the magnitude is much larger than the no-noncompete mean and should be interpreted cautiously in light of the skewed earnings distribution and the IV concerns discussed in Appendix Q.8. The post-reminder IV estimates are similarly negative but also not statistically significant. Thus, while the signs are broadly consistent with noncompetes reducing platform earnings, the IV earnings results are not stable enough to support a separate quantitative conclusion.

Table Q.6 gives the skipper difference-in-differences earnings estimates. The post-reminder hidden interaction is -\$83 per month in all four specifications, both including and excluding Firms A and B, and both with and without individual fixed effects. This sign is consistent with the open-contract skipper estimates in Table Q.3, but the standard error is \$152, so the estimate is not statistically significant. We therefore interpret the earnings evidence as complementary rather than central. It is consistent with the extensive-margin evidence that hidden noncompetes reduce covered platform work after the reminder, but it is too noisy to estimate the exact dollar value of the foregone work.

Table Q.4: Effect of Requiring Noncompete on Monthly Platform Earnings

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Monthly Pay					
	Including Firm A+B	Excluding Firm A+B	Including Firm A+B	Excluding Firm A+B	Including Firm A+B	Excluding Firm A+B
	Overall		Before the Reminder		After the Reminder	
Offered Salient Noncompete	-6.3 [-47,34]	-6.1 [-46,34]	-14 [-75,47]	-13 [-75,48]	1.2 [-49,51]	1.3 [-49,51]
Offered Hidden Noncompete	-21 [-69,27]	-21 [-69,27]	-43 [-99,12]	-43 [-99,12]	.57 [-76,77]	.7 [-76,77]
Sample	Invited	Invited	Invited	Invited	Invited	Invited
Estimator	OLS	OLS	OLS	OLS	OLS	OLS
Weighted	Y	Y	Y	Y	Y	Y
No-Noncompete Mean	117.90	116.82	134.54	132.69	101.26	100.94
Salient - Hidden	15.11 (23.31)	15.16 (23.31)	29.56 (26.78)	29.73 (26.78)	0.66 (37.74)	0.59 (37.74)
Observations	83,106	83,106	41,553	41,553	41,553	41,553
R ²	.0071	.0071	.0093	.0093	.0083	.0083

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the individual level. 95% confidence interval is in brackets. The dependent variable is monthly pay from jobs on the platform. Columns (1)-(2) refer to the full experimental period, while columns (3)-(4) and columns (5) and (6) refer to the pre-reminder and post-reminder periods, respectively. The dependent variable in columns (1) and (3) and (5) is total monthly pay from the platform including Firm A and B; columns (2) and (4) and (6) include total monthly pay from the platform excluding Firm A and B. Control variables include strata FE, invite order FE, and month FE. Sample is limited to the experimental months, starting with the first invitation and ending with the last release from the noncompete.

Table Q.5: Effect of Signing Noncompete on Monthly Platform Earnings, 2SLS Approach

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Monthly Pay					
	Including Firm A+B	Excluding Firm A+B	Including Firm A+B	Excluding Firm A+B	Including Firm A+B	Excluding Firm A+B
	Overall		Before the Reminder		After the Reminder	
Signed Salient	-112 [-626,402]	-112 [-626,402]	-137 [-837,563]	-137 [-837,563]	-87 [-747,573]	-87 [-747,574]
Signed Hidden	-317 [-786,152]	-317 [-785,152]	-621** [-1,204,-38]	-621** [-1,204,-38]	-13 [-655,629]	-12 [-654,630]
Sample	Pop.	Pop.	Pop.	Pop.	Pop.	Pop.
Estimator	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Weighted	N	N	N	N	N	N
No-Noncompete Mean	163.94	154.97	232.61	217.60	95.27	92.35
KP RK Wald F	302.10	302.10	301.99	301.99	301.99	301.99
Cragg-Donald Wald F	2226.28	2226.28	1112.72	1112.72	1112.72	1112.72
Salient - Hidden	204.87 (240.17)	204.85 (240.17)	483.88 (323.77)	484.39 (323.77)	-74.14 (326.54)	-74.70 (326.54)
Observations	174,030	174,030	87,015	87,015	87,015	87,015

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the individual level. 95% confidence interval is in brackets. The dependent variable is monthly pay from jobs on the platform. Columns (1)-(2) refer to the full experimental period, while columns (3)-(4) and columns (5) and (6) refer to the pre-reminder and post-reminder periods, respectively. The dependent variable in columns (1) and (3) and (5) is total monthly pay from the platform including Firm A and B; columns (2) and (4) and (6) include total monthly pay from the platform excluding Firm A and B. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. Control variables include strata FE and month FE. Sample is limited to the experimental months, starting with the first invitation and ending with the last release from the noncompete. Note, the no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample

Table Q.6: Effect of Signing Noncompete on Monthly Platform Earnings, Skipper Difference-in-Differences

	(1)	(2)	(3)	(4)
Dependent Variable:	Monthly Pay			
	Including Firm A+B		Excluding Firm A+B	
Signed Hidden	195 [-171,561]		196 [-170,562]	
Signed Hidden $\times$ Post-Reminder	-83 [-382,216]	-83 [-382,216]	-83 [-382,216]	-83 [-382,216]
Sample	Skipper	Skipper	Skipper	Skipper
Estimator	OLS	OLS	OLS	OLS
Weighted	Y	Y	Y	Y
Individual FE	N	Y	N	Y
Month FE	Y	Y	Y	Y
No-Noncompete Mean	129.74	129.74	120.53	120.53
Observations	1,926	1,926	1,926	1,926
R^2	.077	.22	.077	.22

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the individual level. 95% confidence interval is in brackets. The dependent variable is monthly pay from jobs on the platform. The dependent variable in columns (1) and (2) refer to monthly pay including Firm A and B; columns (3) and (4) refer to monthly earnings excluding Firm A and B. Base control variables include strata FE, invite-order FE, and month FE. Individual FE are included in even numbered columns, subsuming the main effect of signing a hidden noncompete. Sample is limited to the experimental months, starting with the first invitation and ending with the last release from the noncompete.

Q.7 LLM Prompts to Categorize “Covered” Jobs

A small literature attempts to provide best practices for using LLMs to code data. The prompt below was informed by the approach outlined by [Ziems et al. \(2024\)](#) and given to gpt-5.4-mini with the title of each job in our platform dataset performed by our 29K subjects.³⁰

You are classifying freelance job-task titles for whether they plausibly overlap with the restricted service domain of a partner firm covered by a noncompete.

The partner firm’s restricted domain is:
core HR and recruiting services, especially hiring-related work such as recruiting, sourcing, screening, interviewing, and talent acquisition.

Your job is to make two separate judgments:

1. domain_relevance

Does the title appear to be in the HR / recruiting / talent domain?

Allowed values:

- yes
- maybe
- no

2. direct_service

Does the title suggest actually performing direct HR or recruiting work (e.g., hiring, screening, sourcing, interviewing), rather than writing about it, advising loosely, or working in adjacent areas?

Allowed values:

- yes
- maybe
- no

Then produce final_label using this guidance:

- overlap:
Use ONLY when BOTH:
 - domain_relevance = yes
 - direct_service = yes

³⁰The platform data did not contain a longer description beyond the title.

AND the task clearly reflects core hiring or HR service work such as:

recruiting, sourcing, resume screening, interviewing, talent acquisition, or closely related HR execution.

- unsure:

Use when:

- either dimension is "maybe"
- the task is HR-adjacent but not clearly core recruiting/service work
- the task is in areas such as payroll, benefits, contracts, compliance, legal, or general people operations where overlap is uncertain
- the title is vague, generic, or underspecified

IMPORTANT:

Payroll, benefits, employment contracts, HR compliance, HR consulting, and general people-ops tasks should default to "unsure" unless the title clearly indicates core recruiting or hiring work.

- not_overlap:

Use when:

- domain_relevance = no

OR

- the task is clearly not direct service work, such as:
 - writing, blogging, editing, content creation about HR
 - marketing or copywriting about HR/recruiting
 - unrelated domains (tutoring, translation, web design, bookkeeping, etc.)

CRITICAL DISTINCTIONS:

- Writing about HR is NOT overlap
- Marketing about HR is NOT overlap
- Teaching HR concepts is usually NOT overlap
- Payroll, legal, and general HR advisory work are NOT automatically overlap
- Only core recruiting / hiring / talent execution tasks should be labeled overlap

IMPORTANT DISAMBIGUATION:

- Terms like "sourcer" or "sourcing" should ONLY count as overlap when clearly referring to recruiting, candidates, hiring, or talent acquisition.

- If "sourcer" refers to media, video, images, products, or other non-candidate objects, it is NOT overlap.

Guidelines:

- Be conservative in labeling overlap
- Prefer unsure over overlap when not clearly hiring-related
- Prefer unsure over not_overlap when the domain is ambiguous

Return valid JSON only with keys:

domain_relevance, direct_service, final_label, confidence

Q.8 Direct Offer Bias in the Platform IV Specifications

This subsection clarifies how a direct effect of receiving a Firm A offer would enter the platform 2SLS estimates. The concern is distinct from the reduced-form effect of requiring a noncompete. If receiving any Firm A offer shifted other-platform activity in the no-noncompete, salient-noncompete, and hidden-noncompete arms, that effect is part of the treatment effect and does not induce any bias.

The 2SLS specification is different because it rescales offer-arm differences by realized Firm A take-up. To see the sign intuition, consider a simplified version with no controls and a common positive direct activation effect $\delta > 0$ from receiving any Firm A offer. Let p_0 , p_S , and p_H denote Firm A take-up in the no-noncompete, salient-noncompete, and hidden-noncompete arms, respectively. The common activation channel shifts each offer-arm reduced form by δ , but the IV estimates scale this shift by take-up. In the nested specification, the induced bias in the incremental salient and hidden noncompete coefficients is

$$\delta \left(\frac{1}{p_S} - \frac{1}{p_0} \right) \quad \text{and} \quad \delta \left(\frac{1}{p_H} - \frac{1}{p_0} \right),$$

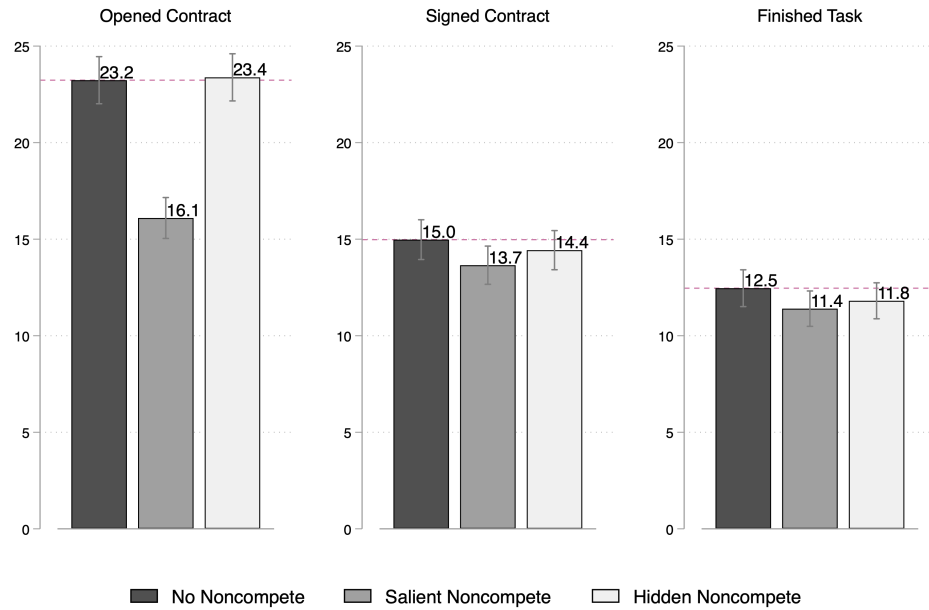
respectively.

If take-up is identical across offer arms, this common activation effect loads on the Firm A participation coefficient and cancels from the incremental noncompete coefficients. If take-up in a noncompete arm is lower than in the no-noncompete arm — as shown in Section IV, Table III — the corresponding bias term above is positive.

As such, the activation channel pushes that incremental noncompete coefficient in a more positive direction. Thus, for a negative true noncompete effect, this benchmark channel would either attenuate the estimate toward zero or even turn the point estimate positive. Offer-arm-specific direct effects could generate different bias patterns.

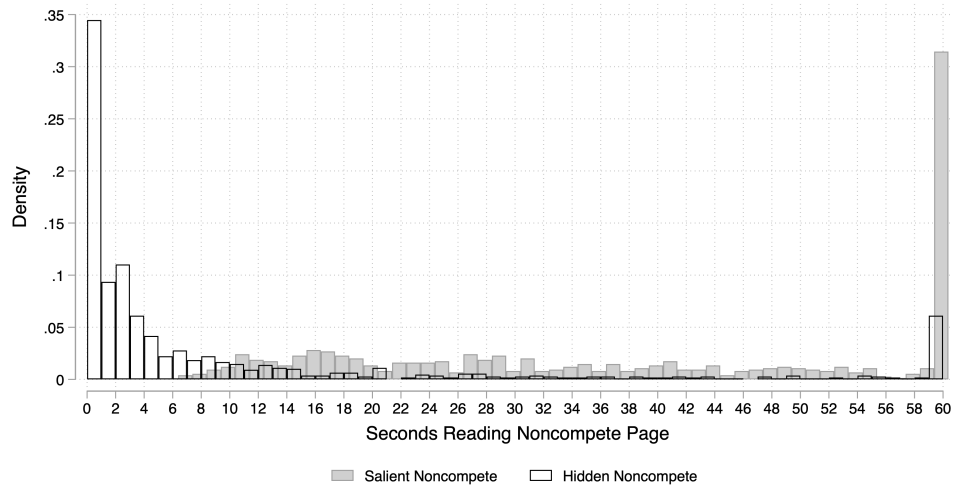
R Unweighted Results

Figure R.1: What unweighted percentage opened the contract, signed it, and completed the task?



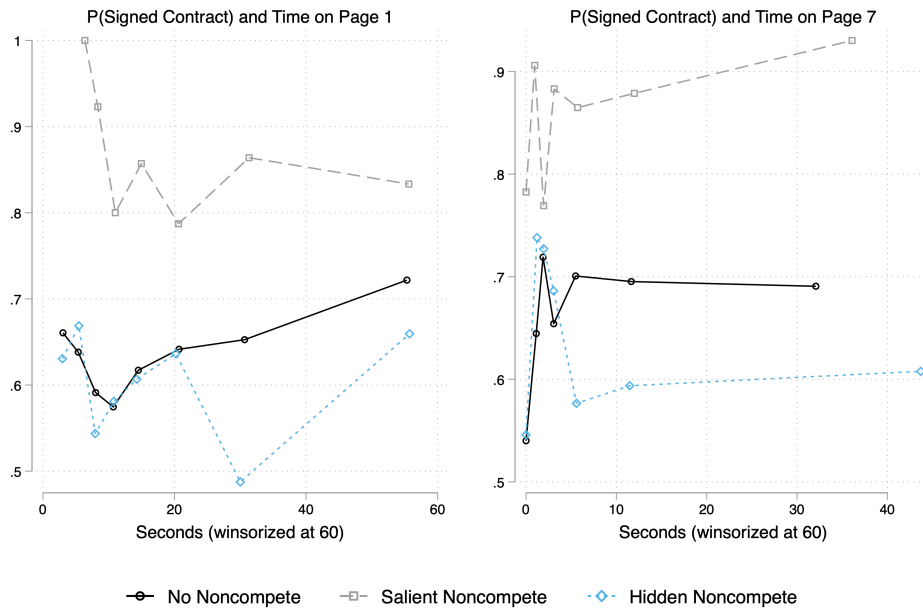
Notes: This figure shows the unweighted probabilities of opening the contract, signing the contract, and completing Firm A's task, according to the noncompete treatment condition.

Figure R.2: Unweighted Histogram of Time Spent Reading Noncomplete Page



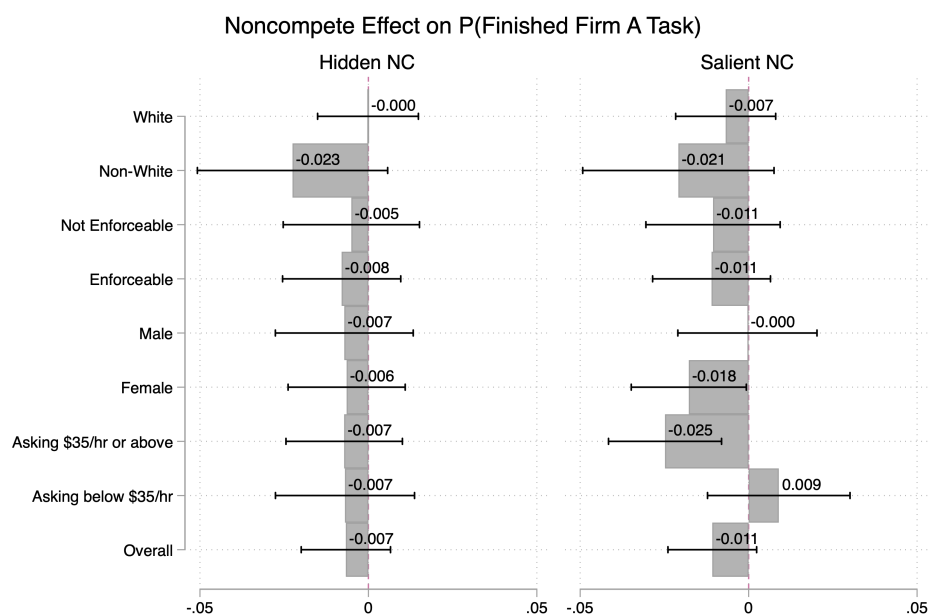
Notes: Results are unweighted. Reading variables are winsorized at 60 seconds.

Figure R.3: Unweighted Probability of Signing Contract Given Reading on Pages 1 and 7



Notes: Reading variables are winsorized at 60 seconds.

Figure R.4: **Unweighted Heterogeneity in the Effect of Noncompetes 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, and then Equation (2) is estimated on that subsample, unweighted, and the coefficients and 95% confidence intervals on the hidden and salient noncompetite variables are shown.

Table R.1: Unweighted Phase 1 3 x 2 Treatment Effects on Contract Opening, Signing, and Completion

	(1) ℓ (Opened Contract)	(2) ℓ (Signed Contract)	(3) ℓ (Completed Task)
Offered Hidden Noncompete, \$60/hr	.076*** (.012)	.057*** (.01)	.053*** (.0095)
Offered Hidden Noncompete, \$25/hr	.0066 (.012)	-.0015 (.0094)	-.0033 (.0085)
Offered No Noncompete, \$60/hr	.08*** (.012)	.067*** (.01)	.064*** (.0097)
Offered Salient Noncompete, \$60/hr	-.00099 (.012)	.048*** (.01)	.045*** (.0094)
Offered Salient Noncompete, \$25/hr	-.062*** (.011)	-.0075 (.0093)	-.0031 (.0085)
Baseline Mean (No Noncompete, \$25/hr)	0.19	0.12	0.09
Weighted	N	N	N
<i>Differential Wage Effects (\$60-\$25):</i>			
Hidden vs. No Noncompete	-0.01 (0.02)	-0.008 (0.01)	-0.007 (0.01)
Salient vs. No Noncompete	-0.02 (0.02)	-0.01 (0.01)	-0.02 (0.01)
Salient vs. Hidden	-0.009 (0.02)	-0.004 (0.01)	-0.009 (0.01)
Observations	13,851	13,851	13,851
R ²	.028	.021	.02

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. This table repeats the weighted Phase 1 3 x 2 specification without inverse-probability weights. The omitted cell is no noncompete with a 25 dollar per hour Firm A wage offer. Models include strata and batch fixed effects; heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Indicator coefficients are percentage points. Joint test p-value tests whether the hidden-by-high-wage and salient-by-high-wage interactions are jointly zero.

Table R.2: **Unweighted Phase 1 Effects on Contract Opening, Signing, and Task Completion**

	(1) 1(Opened Contract)	(2) 1(Signed Contract)	(3) 1(Completed Task)
Offered Salient Noncompete	-.072*** (.0082)	-.013* (.0072)	-.011 (.0067)
Offered Hidden Noncompete	.0013 (.0087)	-.0056 (.0073)	-.0067 (.0068)
Firm A Wage Offer	.002*** (.0002)	.0017*** (.00017)	.0016*** (.00016)
Weighted No-Noncompete Mean	N 0.23	N 0.15	N 0.12
Salient - Hidden	-0.07*** (0.008)	-0.008 (0.007)	-0.004 (0.007)
Salient Wage Equivalent	35.65*** (5.3)	7.79* (4.3)	6.72 (4.2)
Hidden Wage Equivalent	-0.67 (4.4)	3.24 (4.3)	4.17 (4.2)
Observations	13,851	13,851	13,851
R ²	.028	.021	.019

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. This table repeats the weighted specification without inverse-probability weights. The omitted group is no noncompete. Models include strata and batch fixed effects; heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. No-Noncompete Mean is the omitted-group mean. Wage offers and wage equivalents are dollars per hour; indicator coefficients are percentage points.

Table R.3: **Unweighted Phase 1 Task Completion by Page 7 Reading-Time Quartile**

	(1)	(2)	(3)	(4)
Dependent Variable:	1(Completed Firm A Task)			
Sample:	Quartile of Seconds on p.7			
	1st	2nd	3rd	4th
Offered Salient Noncompete	.18*** (.04)	.23*** (.049)	.082 (.051)	.26*** (.066)
Offered Hidden Noncompete	-.037 (.037)	.095* (.053)	-.13*** (.045)	-.054 (.053)
Firm A Wage Offer	.002** (.00091)	.0031** (.0013)	.0042*** (.0012)	-.0012 (.0014)
Weighted	N	N	N	N
No-Noncompete Mean	0.46	0.52	0.61	0.59
Min Seconds	0	.74	2.66	10.47
Max Seconds	0	2.63	10.43	3939.09
Observations	1,048	641	680	517
R^2	.17	.19	.21	.24

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table repeats the weighted reading-time-quartile specification without inverse-probability weights. The sample reached page 7; columns are quartiles of page 7 reading time among hidden-noncompete workers, with Min and Max in seconds. The outcome is completing Firm A's task. The omitted group is no noncompete. Models include strata and outreach-date fixed effects; heteroskedasticity-robust standard errors are in parentheses. Hidden noncompetes appear on page 7; salient noncompetes appear on page 1. Indicator coefficients are percentage points.

Table R.4: Unweighted Phase 1 Effects on Wage Bargaining by Contract-Reading Sample

	(1)	(2)	(3)	(4)
Dependent Variable:	1(Wage Bargaining)			
Sample:	Read p.7 for ---			
	Invited	Opened Contract	> 0 seconds	> 15 seconds
Offered Salient Noncompete	.0025 (.0024)	-.0085* (.005)	-.0096 (.0065)	-.026 (.023)
Offered Hidden Noncompete	.0019 (.0023)	.0037 (.0059)	.0027 (.0077)	.031 (.023)
Firm A Wage Offer	-.00036*** (.000056)	-.0004*** (.00015)	-.0004** (.00019)	-.00053 (.00052)
Weighted	N	N	N	N
No-Noncompete Mean	0.01	0.02	.021	.0086
Observations	13,851	2,895	1,887	367
R ²	.035	.08	.088	.34

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. This table repeats the weighted bargaining specification without inverse-probability weights. The outcome is a wage-bargaining indicator. Columns use the samples named in the headings. The omitted group is no noncompete. Models include strata and outreach-date fixed effects; heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Wage offers are dollars per hour; indicator coefficients are percentage points.

Table R.5: Unweighted Phase 1 Effects on Firm A Task Completion, Hourly Wages, and Billing

	(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 Noncompete	-.0031 (.0085)	-.0032 (.0085)	-.019* (.01)	-.82 (1.1)	-.71 (1.1)
Offered Hidden Noncompete	-.0033 (.0085)	-.0033 (.0084)	-.01 (.011)	-.12 (1)	.66 (1.1)
High Wage (Firm A)=1	.064*** (.0097)				
Salient × High Wage	-.015 (.013)				
Hidden × High Wage	-.0068 (.014)				
Constant	.093*** (.006)	.093*** (.006)	.16*** (.0076)	47*** (.73)	45*** (.76)
Weighted	N	N	N	N	N
No-Noncompete Mean	0.12	0.09	0.16	46.97	45.57
Observations	13,851	6,927	6,924	1,646	1,646
R ²	.02	.023	.019	.065	.066

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. This table repeats the weighted Firm A wage-bill specification without inverse-probability weights. The omitted group is no noncompete. Columns 1-3 estimate Firm A task completion overall and by wage offer; columns 4-5 condition on completing Firm A's task. Models include strata and outreach-date fixed effects; heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. High Wage is a 60 dollar per hour Firm A offer; indicator coefficients are percentage points.

Table R.6: Unweighted Firm A Noncompetes and Joining Firm B Before and After Reminder

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	$\mathbb{1}(\text{Accepted Firm B})$							
	Before the Reminder				After the Reminder			
Offered Salient Noncompete	-0.0029 (.0058)				-.018*** (.0042)			
Offered Hidden Noncompete	.001 (.0058)				-.027*** (.004)			
Firm A Wage Offer	.0011*** (.00014)				.00041*** (.000092)			
Signed Salient		.061* (.032)	.02 (.029)			-.12*** (.03)	-.11*** (.027)	
Signed Hidden		.047 (.033)	-.0025 (.028)	.043 (.07)		-.2*** (.028)	-.19*** (.026)	-.066 (.062)
Firm A Realized Wage		-.00037 (.00081)	.0014 (.0014)	-.0022 (.0019)		-.00052 (.00072)	-.0011 (.0013)	-.0019 (.0018)
Firm B Wage Offer			.0048*** (.00065)	.0064*** (.0018)			.0038*** (.00061)	.0072*** (.0016)
Inverse Mills Ratio (λ)			.19 (.18)				-.15 (.16)	
Sample	Invited	Pop.	Heckman	Skippers	Invited	Pop.	Heckman	Skippers
Estimator	OLS	2SLS	OLS	OLS	OLS	2SLS	OLS	OLS
Weighted	N	N	N	N	N	N	N	N
No-Noncompete Mean	0.09	0.64	0.64	0.63	0.05	0.39	0.39	0.37
KP RK Wald F		183.58				183.58		
Cragg-Donald Wald F		257.32				257.32		
Heckman: χ^2 (Excl. Inst.)			33.88***				33.88***	
Salient - Hidden	-0.001 (0.006)	0.01 (0.03)	0.02 (0.03)		0.008** (0.004)	0.08*** (0.03)	0.08*** (0.03)	
Salient Wage Equiv.			-4.16 (6.18)				29.36*** (8.50)	
Hidden Wage Equiv.			0.52 (5.93)	-6.64 (11.03)			49.96*** (10.47)	9.19 (8.93)
Observations	13,851	29,005	13,851	314	13,851	29,005	13,851	314
R^2	.018	.57		.33	.017	.25		.38

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table repeats the weighted mobility specifications without inverse-probability weights. The outcome is having an open Firm B contract; the omitted group is no noncompete. Columns 1-4 are pre-reminder and columns 5-8 are post-reminder. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. Column samples and designs are listed in the headings; skippers signed Firm A's contract without reading page 7. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Robust standard errors are in parentheses. Wage offers and wage equivalents are dollars per hour; indicator coefficients are percentage points. Note, the no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample.

Table R.7: Unweighted Firm A Noncompetes and Sharing Firm A Résumés with Firm B Before and After Reminder

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	1(Shared Firm A Resumes with Firm B)							
	Before the Reminder				After the Reminder			
Offered Salient Noncompete	.00059 (.0014)				.00042 (.00068)			
Offered Hidden Noncompete	.0017 (.0015)				.00022 (.00065)			
Firm A Wage Offer	.000045 (.000035)				-.000012 (.000016)			
Signed Salient		.0082 (.012)	.0014 (.013)			.0041 (.0058)	.0014 (.0059)	
Signed Hidden		.017 (.012)	.013 (.012)	.035 (.034)		.0021 (.0054)	.0012 (.0057)	-.012 (.016)
Firm A Realized Wage		-.00019 (.00029)	.0004 (.0006)	.000029 (.00087)		-.00022 (.00014)	-.00017 (.00028)	-.00051 (.00034)
Firm B Wage Offer			.0011*** (.00028)	.0032*** (.00091)			-.000049 (.00013)	-1.4e-06 (.00023)
Inverse Mills Ratio (λ)			.084 (.077)				.011 (.036)	
Sample	Invited	Pop.	Paid by Firm A	Skippers	Invited	Pop.	Paid by Firm A	Skippers
Estimator	OLS	2SLS	Heckman	OLS	OLS	2SLS	Heckman	OLS
Weighted	N	N	N	N	N	N	N	N
No-Noncompete Mean	0.00	0.03	0.03	0.04	0.00	0.01	0.01	0.01
KP RK Wald F		183.58				183.58		
Cragg-Donald Wald F		257.32				257.32		
Heckman: χ^2 (Excl. Inst.)			33.88***				33.88***	
Salient - Hidden	-0.001 (0.002)	-0.008 (0.01)	-0.01 (0.01)		0.0002 (0.0007)	0.002 (0.006)	0.0002 (0.006)	
Salient Wage Equiv.			-1.26 (11.94)				28.68 (143.56)	
Hidden Wage Equiv.			-12.57 (11.96)	-10.95 (10.88)			24.62 (134.06)	-8934.45 (1.45e+06)
Observations	13,851	29,005	13,851	314	13,851	29,005	13,851	314
R ²	.0092	.041		.38	.0085	.01		.24

Notes: Stars denote p-values: * p<0.10, ** p<0.05, *** p<0.01. This table repeats the weighted résumé-sharing specifications without inverse-probability weights. The outcome equals one if the worker shared at least one Firm A résumé with Firm B; non-joiners are coded as non-sharers. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. The omitted group is no noncompete. Columns 1-4 are pre-reminder and columns 5-8 are post-reminder. Column samples and designs are listed in the headings; skippers signed Firm A's contract without reading page 7. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. Robust standard errors are in parentheses. Indicator coefficients are percentage points. Note, the no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample.

Table R.8: **Unweighted Firm A Noncompetes and Earnings from Firms A and B**

	(1)	(2)	(3)	(4)	(5)	(6)
	Firm A + B Compensation					
Offered Salient Noncompete	-.15** (.062)	-.91** (.39)				
Offered Hidden Noncompete	-.12* (.062)	-.75* (.39)				
Signed Salient			-3** (1.5)	-3.7** (1.5)		
Signed Hidden			-3.3** (1.4)	-3.2** (1.4)	-.12** (.052)	-6.1** (2.7)
Inverse Mills Ratio (λ)				1.8 (8.8)		
Sample	Invited	Invited	Pop.	Paid by Firm A	Skippers	Skippers
Estimator	PPML	OLS	2SLS	Heck- man	PPML	OLS
Weighted	N	N	N	N	N	N
No-Noncompete Mean	6.74	6.74	53.91	53.91	54.83	54.83
KP RK Wald F			301.54			
Cragg-Donald Wald F			370.34			
Heckman: χ^2 (Excl. Inst.)				33.51***		
Salient - Hidden	-0.03 (0.06)	-0.16 (0.37)	0.37 (1.44)	-0.47 (1.43)		
Observations	13,851	13,851	29,005	13,851	321	321
R^2		.01	.81			.046

Notes: Stars denote p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table repeats the weighted earnings specifications without inverse-probability weights. Earnings are experimental payments from Firms A and B only. In the Sample row, Pop. denotes the full 29K recruiter population; Invited denotes the 14K recruiters who received a Firm A offer. The omitted group is no noncompete. All models include strata fixed effects; columns 1-2 also include outreach-date fixed effects. Skipper specifications use simplified strata-component fixed effects. Heteroskedasticity-robust standard errors are in parentheses. Salient means disclosed before contract review and placed on page 1; hidden means placed on page 7 and not mentioned in outreach. PPML coefficients are log points; other coefficients are dollars. Offered rows use randomized assignment; Signed rows use realized Firm A contract status. Note, the no-noncompete mean in the 2SLS models is conditional on working for Firm A and being in the estimation sample.

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