Increasing the Demand for Workers with a Criminal Record*

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Abstract

State and local policies increasingly restrict access to job applicants' criminal records, but without addressing the underlying reasons that employers conduct criminal background checks. Employers may thus still ask about an applicant's criminal record later in the hiring process or make inaccurate judgments about an applicant's criminal record based on demographic characteristics. In this paper, we use a field experiment conducted in partnership with a nationwide staffing platform to test a range of alternative policies that more directly address the reasons that employers may conduct criminal background checks. The experiment asked hiring managers at nearly a thousand U.S. businesses to make actual hiring decisions under different randomized conditions. We find that 39% of businesses in our sample are willing to work with workers with a criminal record at baseline, which rises to over 50% when businesses are offered crime and safety insurance, a single performance review, a background check covering just the past year, or objective information on the productivity of these workers. Wage subsidies can achieve similar increases but at a substantially higher cost. Based on our findings, the staffing platform modified its user interface to relax the criminal background check requirement and offer crime and safety insurance.

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1. Introduction

Employers are significantly less likely to interview or hire workers with a criminal record (WCs) compared to otherwise similar workers without a record (e.g., Pager, 2003; Holzer et al., 2006; Holzer, 2007; Agan and Starr, 2017). In 2008, for example, the average unemployment rate among formerly incarcerated people—27 percent—was higher than the U.S. unemployment rate for the general population at any point in history, including the Great Depression (Couloute and Kopf, 2018). The limited employment opportunities for WCs exacerbate existing socioeconomic and racial inequalities and likely contribute to the high rates of recidivism among recently released individuals (e.g., Yang, 2017; Schnepel, 2018).

In an attempt to mitigate the scarring effects of a criminal record, over 150 cities and counties and 35 states have adopted "Ban the Box" (BTB) policies that delay questions about a job applicant's arrest and conviction record. These policies are meant to increase hiring rates among WCs by making it more difficult to screen applicants based on criminal history, helping these workers get a foot in the door when seeking employment. However, "Banning the Box" may not address the underlying reasons that employers may choose to screen out WCs, such as the potential for lower expected productivity or higher downside risk for the employer. Employers may therefore still want to ask about an applicant's criminal record later in the hiring process or make inaccurate judgments about an applicant's criminal record based on their race or other demographic characteristics (e.g., Bushway, 2004; Holzer et al., 2006; Stoll, 2009; Agan and Starr, 2018; Doleac and Hansen, 2020).[1]

In this paper, we use a field experiment involving actual hiring decisions at nearly thousand U.S. businesses to test several alternative approaches to increasing WC employment. Each of the alternatives we consider is meant to addresses the underlying reasons that businesses may choose to screen WCs. For example, we offer different levels of crime and safety insurance in an attempt to address the concern that WCs could be more likely to steal or damage company property. We similarly offer objective information on the productivity of WCs to address the concern that WCs may be less productive, on average, than non-WCs. Other alternatives we consider, including past performance reviews and background checks covering just the most recent criminal records, are meant to address both sets of concerns. We benchmark the effects of each of these alternatives against the effects of a wage subsidy, a natural but potentially costly approach to increasing employment among WCs.

The partner for our study is a large anonymous online labor platform based in the United

[1] Perhaps as a result of these factors, much of the existing work shows no clear positive effects of BTB policies on the employment of WCs. Jackson and Zhao (2017) find, for example, that a 2010 law in Massachusetts that included a BTB provision had a small negative impact on the employment and earnings of ex-offenders, while Rose (2021) finds negligible impacts of a 2013 Seattle law prohibiting employers from asking about an applicant's arrest and conviction record until after an initial screening. By contrast, Craigie (2020) finds that BTB policies increase the probability of public-sector employment for WCs. In aggregate data that include both those with and without a criminal record, Doleac and Hansen (2020) find that state and local BTB laws decrease employment rates for young, low-skill Black and Hispanic men, while Shoag and Veuger (2016) find that these state and local BTB laws increase employment rates in high-crime counties. In an influential study, Agan and Starr (2018) also show that the white-Black gap in call-back rates increased from 7% to 45% at affected firms following the introduction of a BTB policy in New Jersey and New York City.

States (hereafter, "the Platform"), which traditional businesses use to source workers for temporary staffing. On the Platform, businesses list jobs with required qualifications and workers who meet the criteria can accept those jobs on a first come, first serve basis. After a worker accepts a job, businesses do not perform any additional screening. Cancellation of assignments is also costly and rare, occurring in less than 1% of cases. Allowing a particular type of worker to "accept a job" is therefore equivalent to extending a job offer to that worker. Like many peer staffing services, up to 30% of applicants interested in accepting work on the Platform are currently screened out by a comprehensive criminal background check and are unable to accept any of the posted jobs.

The Platform's labor market design allows us to ask hiring managers to make incentive-compatible choices over *actual hiring decisions*, as opposed to the callback or interview decisions considered in past work. Hiring managers on the Platform are already familiar with submitting criteria for workers who can accept their jobs. We truthfully informed these managers that their responses constituted high stakes decisions, as their choices could be implemented and determine whether WCs would be allowed to accept their jobs in the future. For example, if a hiring manager indicated that they would be willing to work with WCs under a certain insurance policy, then the Platform could allow WCs to accept jobs posted by that manager after the insurance policy is made available. The high stakes nature of these choices is not just theoretical — the choices that hiring managers made during the experiment actually *did* affect whether WCs could accept their posted jobs, as we describe below.

In the experiment, the Platform asked hiring managers at nearly 1,000 businesses whether they would allow WCs to accept their jobs given the availability and level of wage subsidies, crime and safety insurance, past performance history, and more targeted screening on criminal records. Starting with the baseline level of demand for WCs in our sample, we find that a sizable share of businesses, 39%, are willing to work with WCs without additional incentives or conditions. The level of demand, still without additional incentives or conditions, increases to 45% for jobs that do not involve customer interactions and 51% for jobs that do not involve high-value inventory, consistent with businesses perceiving risks related to customer safety or inventory theft. We also find that the share of businesses willing to work with WCs increases to 68% if the Platform is having a hard time filling a job, consistent with businesses being more likely to consider non-traditional workers when jobs are hard to fill.

Turning to our main results, we find that the share of businesses willing to work with WCs further increases by at least 10 percentage points when businesses are offered a modest level of crime and safety insurance, a single performance review, or a background check covering just the past year. Wage subsidies can achieve similar increases but only at relatively high subsidy levels that may be cost prohibitive.[2] For example, crime and safety insurance covering damages up to $5,000 increases the level of demand for WCs by 12 percentage points, equivalent to the effects of an

[2]The share of businesses willing to work with WCs further increases by approximately 2.1% for every 10% increase in the offered wage subsidy, broadly consistent with elasticities discussed as reasonable for low-wage workers in, for example, Katz (1996). We show in Section 7 that our elasticity estimate implies that all of the policies we consider can increase the demand for WCs at one-half to one-tenth the cost of wage subsidies under reasonable assumptions.

80% subsidy according to a linear extrapolation of our experimental subsidy estimates. Requiring WCs to have satisfactorily completed one prior job on the Platform similarly increases the level of demand by 13 percentage points, again roughly equivalent to the effect of an 80% subsidy. Limiting the pool of WCs to those who have maintained a clean record for at least one year increases the level of demand by 22 percentage points, greater than the effect of a 100% wage subsidy.

The final option we consider is providing hiring managers with objective information on the performance of WCs on the Platform. We exploit the fact that some WCs inadvertently access the Platform before their background screening results are known, allowing us to compare the performance ratings of WCs and non-WCs in their first jobs on the Platform. We find that hiring managers underestimate the performance of WCs, in terms of both high- and low-performance ratings. Providing objective information on the true share of high-performance ratings received by WCs leads to more accurate beliefs and increases WC hiring by 7 percentage points, equivalent to the effect of a 45% wage subsidy. Providing objective information on the share of low-performance ratings, typically resulting from absenteeism, also leads to more accurate beliefs but only increases WC hiring by a statistically insignificant 1.5 percentage points.[3]

Based on our findings, the Platform carried out a staged roll-out to relax the criminal background check and allow WCs to accept jobs. First, the Platform *did* allow WCs to accept the jobs of those businesses that responded positively when asked whether WCs could perform their jobs under current Platform conditions. Second, the Platform viewed the demand estimates as justification for changing its user interface nation-wide so that thousands of businesses posting new jobs could have the option of accepting WCs regardless of their choices or participation in the experiment. These changes are being rolled out in stages. The Platform created the option for all businesses to allow WCs to accept their jobs with up to $1 million covered by crime and safety insurance, one of the most promising randomized conditions tested in this study. Eventually the Platform plans for the inclusion of WCs to be the default option, and for their exclusion to incur an additional cost for businesses. To date, demand from our study participants combined with the staged roll-out has led to approximately 6,700 jobs being available to WCs. This expansion in the number of jobs available to WCs opens new questions for future research, including the evolution of demand for WCs as businesses gain experience working with WCs, and long-term employment opportunities created for workers that accept jobs as a result of this policy change.

Beyond demonstrating that a range of policies can increase the demand for WCs, our paper provides new evidence on why businesses may be less likely to work with WCs compared to otherwise similar non-WCs. Several of our results, including the large demand response to crime and safety insurance and the even larger response to insurance among businesses whose jobs involve customer interaction or high-value inventory, suggest that businesses are particularly sensitive to the downside risk of hiring WCs. Other results, including the large response to objective information on the

[3] Our estimates are sizable compared to other information treatment experiments. Bursztyn et al. (forthcoming) find that correcting beliefs about male support for female labor market participation in Saudi Arabia increases the likelihood that husbands sign up their wives for job platform by 9 percentage points, or 36%, while Allcott (2011) find that a letter on energy use comparisons reduces consumption by 2%.

productivity of WCs, are consistent with the view that some businesses view WCs as less productive on average than otherwise similar non-WCs. The positive effects of the wage subsidies, performance screening, and screening of the most recent records are consistent with either channel.[4]

Our paper builds on important work by Holzer (2007), Holzer et al. (2007), and Hunt et al. (2018) measuring the demand for WCs using non-incentivized surveys of employers. In a survey of 107 firms, for example, Hunt et al. (2018) find that employers report being more willing to hire WCs if there are wage subsidies, certificates of validated work performance history, or guaranteed replacement workers. These employers also report that "any violent felony conviction" and the "skills to get the job done" are their two most serious concerns with hiring WCs in the absence of these policies in these studies. However, the hypothetical and low-stakes nature of these surveys makes it difficult to know whether employers are expressing their true preferences or just their aspirations. We add to this literature by measuring the demand for WCs using the actual hiring choices of nearly a thousand U.S. businesses under different counterfactual policies.

The remainder of the paper is organized as follows. Section 2 describes the experimental context and design. Section 3 presents our baseline estimates of the labor demand for WCs with and without wage subsidies. Section 4 presents results for our primary experimental interventions, Section 5 presents results from the information experiment embedded in the Platform's hiring flow, Section 6 discusses alternative explanations for our results, and Section 7 concludes. The Online Appendix provides additional results and details of the experimental design.

2. Context and Experimental Design

2.1. The On-Demand Staffing Platform

The context for our study is a leading online labor platform that thousands of traditional businesses use to source workers for temporary staffing, often in large numbers. Businesses use the Platform to fill a wide range of entry-level jobs in sectors that report being more willing to hire WCs, such as general labor and transportation, as well as entry-level roles in customer-facing or administrative sectors that are traditionally more averse to hiring WCs (e.g., Holzer et al., 2004; Raphael, 2010; Yang, 2017; Schnepel, 2018). The Platform is hosted on the internet, but the work they support generally does not involve computers or the internet, nor does it require a college degree or significant prior experience. The variety of job types and focus on entry-level positions provides an ideal setting for estimating the demand for WCs.

The Platform's labor market allows us to ask businesses to make incentive-compatible choices over hiring decisions. Businesses post job listings that include a job description, the hourly wage, and qualifying criteria. For example, some listings require that workers have experience driving a truck or are comfortable with heavy lifting. Businesses do not decide whether to work with individual

[4] Our results also contribute to the literature in personnel and organizational economics on hiring (see Oyer and Schaefer (2011) for a review of recent work in this area). While past work explores the option value of hiring high-variance workers for long-term positions (Lazear, 1998; Bollinger and Hotchkiss, 2003), we explore how to protect businesses from the perceived downside potential of hiring disadvantaged workers for short-term positions.

workers. Posted job opportunities are sent to the pool of workers registered on the Platform who meet the qualifying criteria. Workers then have the option to accept or reject these job postings with no penalty. Upon accepting, the worker is matched to the job and the posting is withdrawn from other workers. Matches are typically made within a few hours of the job posting. The business must pay a cancellation fee of 50% of the wage bill to unmatch from the worker within 12 hours of the start time. Less than 1% of matches are canceled in practice and the majority of workers who accept a first job from a business go on to accept other jobs posted by the same business.[5]

By asking businesses to make decisions about what workers they would allow in their pool, we are therefore asking businesses to make incentive-compatible choices over *actual hiring decisions*, as opposed to over callback or interview decisions that have generally been considered in previous work and may differ from the decision of who to hire (Jarosch and Pilossoph, 2019). When a hiring manager states they are willing to accept WCs through our experiment, the Platform then legally has permission to extend job openings to WCs who in turn accept jobs on a first come, first serve basis.

The Platform collects data on worker performance from businesses, a feature that allows us to provide businesses with information about the performance of WCs. At the end of each job, the business's hiring manager is asked to rate each worker's overall performance on a scale of 1 to 5. Hiring managers also rate workers on specific attributes such as timeliness, cooperation, and quality of work. In practice, hiring managers complete the overall rating after 86% of jobs, but only complete the more comprehensive review after 8% of jobs. Eighty-five percent of the overall ratings are perfect 5-stars and approximately 5% are 1- or 2-stars, typically used for absenteeism and incomplete work. No-shows comprise an additional 4% of the overall job assessments. We thus define high-performance using the median share of perfect 5-star ratings across all jobs as a cutoff (85%), classifying workers as having a share of 5-star ratings above or below this cutoff. Similarly, we define low performance as those workers with an above-median (5%) share of low ratings or no-shows. The intuition for considering the two ends of the performance spectrum separately is that they are only weakly correlated—a worker can perform at a high level conditional on completing the job while also exhibiting high no-show rates. Some businesses might care more about mitigating poor performance and absenteeism than about ability to perform well.

Like many other labor platforms for independent contract workers (e.g., Uber, Lyft), up to 30% of Platform applicants are currently screened out by a criminal background check. The researchers' collaboration with the Platform grew out of a series of conversations between the researchers and the Platform's Chief Executive Officer, Chief Technology Officer, Board Members and other top executives and managers. The Platform's performance data suggest that WCs could contribute productively to the marketplace and expand the pool of available workers, and several states and localities had recently enacted legislation limiting the use of background checks for businesses in those areas. The Platform's executives also felt that expanding employment opportunities for WCs

[5]Workers also have the possibility of joining the businesses as full-time employees through the Platform. While only 2% of workers who completed at least one job formally transitioned to full-time W2 employment through the Platform in 2019, it is likely that more transitioned through other channels.

was important for corporate social responsibility, particularly in light of the Black Lives Matter movement. The Platform was thus actively searching for ways to modify, reduce, or eliminate its use of criminal background checks, and agreed to partner with the research team to understand the potential barriers to hiring WCs in their context.

2.2. Experimental Design

The experiment was designed and implemented through an intense multi-year collaboration between the research team and top executives and managers at the Platform following the initial conversations discussed above. The goal of the collaboration was to understand the potential barriers to including WCs in the pool of independent contract workers on the Platform, so that the Platform could relax the criminal background check requirement and provide opportunities to a broader set of workers. The Platform's top executives and managers piloted the experimental conditions and phrasing, while the Platform's general counsel closely scrutinized and edited the conditions to ensure that the hiring managers' responses could legally determine whether WCs would be allowed to accept their business' jobs in the future (hence, ensuring the high-stakes nature of the responses provided during the experiment). The Platform's general counsel also ensured that that the proposed policies were in compliance with relevant local, state, and federal laws.

A central feature of the experiment is that hiring managers make incentive-compatible choices over actual hiring decisions under different randomized conditions. As discussed above, the Platform's labor market features a matching process where workers who meet the posted job requirements are matched on a first come, first serve basis, with no additional screening after the initial matching process. In addition, hiring managers on the Platform are familiar with submitting criteria for workers who can accept their jobs, making the high-stakes nature of their choices both apparent and natural in our context. These institutional features, as well as the input of the Platform's general counsel, allowed the Platform to truthfully inform hiring managers that their responses during the experiment constituted high-stakes decisions that could determine whether WCs would be allowed to accept their jobs in the future.

The incentive-compatible nature of the hiring managers' choices was reinforced by two actions taken by the Platform following the experiment. First, the Platform now allows WCs to accept the jobs of those businesses that responded positively when asked whether WCs could perform their jobs under current Platform conditions, if there is a pool of WCs in the business' location. Second, the Platform is using the encouraging results from our collaboration to change their policies for all business on the Platform, regardless of their choices or participation in the experiment. These changes are being rolled out in the three stages. First, the Platform recently modified its user interface so that thousands of additional businesses posting new jobs have the option of actively accepting WCs with up to $1 million covered by crime and safety insurance, one of the most promising randomized conditions tested in this study. Second, the Platform is planning to change the default option to be allow WCs to accept the jobs businesses post, while retaining the crime and safety insurance, in the coming months. Finally, the Platform is planning on only allowing businesses to exclude WCs from their pool of independent contracts if they pay an additional fee. To date, demand from our study participants combined with new reforms has led to the addition of approximately 6,700 jobs available to WCs.

Incentive-compatibility can be judged by whether the questions are perceived by respondents as having potential to affect their outcome, with the exact probability of a choice is implemented generally mattering relatively little (Carson and Groves, 2007; Charness et al., 2016). The Platform carefully crafted the initial outreach to businesses to elicit truthful answers: "Please share your truthful and considered views – they matter to us," and the Platform's Executives and Board Members were especially committed to making it possible for those who expressed interest in hiring WCs, could do so. As a result, the questions were designed to change practice: nearly one hundred choices made during the experiment were directly implemented and every participant has or will be affected by the Platforms' roll-out decisions in some way.[6]

We leverage this high-stakes setting to test several approaches to increasing WC employment. Each of the alternatives we consider is meant to addresses the underlying reasons that businesses may choose to screen out WCs. We began by asking hiring managers about their willingness to accept WCs under one of several randomly assigned wage subsidy levels (0% or one of several positive levels) to establish the baseline level of demand and provide a benchmark for the other randomized treatments. The randomly assigned subsidy level remained in place throughout all subsequent questions. We then asked hiring managers about their willingness to accept WCs under the main experimental conditions, including different levels of crime and safety insurance and past performance reviews and background checks covering only the most recent criminal records. Then came a series of descriptive questions about the hiring practices at the firm and the types of jobs posted on the Platform. The experiment concluded with an information treatment experiment motivated by the large dispersion in prior beliefs about the performance of WCs on the Platform, which is described in detail in Section 5.

The remainder of this section summarizes the most important details of the experiment, also detailed in Table 1 and Appendix Table A.1. We begin by describing how the Platform contacted hiring managers, before describing each of the main experimental conditions and subsample comparisons.

Outreach. The experiment began with the Platform emailing all 7,450 hiring managers who had been active in listing positions on the Platform in the last 16 months. The Platform did not email hiring managers who had joined within the last 3 months at the request of the company's account managers. The Platform sent the email from a Platform-branded account using their own

[6]Methodologically, we build on Mas and Pallais (2017), Low (2017), and Kessler et al. (2020) to generate incentive-compatible responses in field experiments. Mas and Pallais (2017) examine the choice of applicants regarding schedule flexibility over jobs within a call-center. Low (2017) and Kessler et al. (2020) examine hypothetical candidates (for dating and hiring, respectively) with randomized attributes, where respondents are truthfully informed that their decisions will affect who can accept their jobs. In the experiment, businesses make multiple decisions about hiring under different randomized conditions, where the Platform truthfully informed them that their decisions may affect whether WCs are included among the workers who can accept their jobs and under what conditions. Our approach is also similar to the "strategy method" in lab experiments (Brandts and Charness, 2011), where players make multiple conditional decisions (e.g., a different decision for each information set) and one decision is potentially randomly chosen to count for pay.

signature ("Sincerely, [Platform] Management") and logo. They contacted hiring managers up to five times between March 6, 2020, and April 11, 2020. The email messages stated: "We are considering expanding the pool of [workers] who can perform the jobs that you post, and we want your guidance." The Platform offered a $50 or $35 cash gift for complete answers to underscore the value of thoughtful and considered responses, as well as to motivate businesses to complete all questions. Such cash transfers are a standard practice for the Platform when requesting input from hiring managers to make Platform design decisions.

The initial emails did not mention WCs, and hiring managers were not aware that they were part of a randomized study. In total, 1,095 hiring managers from 913 businesses completed the hiring flow questions, or 14% of the hiring managers contacted. Conditional on opening the email communication, the completion rate was 86%, with 91% of managers completing all questions conditional on reaching the first question related to WCs. Eighty percent of the hiring managers in our sample also report having authority to unilaterally allow WCs to be perform the jobs they post or to significantly influence this decision. Our results are qualitatively unchanged if we calculate upper and lower bounds of all treatment effects that account for early attrition or restrict the sample to the subset of hiring managers with unilateral authority to allow WCs to be hired or either the unilateral authority or the power to influence this decision.[7]

Following a series of short introduction questions, the Platform showed participating hiring managers the following message:

> We are considering expanding our pool of [Platform Workers] to include individuals that have a criminal record. We want to learn whether this expanded pool would suit your needs.
>
> If you indicate that you're interested in connecting with [Platform Workers] with a criminal record, then (and only then) your choice could affect whether these [Platform Workers] are able to accept jobs you post. These individuals would be at most 5% of your pool of possible matches.

The Platform then asked participating hiring managers about their willingness to work with WCs under different randomized conditions, where randomization occurred at the business level to ensure that hiring managers at the same business were not given conflicting options.

Baseline Demand. We measure the baseline demand for WCs with no additional incentives or conditions by simply asking hiring managers whether their business would permit WCs to accept their jobs:

> Would you permit [Platform Workers] with a criminal background to perform jobs you post?

[7]The experiment asked hiring managers "Would you currently have the authority to permit a [Platform Worker] with a criminal record to perform the jobs you post?" Ignoring the 9% of hiring managers who said "Not Sure," 53% of managers answered "Yes," 27% said "My opinion would matter, but I would not be the final decision-maker," and 20% said "No." Relative to the authority of plant managers in manufacturing to make various decisions on their own (Bloom et al., 2012), our managers have a high degree of authority to hire WCs.

We asked 1/5 of hiring managers this question, meant to measure demand for WCs under the Platform's current conditions and establish a baseline for a wage subsidy of 0%. Hiring managers were given the option of selecting "Yes", "Only if it's hard to fill my jobs," or "No". Answering "Yes" to this question immediately extended permission to the Platform to allow WCs to accept the client's job posting, without any policy changes or conditions being met.

Wage Subsidies. We measure demand for WCs under different wage subsidies by asking hiring managers whether their business would permit WCs to accept their jobs under one of several randomly assigned wage subsidy levels:

> If the [Platform] gave you a [Wage Subsidy] discount for [Platform Workers] with a criminal record, would you permit such [Workers] to perform jobs you post? This means you would only pay (100 - [Wage Subsidy]) of the wage for those with a criminal record. All [Platform Workers] would still receive the full pay amount after the discount (the [Platform] would pay the difference).

The wage subsidy levels were 5%, 10%, 25%, 50%, and 100%, randomly assigned with probabilities 1/5, 1/10, 1/10, 1/5, 1/5, and 1/5, respectively. The wide range of randomized subsidy levels allows us to trace out a labor demand curve with minimal assumptions, as well as explore whether there are non-linear effects for very small or very large wage subsidies. We cover a range of economically relevant subsidy levels, with the Federal Work Opportunity Tax Credit (WOTC) currently offering a 25% wage subsidy to firms who employ WCs for at least 120 hours in their first year of employment and a 40% wage subsidy to firms who employ WCs for at least 400 hours in their first year. For exposition, we pool the 5% and 10% subsidy levels, which results in a uniform number of observations across values displayed.

Managers were randomly assigned to no subsidy (20% chance) or to one of the 5 wage subsidy levels, not to both.

Crime and Safety Insurance. We measure the effect of crime and safety insurance by asking hiring managers if, at a given subsidy level, their business would permit WCs to be accept their jobs under one of several randomly assigned insurance levels:

> If the [Platform] could cover damages up to [Crime and Safety Insurance Cap] related to theft or safety incurred by [Platform Workers] with a criminal record, would you permit such [Workers] to perform jobs you post? The [Platform] would still give you a [Wage Subsidy] discount, but no other supplementary policies would apply.

The randomly assigned insurance levels were \$1,000, \$5,000, \$100,000, and \$5 million, randomly assigned with probabilities of 1/6, 1/6, 1/3, and 1/3, respectively. These randomized insurance levels cover a wide range of economically relevant values. The U.S. Federal Bonding Program, for example, offers an insurance bond of \$5,000 to provide insurance against liability for relatively less serious crimes like robbery or theft. The highest level of insurance in our experiment, $5 million, would also provide liability against much more serious crimes like sexual assault and murder.[8] Crime and safety insurance directly addresses the concern that a WC might act violently towards coworkers or customers. For exposition, we pool the $1,000 and $5,000 insurance levels, which results in a uniform number of observations across values displayed.

Screening Based on Performance History. We measure the effect of performance history on the Platform by asking hiring managers if, at a given subsidy level, their business would permit WCs to accept their jobs under one of several randomly assigned job histories:

> If the [Platform] required [Platform Workers] with a criminal record to have satisfactorily completed [Performance History] job(s), receiving more than 85% 5-star reviews, would you permit such [Workers] to perform jobs you post? The [Platform] would still give you a [Wage Subsidy] discount, but no other supplementary policies would apply.

The randomly assigned job histories consisted of 1, 5, and 25 jobs, randomly assigned with 1/3 probability each. These randomized job histories again cover a wide range of economically relevant values. Pallais (2014) shows that workers having 1 prior job substantially increases the chance of getting hired on oDesk, motivating the inclusion of this job history in our experiment, while the highest value of 25 jobs corresponds to an above the 90th percentile of past performance history on the Platform. Performance screening could potentially address business concerns about both productivity and on-the-job crime.

Screening Based on Criminal Record History. We measure the effect of selectively screening on criminal records by asking hiring managers if, at a given subsidy level, their business would permit WCs to accept their jobs if the WC committed their last offense at least 1, 3, or 7 years ago, with these values randomly chosen with 1/3 probability each. We chose these randomized values because the probability of criminal re-offending is particularly high in the first two years post-incarceration, while background checks are often limited to considering criminal convictions within the last 7 years.

We also measure the effect of selectively screening crimes by asking hiring managers if, at a given subsidy level, their business would permit WCs to accept their jobs if they were convicted for a distinct category of crimes, including (1) a property/financial felony, (2) a violent felony, (3) a substance-related felony, (4) a property/financial misdemeanor, (5) a violent misdemeanor, or (6) a substance-related misdemeanor. These categories include a wide variety crimes, but do not encompass all possible convictions and do not include arrests. We therefore do not expect these crime-type specific results to aggregate to our baseline results that include all arrest and conviction types.

[8] We selected the $5 million cap to cover plausible damages from sexual harassment cases. For example, an Uber driver was ordered to pay $8.2 million for sexually assaulting a customer in December 2019 (`https://molawyersmedia.com/2019/12/17/uber-assault-case-results-in-8-2-million-judgment/`) and Airbnb currently offers insurance up to $1 million to hosts.

Objective Performance Information. We measure how providing hiring managers with objective performance information about the average productivity of WCs shifts businesses' beliefs about the performance of WCs and subsequently causes them to revise their hiring choices. This could address business concerns that WCs may be less productive than other workers. To provide objective information, we exploit the fact that some WCs inadvertently access the Platform before their background screening results are known, allowing us to compare the performance ratings of WCs and non-WCs in their first jobs on the Platform.

We elicit baseline performance beliefs about WCs in an incentive-compatible manner by using a guessing game about the relative performance of WCs on the Platform and rewarding accuracy. We used an objective measure of WC performance to reward accuracy, exploiting the fact that 265 WCs inadvertently completed their first job on the Platform in 2019 before their criminal record tests were registered in the system. Rewards in the guessing game ranged between \$2 and \$10 for an answer within 5% of the truth, where we found no difference in respondent accuracy across the reward amounts in unreported results. For approximately one half of the participants, we asked the following question about high-performance:

> In 2019, 86% of jobs on the [Platform] resulted in a 5-star rating. What percentage of jobs completed by [Platform Workers] with a criminal record do you think would result in a 5-star rating on the [Platform] or a similar platform? If your guess is within 5% of the truth, we will send you an additional [Bonus] reward!

We asked the other half of participants about low-performance:

> In 2019, 5% of jobs on the [Platform] resulted in either a no-show or low rating (1 or 2 stars). What percentage of jobs completed by [Platform Workers] with a criminal record do you think would result in a no-show or low rating on the [Platform] or a similar platform? If your guess is within 5% of the truth, we will send you an additional [Bonus] reward!

We then randomly assigned half of these participants to an information treatment group. Participants who initially made guesses about high-performance received objective information about high-performance:

> The truth is that 87% of jobs completed by [Platform Workers] with a criminal record resulted in a 5-star rating on the same or a similar platform – actually better than everyone else. Please take some time to read and understand this information carefully. When you are ready, proceed to the next screen.

Similarly, half of the participants who initially made guesses about low-performance received objective information on low-performance:

> The truth is that only 3% of jobs completed by [Platform Workers] with a criminal record resulted in either a no-show or a low rating (1 or 2 stars) on the same or a similar

> platform – actually fewer no-shows and low ratings than everyone else. Please take some time to read and understand this information carefully. When you are ready, proceed to the next screen.

We then asked all participants, regardless of whether or not they were shown the new objective information, to report their posterior beliefs about the performance of WCs by replicating the same guessing game question. Finally, we allowed participants to revise their answer to the very first question about hiring WCs after eliciting posterior beliefs. By allowing participants to revise their willingness to work with WCs, we can learn how the information about performance impacted hiring decisions.

Heterogeneity by Labor Market Conditions. We also explore heterogeneity across labor market conditions, testing whether particular business concerns are more salient under different market conditions. For all of the questions in the experiment, hiring managers were given the option of selecting "Only if it's hard to fill my jobs," providing a targeted measure of labor market tightness that is specific to each business's context. In addition, we explore the effects of local labor market unemployment, a more traditional measure of labor market tightness, by asking whether a hiring manager would want to work with WCs if the local unemployment rate were to be at a certain level, randomized between 2%, 6%, or 10%. Finally, we estimate results separately for businesses located in counties with above- and below-median unemployment rates as of early March 2020 (e.g., Los Angeles County with 6.6% unemployment vs. San Francisco County with 3.1% unemployment) and for businesses located in counties in the top and bottom three quartiles of COVID-19 rates as of March 2020 (e.g., Cook County, IL (Chicago) vs. Los Angeles County), providing an additional set of estimates related to local labor market tightness.

Heterogeneity by Job Characteristics. Finally, we asked hiring managers about the typical jobs they post, including whether there are any customer interactions or access to high-value inventory, thereby allowing us to explore to what extent businesses are concerned about theft as compared to violence when considering downside risk. We also asked whether their company or organization has a hiring policy related to WCs.

2.3. Motivating Framework

The experimental design described above is motivated by a stylized theoretical framework of how businesses decide whether to work with WCs. The framework formalizes the idea that businesses may see WC status as a potential marker of either expected productivity or the probability of a very negative event for the business. For example, a business may worry that a WC may pack boxes more slowly than a non-WC (i.e., lower productivity) or be more likely to get in a fight with a coworker or customer (i.e., higher downside risk). The purpose of this framework is to help explain how we can use our results to determine why businesses want to obtain potential workers' criminal records, and what policies could be most effective in addressing business concerns and thus increasing WC employment.

Consider a single business deciding whether to work with a single WC. The business's expected profits from working with the WC can be described as a function of expected productivity and the risk of a costly event occurring on-the-job:

$$\pi = y - w - b \cdot \max\{k - I, 0\}$$

where y is the expected productivity of the WC (e.g., the rate at which the worker packs boxes), w is the WC's wage, b is the probability of a bad event occurring as a result of the WC's behavior (e.g. theft), $k \geqslant 0$ is the cost of a bad event (e.g., the value of stolen inventory), and $I \geqslant 0$ is the amount of crime and safety insurance provided. The business has an unobserved shadow value, θ, of not hiring the WC (e.g., there is some probability that the business can fill the slot instead with a non-WC). The business chooses to work with the WC, $H = 1$, when $\pi > \theta$. The first prediction is that wage subsidies increase demand for WCs regardless of expected productivity or downside risk. We are thus able to use the effect of the wage subsidy on demand as a benchmark, comparing its effect to policies that primarily target either expected productivity or downside risk. Our framework yields the following predictions.

Heterogeneity by Labor Market Conditions. When local labor market conditions are such that the business has strong alternative options to hiring WCs, the shadow value of labor in our framework, θ, rises. The business chooses to work with the WC, $H = 1$, when the value reaped from hiring is greater than the alternative, $\pi > \theta$. Hence we expect that demand for WCs is lower when the business faces favorable labor market conditions, and their job is easy to fill.

Heterogeneity by Job Characteristics. Businesses with jobs that involve high-valued inventory likely face a higher probability of a bad event occurring (i.e., higher b) or a higher cost from such an event (i.e., higher k). Thus, jobs involving high-valued inventory should have relatively lower WC demand if downside risk is a relevant factor for businesses. Similarly, jobs with frequent customer interactions may also imply more opportunities for costly infractions to occur, although these infractions may be one of several additional considerations when in-person customer interactions are part of the job. For example, businesses may also be concerned that the general presentation of a worker with a past criminal record differs from other workers, and could lower the productivity of the interaction.

Crime and Safety Insurance. Crime and safety insurance (i.e., greater I) would increase WC hiring as long as downside risk is a relevant factor for businesses. The effect of insurance on hiring should be larger for businesses with a greater probability of, or larger costs from, a bad event. This would include businesses with jobs that involve high-valued inventory. If the primary downside risks involve infrequent but very costly events (e.g. violent crimes), then we expect that an insurance policy with a low cap will not impact hiring demand but a very generous insurance policy will. If, on the other hand, the primary downside risks are minor infractions (e.g. petty crime), then we expect the effect of insurance policies with low and high insurance caps per event to similarly increase hiring demand.

Screening Based on Performance History and Objective Performance Information. Requiring that the WC successfully complete a prior job can be viewed primarily, but not exclusively, as increasing the expectation about productivity, $\mathtt{y}$, for that worker. While screening could also decrease the perceived probability of a bad event $\mathtt{b}$, our conversations with the Platform suggest that expected productivity is the primary signal contained in prior ratings. Nevertheless, screening based on performance history should increase the demand for WCs if either productivity or downside risk is a relevant factor for businesses. If businesses have negatively biased beliefs about $\mathtt{y}$ and $\mathtt{b}$ for WCs, providing objective performance information regarding WC performance can also increase demand for WCs.

Screening Based on Criminal Record History. Expected productivity $\mathtt{y}$ may be higher and both the probability of a bad event $\mathtt{b}$ and the cost of that bad event $\mathtt{k}$ may be lower for WCs with less recent criminal histories or convicted of less serious crimes. This combination of lower $\mathtt{b}$ and $\mathtt{k}$ leads to higher demand for WCs with less recent criminal histories or convicted of less series crimes compared to WCs with more recent criminal histories or convicted of more serious crimes if either productivity or downside risk is a relevant factor for businesses.

2.4. Descriptive Statistics and Randomization Assessment

The analysis sample is comprised of the 1,095 hiring managers from 913 businesses that completed the experiment. Businesses in the experimental sample are generally representative of all businesses that use the Platform. Businesses in our experiment have been on the platform for slightly longer than average (1.3 years vs. 1.2), are slightly less likely to be in the transportation & public utilities sector (10% vs. 13%). However, we have similar numbers of firms in all other industries, including service (both 31%), manufacturing (19% vs. 20%) and retail (15% vs. 14%). Table 2 presents descriptive statistics for the experimental sample and a broader set of firms in the United States. Panel A reports information on basic firm characteristics from the Infogroup Historical Business Database (Infogroup, 2016), which contains basic profile data for more than a million U.S. firms. Businesses in our experimental sample are broadly representative of U.S. firms in terms of industrial composition, but skew older (31.1 years vs. 23.6 years) and larger (3,093 employees vs. 21 employees). Businesses in our experimental sample are also somewhat more likely to be in the manufacturing (19% vs. 6%), transportation industries (10% vs. 3%), and public administration (10% vs. 2%), and less likely to be in the service (31% vs. 37%), finance (3% vs. 7%), and construction industries (1% vs. 8%).

Panel B reports information on WC hiring policies from our experiment and a nationwide survey of over 1,000 HR professionals commissioned by the Society for Human Resource Management (SHRM) (Society for Human Resource Management, 2018). Compared to firms in the SHRM survey, businesses in our experimental sample are only somewhat less likely to have a firm-wide WC hiring policy (45% vs. 66%) and to indicate that they want to work with the best candidate for the job regardless of criminal history (46% vs. 53%). Slightly more businesses in our sample indicated that they would want to work with WCs to help give individuals a second chance (50% vs. 38%) or for financial incentives (8% vs. 2%), and a similar number of businesses in both samples are concerned about local or state regulations that make hiring WCs difficult (26% vs. 22%).

Appendix Table A.2 shows that the randomization was balanced in our experimental sample. We regress fifteen business characteristics on indicator variables for all levels of the six randomized treatments. Appendix Table A.2 reports p-values from an F-test of each of the 90 regressions. Only two of the p-values are statistically significant at the 5% level and only five are significant at the 10% level, which is to be expected given the number of tests. These results indicate that randomization was performed correctly and that our sample is balanced across treatment arms.

3. The Labor Demand for Workers with a Criminal Record

In this section, we measure the baseline demand for WCs using the randomized wage subsidies. We first analyze the effects of the wage subsidies on the willingness to work with WCs for all jobs, before turning to its effects for different types of jobs and local labor market conditions. We present our results graphically, providing corresponding regression tables in the Appendix. Our empirical analysis closely follows our pre-analysis plan (PAP).

3.1. Baseline Results

Figure 1 plots the fraction of businesses that are willing to work with WCs by the effective wage, equal to 100% minus the randomized wage subsidy. Panel A shows our baseline results, where we code businesses as willing to work with WCs if they responded "Yes" to the relevant question and unwilling to work with WCs if they responded "No" or "Only if it's hard to fill my jobs." We pre-registered our main analyses using this form of the dependent variable since the answer of "Yes" is unambiguous and allows for choices the ability to be legally binding. Panel B, discussed in more detail below, shows results where we code businesses as willing to work with WCs if they responded "Yes" or "Only if it's hard to fill my jobs" to the relevant question and unwilling to work with WCs if they responded "No." Appendix Table A.3 presents the results from Figure 1 in regression form with standard errors clustered by business, as the treatments are randomized by business.

Panel A of Figure 1 also includes an estimate of the demand elasticity, ϵ^D. To estimate ϵ^D, we first calculate the following the linear specification that includes one observation per respondent:

$$\mathsf{Hire}_i = \gamma_0 + \gamma_1 \cdot \mathsf{EffectiveWage}_i + u_i \tag{1}$$

where Hire_i is an indicator that represents whether business i's is willing to work with WCs and $\mathsf{EffectiveWage}_i$ is equal to 100% minus the assigned wage subsidy in the set {0%; 10%; 25%; 50%; 100%}, as a pseudo-continuous variable, and u_i is an error term. We then calculate $\epsilon^D = \hat{\gamma}_1 \cdot \frac{\bar{w}}{\bar{h}}$, where $\bar{w}$ and $\bar{h}$ are the average effective wage and hiring rates, respectively. Appendix Table A.4 explores robustness to alternate methods of calculating the demand elasticity.

We find that 39% businesses are willing to work with WCs in our baseline case when there is no wage subsidy and the effective wage is 100%. The share of businesses willing to work with WCs is generally increasing in the subsidy level, with 56% of businesses willing to work with WCs when there is a full wage subsidy and no out-of-pocket costs for the business.[9] Estimates of (1) that use information from all of the randomized subsidy levels show that the share of businesses willing to work with WCs increases by approximately 2.1% for every 10% increase in the offered wage subsidy, broadly consistent with elasticities discussed as reasonable for low-wage workers in, for example, Katz (1996).[10] The baseline number of businesses willing to work with WCs is also qualitatively similar to other reported estimates. For example, in a recent review article, Holzer (2007) reports that previous employer surveys have found that approximately 40% of employers would "definitely" or "probably" hire WCs.

The results from Figure 1 highlight both the potential of increasing the demand for WCs using wage subsidies, and its limitation. Raising the subsidy from 0 to 100 percent increases the share of businesses willing to hire WCs by approximately 17%. Before turning to a more formal heterogeneity analysis motivated by our theoretical framework, Appendix Table A.5 presents descriptive statistics for the businesses that are and are not willing to hire WCs at different subsidy levels to better understand these results. Businesses that are and are not willing to hire WCs are similar in terms of hiring manager experience and firm size, but those willing to hire WCs are less likely to have a business-wide policy in place regarding WCs and they utilize the platform less frequently. Businesses that are willing to hire WCs directly express that they want to hire the best candidate regardless of criminal history, and want to give people a second chance when compared to businesses that are not willing to hire WCs. Firms that are willing to hire WCs are also more confident that WCs will perform well and less concerned that WCs will put others at risk or steal or cause damage while on the job compared to businesses that are not willing to hire WCs. These patterns generally hold regardless of the subsidy offer, suggesting that the wage subsidies do not substantially change the mix of businesses willing to hire WCs. The remainder of this section will explore how these baseline results change when the Platform is having a hard time filling jobs and provide a more formal heterogeneity analysis motivated by our theoretical framework.

[9] The estimates shown in Panel A of Figure 1 are non-monotonic over some ranges, with a slightly lower fraction of businesses willing to work with WCs at an effective wage of 75% compared to an effective wage of 90%. The hiring rates at are statistically indistinguishable at these effective wage levels, however, suggesting that the simplest interpretation for these results is that the WC labor demand curve is consistently downward sloping but that our non-parametric estimates include significant sampling error due to the relatively small number of businesses randomized to each effective wage level.

[10] Our estimate is slightly smaller in magnitude than Katz (1996)'s "reasonable guess" of -0.5 for U.S. low-wage workers, though he notes that there is considerable uncertainty around this estimate and that both higher or lower numbers could be reasonable. Our estimated elasticity of ϵ^D= -0.21 is also broadly in line with elasticities estimated for general populations, though somewhat lower in magnitude. For example, our estimated elasticity is similar to the own factor price elasticities of -0.3 to -0.4 that Borjas (2003) finds for male U.S.-born workers across years of experience and education, but smaller than the labor elasticity of -1.2 to -1.5 that Acemoglu et al. (2004) find when examining the effect of the post-WWII increase in female labor supply on wages in the United States.

3.2. Heterogeneity in Demand by Labor Market Conditions

Panel B of Figure 1 plots the fraction of businesses that are willing to work with WCs by the effective wage, where we now code businesses as willing to work with WCs if they responded "Yes" or "Only if it's hard to fill my jobs" to the relevant question and unwilling to work with WCs if they responded "No." These results thus present evidence on the willingness to work with WCs in a tight labor market using a targeted, context-specific measure rather than the rough proxies typically used in past work, e.g. local unemployment rates.

We also report the average regression-weighted difference between the baseline demand curve presented in Panel A of Figure 1 and the new demand curve in Panel B. We estimate the average difference between the two demand curves using the following regression specification that includes two observations per respondent:

$$\mathsf{Hire}_i = \Delta \cdot \mathsf{HardtoFill}_i + \sum_{k \in K} \lambda_k \cdot \mathsf{Subsidy}_{ik} + e_i \tag{2}$$

where the first observation codes willingness to work with WCs using our original definition and the second observation codes willingness to work with WCs using this alternate definition reflecting a tight labor market. Hire_i is an indicator that represents whether business i's is willing to work with WCs, $\mathsf{HardtoFill}_i$ is an indicator equal to one if $\mathsf{Hire}_i = 1$ when businesses responded "Yes" or "Only if it's hard to fill my job," $\mathsf{Subsidy}_{ik}$ is a set of indicator variables for the assigned wage subsidy in the set $K = \{0\%; 10\%; 25\%; 50\%; 100\%\}$, and ϵ_i is an error term. We include all possible wage subsidy levels and omit the constant term. Appendix Table A.3 again presents the results in regression form with standard errors, along with our estimated elasticity for this outcome.

We find that the share of businesses willing to work with WCs increases by 29 percentage points, to 68%, if the Platform is having a hard time filling a job in our baseline case when there is no wage subsidy and the effective wage is 100%. The increase in the fraction of businesses willing to work with WCs when jobs are hard to fill is again roughly constant at different effective wage levels, with an average increase of 25 percentage points according to our estimates from (2).

These results indicate that businesses are more likely to consider non-traditional workers when jobs are hard to fill, consistent with prior work on the effect of local labor market conditions on recidivism. For example, Yang (2017) finds that individuals released from prison when local economic conditions are good are less likely to re-offend, possibly because there are more higher-wage low-skill jobs available and employers are more willing to hire WCs.

By comparison, we find no economically significant differences in the willingness to work with WCs by actual local unemployment rates, the randomized unemployment rate, or the intensity of the COVID-19 pandemic during our sample frame, as shown in Appendix Figure A.1. In unreported results, we similarly find no effect if we focus on the local unemployment rate for workers with a high-school degree or less. We interpret these results as suggesting that measures such as local unemployment rates do not capture all of the relevant variation in labor market tightness for the businesses in our sample, and that more targeted measures are required to accurately understand the importance of labor market conditions on the demand for WCs. The results also suggest that the significant demand we document for WCs is unlikely to evaporate due to the COVID-19 pandemic, although we caution that all of our results come from relatively early in the pandemic.

3.3. Heterogeneity by Job Characteristics

Figure 2 explores heterogeneity in the demand for WCs by whether the job involves high-value inventory and customer interactions, two highly-salient characteristics that map to our motivating framework. Panel A splits businesses into two groups based on whether businesses report that their jobs involve customer interaction. Panel B instead splits businesses into two groups based on whether businesses report that their jobs involve access to cash or high-value inventory. Each panel plots the fraction of businesses that are willing to work with WCs by effective wage in each group and the average difference between the demand curves, estimated using a version of the regression specification (2) described above. Following our baseline results, we code businesses as willing to work with WCs if they responded "Yes" to the relevant question and unwilling to work with WCs if they responded "No" or "Only if it's hard to fill my jobs." Appendix Table A.3 again presents the results in regression form with standard errors.

In our baseline case when there is no wage subsidy and the effective wage is 100%, we find that the share of businesses willing to work with WCs increases by 6 percentage points to 45% when jobs do not involve customer interactions. The average increase in the fraction of businesses willing to work with WCs when jobs involve customer interactions across all subsidy levels is 13 percentage points. We similarly find that the share of businesses willing to work with WCs increases by 12 percentage points to 51% for jobs that do not involve high-value inventory in our baseline case when there is no wage subsidy, with an average increase of 18 percentage points across all subsidy levels. The results in Figure 2 are consistent with businesses perceiving greater risks related to customer safety or inventory theft when hiring WCs, as suggested by both our motivating framework. These results are also consistent with prior work suggesting that employers with jobs that require "trust" are generally less willing to hire WCs (Holzer, 2007).

4. Crime and Safety Insurance and Targeted Screening

This section tests several approaches to increasing WC employment that directly address the underlying reasons that businesses may choose to screen out WCs, most notably lower average productivity and higher downside risk. We begin by measuring the effects of crime and safety insurance that is meant to address downside risk concerns. We then measure the effects of performance screening and screening based on criminal record history, policies that are meant to address both average productivity concerns and downside risk.

4.1. Crime and Safety Insurance

Figure 3 plots the fraction of businesses that are willing to work with WCs at each effective wage and randomly assigned level of crime and safety insurance. We also plot the baseline level of demand from Panel A of Figure 1 and report the average difference between the baseline curve and each new demand curves that include the crime and safety insurance, estimated using a version of the regression specification (2) described above. Appendix Table A.6 provides the results from Figure 3 in regression form with standard errors clustered by business, along with estimates of demand elasticities.

We find that providing crime and safety insurance significantly increases the level of demand for WCs, consistent with concerns about downside risk when hiring WCs. In our baseline case with no wage subsidy, insurance that covers damages up to $5,000 increases demand for WCs by 12 percentage points to 51%. The increase in the fraction of businesses willing to work with WCs with insurance that covers damages up to $5,000 is roughly constant at different effective wage levels, with an average increase of 12 percentage points (31%). This 12 percentage point increase is equivalent to the effects of a 80% wage subsidy, based on a linear extrapolation of our baseline estimates from Panel A of Figure 1. We find somewhat larger effects of insurance coverage at higher amounts, with an insurance policy with a cap of $100,000 increasing the share of businesses willing to work with WCs by 17 percentage points (45%) both with no subsidy and averaged over all the subsidy levels. Providing $5m in insurance yields similar effects, with an 18 percentage point increase with no subsidy and a 17 percentage point increase averaged over all the subsidy levels.

Taken together, the results suggest that businesses are particularly concerned about moderate damages from WC hiring (e.g., due to moderate theft), as well as some concerns about more severe tail risk events (e.g., due to violence). Our results for the $5,000 cap are particularly striking, as the $5,000 cap is equal to that of the rarely-used U.S. Federal Bonding program. This estimate thus raises the possibility that the Bonding program's low usage reflects non-demand-based reasons (e.g., that businesses do not know about it or its use it stigmatized). This interpretation of the results is also broadly consistent with Leasure and Andersen (2016), who use an audit study to evaluate Ohio's "Certificate of Qualification of Employment" that lifts occupational licensing restrictions, limits employer liability for negligent hiring claims, and advertises the existence of the Federal Bonding program for WCs. They find that this certificate virtually eliminates the gap in call-back rates between individuals with and without a one-year-old felony drug conviction.

4.2. Screening Based on Performance History

Figure 4 plots the baseline mean willingness to work with WCs at each subsidy level, as well as lines representing mean willingness to work with WCs at each subsidy level given that the WC has satisfactorily completed one, five, or twenty-five previous jobs on the Platform. We report Δ, the mean effect of having each number of completed jobs on willingness to work with WCs compared to baseline level of demand. Appendix Table A.6 reports the regression version of these results, along with estimates of demand elasticities.

Screening by performance history substantially increases the demand for WCs, consistent with concerns about both productivity and downside risk since such screening could reveal instances of both poor productivity and risky behaviour on previous jobs. In our baseline case with no subsidy, businesses are 10 percentage points (27%) more willing to work with WCs if they know that they have successfully completed at least one prior job, increasing total WC demand to 49%. The increase in the fraction of businesses willing to work with WCs who have completed at least one prior job is roughly consistent across effective wage levels, with an average increase of 13 percentage points (33%). This 13 percentage point increase is roughly equivalent to the effect of $5,000 crime and safety insurance or an 80% wage subsidy. Business's demand for WCs increases by a modest amount if WCs are required to have completed more than one prior job satisfactorily. Requiring WCs to have completed five or twenty-five prior jobs increases demand by 19 and 13 percentage points respectively (49% and 33%), relative to the baseline of no performance screening.

These results suggest that businesses see WCs as heterogeneous in their suitability for work, and that they believe that even a single positive or negative review may serve as a reliable screening tool to predict performance and downside risk. These effects are consistent with similar work in other contexts. For example, Pallais (2014) finds that randomly providing a single job's worth of experience along with a positive review has large positive benefits for future employment and wages for inexperienced workers on the online platform oDesk.

4.3. Screening Based on Criminal Record History

Figure 5 plots the baseline mean willingness to work with WCs at each subsidy level, as well as lines representing mean willingness to work with WCs at each subsidy level given that it has been one, three, or seven years since the most recent arrest or conviction. We report Δ, the mean effect of each look-back period compared to baseline level of demand. Appendix Table A.6 reports the regression version of these results, along with estimates of demand elasticities.

We find that offering businesses the opportunity to screen on the most recent arrests or convictions can substantially increase the demand for WCs, again consistent with concerns about both worker productivity and downside risk. In our baseline case with no subsidy, screening WCs so that they are only permitted to accept jobs if it has been at least one year since their most recent arrest or conviction increases demand by 22 percentage points (56%), increasing total WC demand to 61%. The increase in the fraction of businesses willing to work with WCs is roughly constant at different effective wage levels, with the mean effect across all effective wages compared to baseline equalling 17 percentage points (44% percent). These results are greater than the crime and safety estimates, the performance screening estimates, and the effects of a 100% wage subsidy. Effects are even larger if it has been at least five or seven years since the most recent arrest or conviction, with screening out WCs whose arrest or conviction is less than five or seven years old increasing demand by 25 and 28 percentage points respectively (64% and 72%) relative to the baseline of no criminal record history screening.

These results suggest that businesses prefer WC candidates with older criminal records. This could reflect concern that WCs with recent arrests or convictions may have higher risk of recidivism or downside events (e.g., given that the hazard of recidivism is downward-sloping), but could also indicate concern that recently arrested or convicted WCs are less productive and have higher rates of absenteeism.

Figure 6 similarly plots mean willingness to work with WCs at each subsidy level given a specific conviction type. The estimates of Δ presented in this figure represent the mean impact of each crime type on willingness to work with WCs relative to the baseline crime type of violent felony, as we did not attempt to ask about every conviction type. We also did not ask about arrests without convictions, meaning that we do not expect the results in Figure 6 to aggregate to the baseline results. Panel A plots demand for WCs with felony convictions, while Panel B considers misdemeanor convictions. Appendix Table A.6 reports the regression version of these results, along with estimates of demand elasticities.

Businesses are consistently most willing to work with WCs whose convictions arise from issues related to controlled substances and least willing to work with WCs whose convictions were related to violent conduct. In Panel A, with no subsidy, businesses are 21 percentage points (350%) more willing to work with a WC with a substance-related felony than with a violent felony. Across subsidy levels, the mean effect of having a substance- or property-related felony instead of a violent felony is a 24 percentage point (400%) or 6.6 percentage point (116%) increase, respectively. In Panel B, the same pattern holds, though the overall demand is higher. With no subsidy, businesses are 41 percentage points (410%) more willing to work with WCs with a substance-related misdemeanor as compared to a violent misdemeanor. Across subsidy levels, the mean effect of having a substance- or property-related misdemeanor instead of a violent felony is a 46 percentage point (767%) or 25 percentage point (417%) increase, respectively.

The results in Figures 5 and 6 show that selective screening based on criminal record history can have large effects on WC hiring. These results are broadly aligned with other research findings in this area. For example, Holzer et al. (2007) find that employers report being more willing to hire workers with drug and property convictions compared to other types of convictions, while audit studies show that there are relatively small effects of having a misdemeanor arrest (Uggen et al., 2014) but large effects of any type of both felony drug and property convictions on call-back rates (Agan and Starr, 2017).

4.4. Heterogeneity by Job Characteristics

Appendix Figures A.2 and A.3 explore heterogeneity by whether the job involves high-value inventory and customer interactions, following our baseline heterogeneity results discussed above. Each panel plots the fraction of businesses that are willing to work with WCs by effective wage in each group. Each panel also presents the average difference between the demand curves, estimated using a version of the regression specification (2) described above. Panel A of Appendix Figure A.2 shows that the impact of insurance on hiring is larger for high-value inventory businesses, consistent with the idea that businesses are sensitive to the downside risk of hiring WCs. For example, $5m in insurance boosts hiring by 10 percentage points in jobs without high-value inventory, but by twice as much (21 percentage points) in jobs with high-value inventory. We are able to strongly reject the null hypothesis that the elasticity of hiring with respect to the insurance level is the same across jobs with and without high-value inventory ($p < 0.001$). In our motivating framework, this prediction follows because insurance is more valuable to businesses with a greater chance or cost from a loss. By comparison, the impact of screening on performance history or years since the most recent arrest or conviction is similar across businesses with and without high-value inventory. Similarly, Appendix Figure A.3 shows that the effect of insurance is larger for businesses with customer interaction but that the effect of performance or criminal history screening is similar across businesses with and without customer interaction.

5. Objective Performance Information and Correcting Misperceptions in Beliefs

In this section, we measure the effect of providing objective performance information about the average productivity of WCs on the labor demand for WCs. This policy, unlike others, requires no individual information. We again hold fixed the subsidy level established in the first part of the experiment.

5.1. Objective Performance Information

We begin by testing whether hiring managers have accurate perceptions about the performance of WCs. In Figure 7, Panels A and B present the distribution of prior beliefs in dashed lines. Prior beliefs about the performance of WCs varies substantially, but on average, hiring managers underestimate the likelihood of a WC earning a 5-star rating by 12% and overestimate the likelihood of a low-performance rating by 14%. These pessimistic beliefs are consistent with businesses' use of WC status as a performance signal, creating the potential for more explicit performance screening or average performance information to replace WC status signals.

Figure 7 plots the distribution of posterior beliefs and the willingness to work with WCs at each subsidy level following our randomized information intervention. Panels A and B of Figure 7 plot the distribution of posterior beliefs about low and high WC performance, where we elicit both sets of beliefs at the end of our incentive-compatible guessing game. The solid line histograms represent posterior beliefs for the respondents who were shown objective information about WC performance. The dashed line histograms represent the prior beliefs of these same participants. The vertical dash-dotted lines demarcate the true average performance of WCs on the Platform. As indicated by the compression of posterior beliefs around the truth, the objective information lead participants to update their beliefs toward the truth. On average they shifted their beliefs downwards about the likelihood of receiving a no-show or low rating by 5.4 percentage points (31%, 0.25 standard deviations) and upwards about the likelihood of receiving a 5-star rating by 6.7 percentage points (9%, 0.35 standard deviations).

Panels C and D plot reduced form results, illustrating how the information treatment affects mean willingness to work with WCs. The horizontal axis plots the effective wage, equal to 100% minus the randomized wage subsidy. The vertical axis plots the fraction of businesses that respond that they are willing to work with WCs at the given effective wage. The bottom, lighter line in both panels represents willingness to work with WCs among businesses in the treatment group prior to the information treatment, while the upper, darker line represents willingness to work with WCs among the same businesses after the information treatment. Both panels also provide estimates of the mean difference between the two curves, Δ.

Panel C shows that providing information about low-performance only increases the share of businesses willing to work with WCs by 3.3 percentage points, and that the increase is not statistically significant. By comparison, Panel D shows that providing information about high-performance increases hiring by 10.5 percentage points, which is approximately equivalent to the effect of a 45% wage subsidy according to a linear extrapolation of our experimental subsidy estimates. We interpret the 10.5 percentage point treatment effect of high-performance information on hiring as economically substantial. The treatment effect is only slightly less than that of providing $5,000 of insurance or of requiring WCs to have successfully completed at least 1 prior job. The muted effect of low-performance ratings, which primarily reflect no-shows, compared to high-performance ratings, suggests that businesses are less concerned with absenteeism when deciding whether to work with WCs and more concerned that WCs might not satisfactorily meet their performance standards. This is consistent with the view that some businesses view WCs as less productive on average than otherwise similar non-WCs.

In Appendix Section B, we measure the effect of correcting misperceptions in beliefs by exploiting the interaction of cross-firm variation in prior beliefs and our randomized information intervention. We find that for an information shock that raises the business's beliefs about performance by 10%, willingness to hire WCs rises by 15%, implying a hiring elasticity of 1.5 with respect to beliefs about performance. In settings where the intervention to shift beliefs differs considerably from ours, this elasticity may be more helpful than our reduced form estimates in approximating the effect on hiring outcomes.

6. Threats to Validity

In this section, we describe how the details of the experimental design and setting may affect the interpretation of our results.

Social Desirability Bias. One important consideration is whether businesses may express interest in hiring WCs out of a desire to appear socially conscious. The incentive-compatible structure of our experiment directly addresses this concern. Our study is based on businesses making real, high-stakes choices. From a participating hiring manager's perspective, the Platform – to whom they had ceded discretion over circulating their posted jobs – was asking direct questions about whether their business would allow WCs to accept their jobs. Throughout the entire study, the Platform made no mention of research because the primary purpose of the study was indeed to inform their business choices. If a hiring manager expressed interest in working with WCs through the experiment, the Platform could legally allow WCs to accept their posted jobs, after which a WC could show up to their work site without any additional screening. And, of course, a WC could appear indistinguishable from other workers at that time, so additional precautions tailored to the individual would also be hard to implement. Indeed, the Platform *did* allow WCs to accept the jobs of those who expressed interest, and these jobs were carried out just as they would be by a non-WC. Moreover, the Platform used the responses to redesign their Platform with regards to expanding WC inclusion. A business that believes it is a poor choice to work with WCs would thus be going against their own economic interests by expressing a social desire to do so.

Screening Expectations. A second consideration is that businesses could have assumed that the pool of WCs the Platform would allow to accept their jobs was pre-screened or a select subset of the full pool of WCs. Fortunately, we asked the direct question about whether the business would allow workers with a criminal record perform their jobs at the start of the experiment, and again at the end when we allowed every participant to revise their initial answer to the same question. In between these two questions, hiring managers were asked to consider WCs who were convicted of specific crimes, ranging from substance-related crimes to violent crimes. As a result, it was likely very clear that the pool of WCs included individuals convicted of more serious crimes, including violent felonies. But, when we compare the answers at the start and end of the experiment (among those that did not receive performance information), the responses are nearly identical. The consistency of the answers at the start and end of the experiment also alleviates concerns about measurement error from participants who may not have been paying full attention.

External Validity. A final and more general consideration is the external validity of our estimates in other contexts. We chose our setting because it is uniquely suited to estimate the demand for WCs across many large traditional businesses seeking workers for entry-level positions and flexible low-skill work. We expected this to offer a large and concentrated pool of appropriate jobs for WCs re-entering the workplace. When extrapolating to other settings, it is important to keep in mind that the work opportunities offered through the Platform are temporary and may thus lower the barrier to working with WCs. While anecdotally many WCs accept long-term positions with business they meet via the Platform, we do not directly observe this transition and cannot speak to the duration of such potential engagements, nor can we speak to the evolution of demand after businesses gain experience working with WCs. Our finding that the level of customer-interactions and presence of high-value inventory affect demand for WCs further suggest that role-specific traits are meaningfully correlated with demand and must be taken into consideration when extrapolating to other settings.

7. Conclusion

This paper uses information from a discrete choice field experiment on a large on-demand staffing platform to test several approaches to increasing WC employment, each of which directly addresses the underlying reasons that employers may choose to screen out WCs. We find that 39% of businesses on the Platform are willing to work with WCs at baseline, increasing to 50% or higher when businesses are offered a modest level of crime and safety insurance, a single performance review, screening of the most recent criminal records, or objective information about the productivity of WCs. We also find higher levels of demand for jobs that do not involve customer interactions or high-value inventory, and when the Platform is having a hard time filling a job.

An important open question is whether these alternative approaches are more cost effective than wage subsidies, which can achieve similar gains at high enough subsidy levels. While a comprehensive cost comparison is beyond the scope of this paper, we can calculate the direct costs of increasing the demand for WCs for each of our main treatments under reasonable assumptions. These calculations reveal that all of these policies can significantly increase demand for WCs at a fraction of the cost of wage subsidies. Performance screening, for example, can achieve notable gains in the share of businesses willing to work with WCs at near-zero cost because a large number of businesses are willing to work with and provide WCs with their first performance review, opening the door to businesses that highly value that first positive review. Providing objective information on the average productivity of WCs can similarly increase the share of businesses hiring WCs at essentially zero additional cost to the Platform. Revising background check matrices to only exclude candidates with the most recent criminal records requires no new costs for the Platform. Finally, we calculate that crime and safety insurance can increase the demand for WCs at one-half to one-tenth the cost of wage subsidies under realistic assumptions of the probability of damages due to WC misbehavior.[11] These calculations suggest that all of the options we consider are substantially more cost-effective than wage subsidies, at least in this context.

Our estimates thus suggest that there is a range of cost-effective options to increase employment among WCs, an important finding in light of recent evidence that simply prohibiting questions about an applicant's arrest and conviction record ("Ban the Box") can encourage employers to make inaccurate judgments about an applicant's arrest and conviction record based on their race (e.g., Bushway, 2004; Holzer et al., 2006; Stoll, 2009; Agan and Starr, 2018; Doleac and Hansen, 2020) without significantly increasing the employment of WCs (e.g., Jackson and Zhao, 2017; Rose, 2021). Based on the findings from our study, the Platform is changing its user interface nation-wide so that thousands of businesses posting new jobs could have the option of accepting WCs regardless of their choices or participation in the experiment. To date, demand from our study participants combined with the staged roll-out has led to approximately 6,700 jobs being available to WCs.

[11]For example, increasing the number of businesses willing to work with WCs by approximately 10 percent would require a 50% wage subsidy. Using a typical Platform wage of $15 per hour, the subsidy approach would thus cost $60 per worker per day. Providing a $5,000 crime and safety insurance policy could also increase WC demand by approximately 10%. Assuming that WCs have either a 1 in 1,000 or 1 in 200 daily chance of incurring $5,000 in damages, this insurance policy would thus have an expected cost of $5 to $25 per worker per day.

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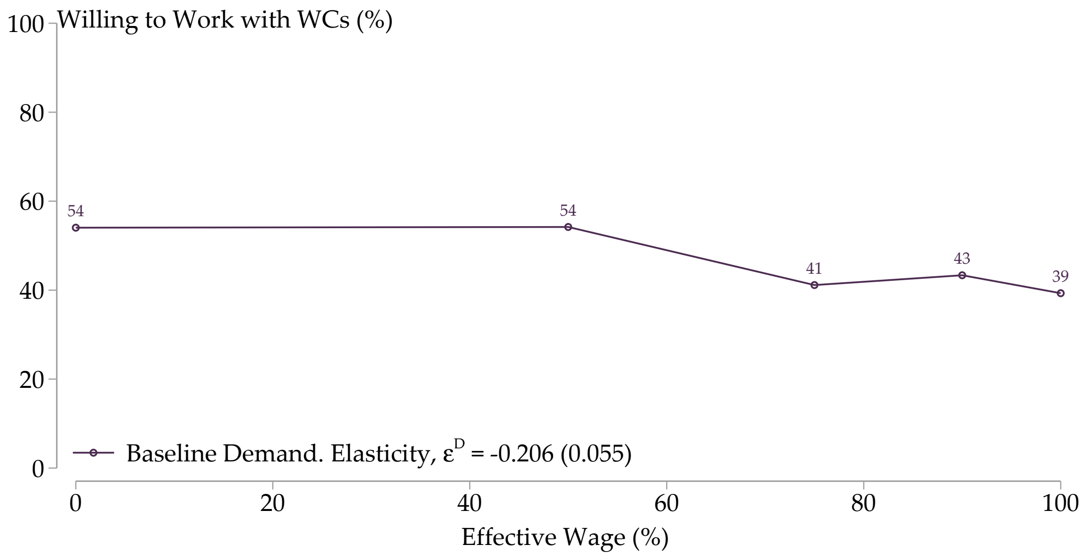
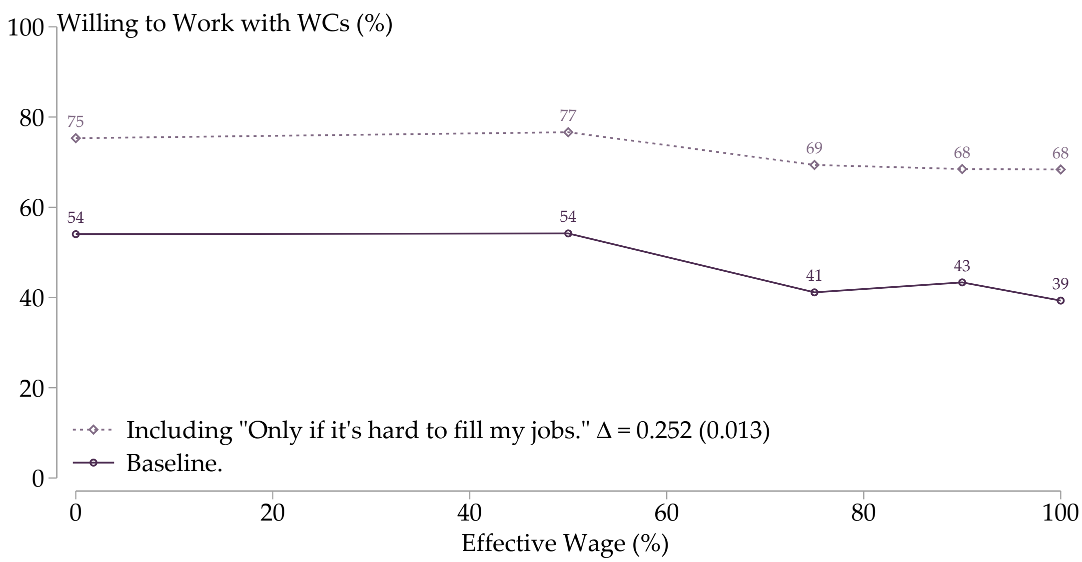
Figure 1: Labor Demand for Workers with a Criminal Record

A. Baseline Definition of “Willing to Work With WCs”



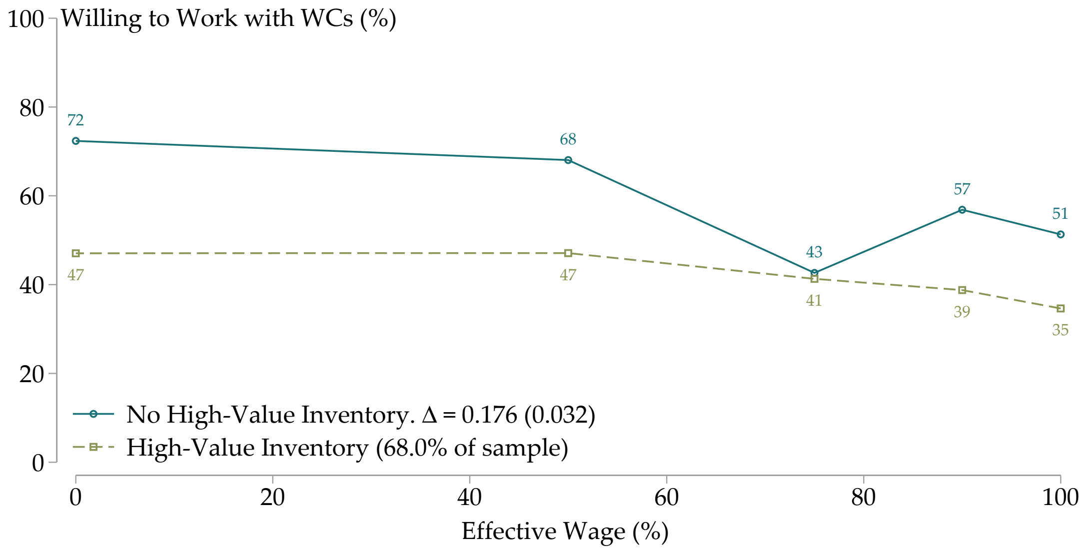
B. Including “Only If It’s Hard to Fill My Jobs”



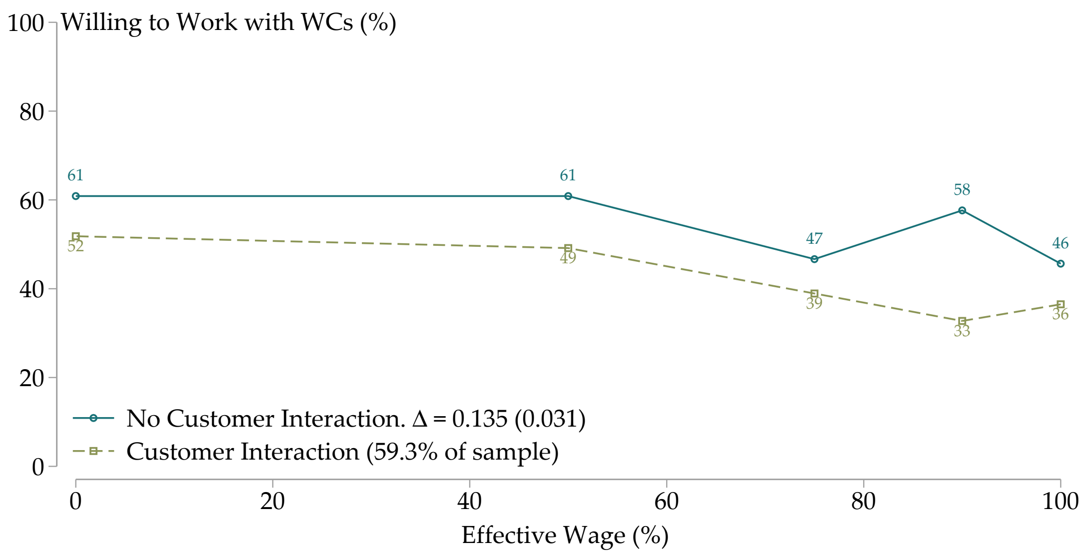
Notes. This figure plots mean willingness to work with WCs against the randomized effective wage. The effective wage is calculated as the share of the wage remaining after the subsidy is applied, or $100 - \mathtt{subsidy\ rate}$. Respondents are asked if they are willing to work with a WC with this wage subsidy and can answer “Yes”, “Only if it’s hard to fill my jobs”, or “No”. Panel A reports results including only those who answer “Yes” as willing to work with WCs. This is our baseline definition of willingness to work with WCs that we use throughout. Panel A also reports the baseline labor demand elasticity estimated using the regression described in text. In Panel B, we plot an additional series in which we consider respondents who answer “Yes” or “Only if it’s hard to fill my jobs” as willing to work with WCs. In Panel B Δ is the mean difference between the baseline series and the series that includes respondents selecting “Only if it’s hard to fill my jobs”. The estimates are based on the experimental sample described in Table 2. The sample includes 1,095 managers from 913 businesses. Precise point estimates are reported in columns 1 (Panel A) and 2 (Panel B) of Appendix Table A.3.

Figure 2: Heterogeneity by Job Characteristics

A. High-Value Inventory

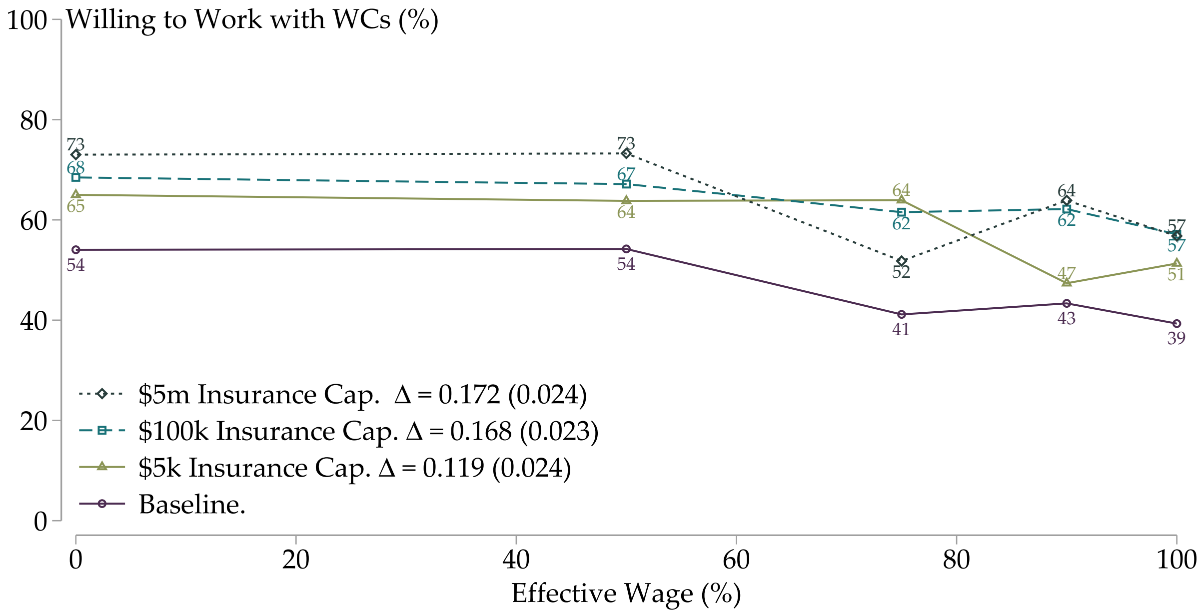


B. Customer Interaction



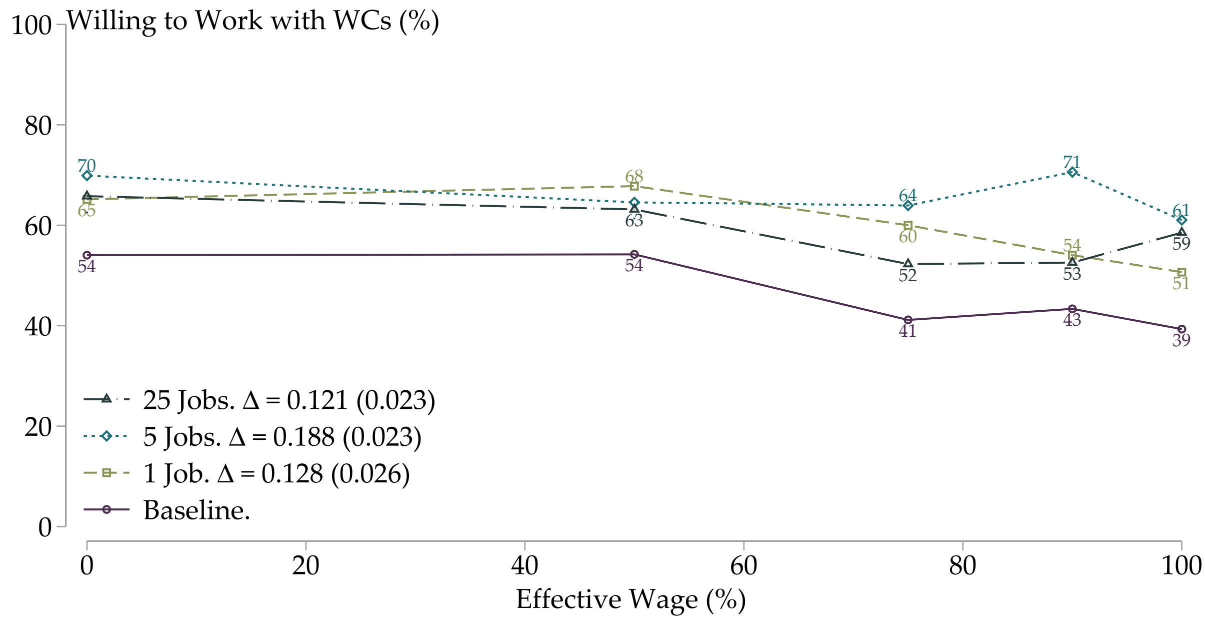
Notes. This figure plots mean willingness to work with WCs against the randomized effective wage. Panel A reports results separately for businesses who report that their jobs do or do not involve high-value inventory. Panel B reports results separately for businesses who report that their jobs do or do not involve customer interaction. In each panel, we report an estimate of the mean difference Δ between the two demand curves. The estimates are based on the experimental sample described in Table 2. The sample includes 1,095 managers from 913 businesses. Precise point estimates are reported in columns 5-6 (Panel A) and 3-4 (Panel B) of Appendix Table A.3.

Figure 3: Crime and Safety Insurance



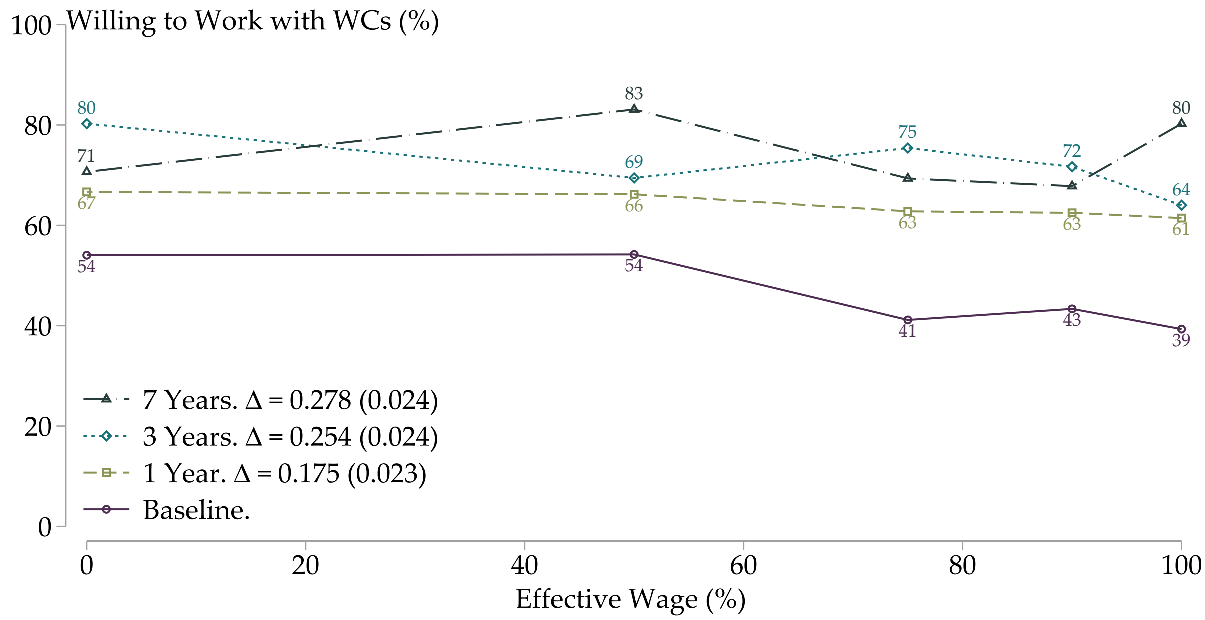
Notes. This figure plots mean willingness to work with WCs against the randomized effective wage. The dotted baseline curve displays baseline hiring rates shown in Panel A of Figure 1. The three upper curves display the effect of the Platform providing a crime and safety insurance policy that covers damages up to $5,000, $100,000, or $5 million. The Δ values estimate the mean effect of each level of insurance across subsidy levels compared to baseline. The estimates are based on the experimental sample described in Table 2. The sample includes 1,095 managers from 913 businesses. Precise point estimates are reported in columns 2-4 of Appendix Table A.6.

Figure 4: Screening Based on Performance History



Notes. This figure plots mean willingness to work with WCs against the randomized effective wage. The dotted baseline curve displays baseline hiring rates shown in Panel A of Figure 1. The three upper curves display the effect of the individual having satisfactorily completed either one, five, or twenty-five previous jobs on the platform. The Δ values estimate the mean effect of screening by each number of completed jobs compared to baseline. The estimates are based on the experimental sample described in Table 2. The sample includes 1,095 managers from 913 businesses. Precise point estimates are reported in columns 2-4 of Appendix Table A.6.

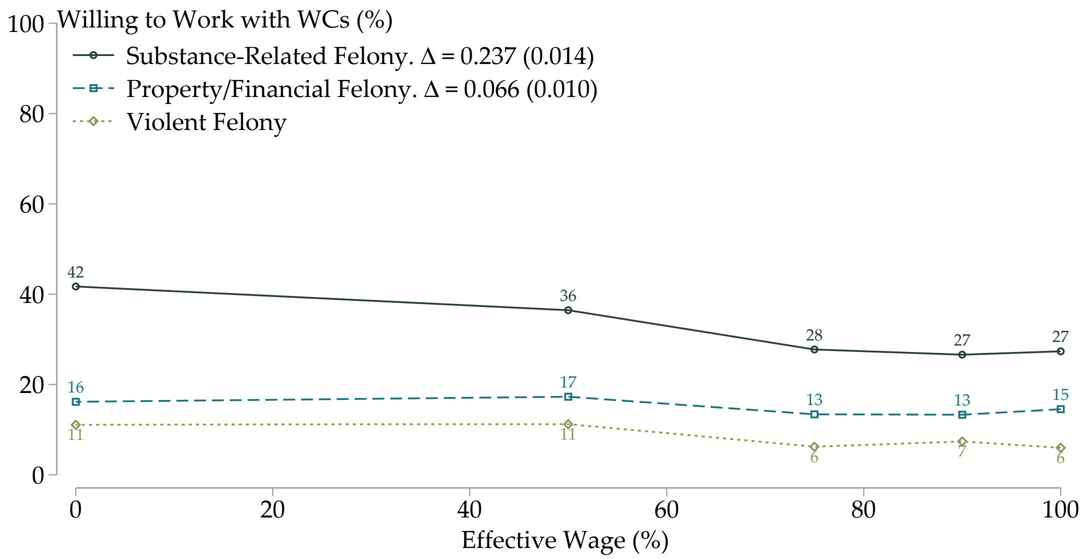
Figure 5: Screening Based on Years Since Most Recent Arrest or Conviction



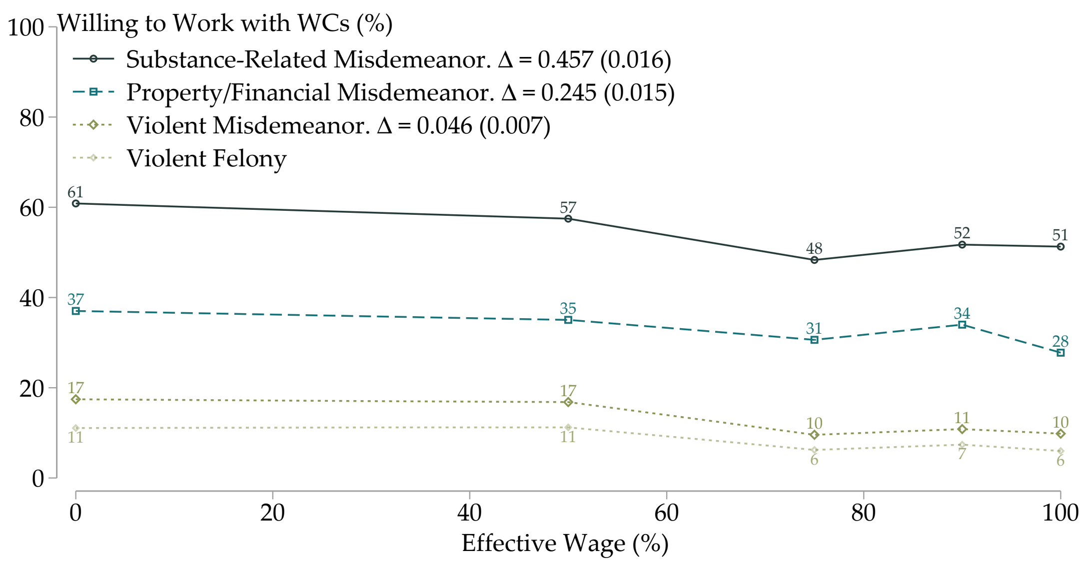
Notes. This figure plots mean willingness to work with WCs against the randomized effective wage. The dotted baseline curve displays baseline hiring rates shown in Panel A of Figure 1. The three upper curves display the effect of it having been one, three, or seven years since the individual was most recently arrested or convicted. The Δ values estimate the the mean effects of screening on the numbers of years since arrest or conviction compared to baseline. The estimates are based on the experimental sample described in Table 2. The sample includes 1,095 managers from 913 businesses. Precise point estimates are reported in columns 2-4 of Appendix Table A.6.

Figure 6: Screening Based on Conviction Type

A. Felony Convictions



B. Misdemeanor Convictions

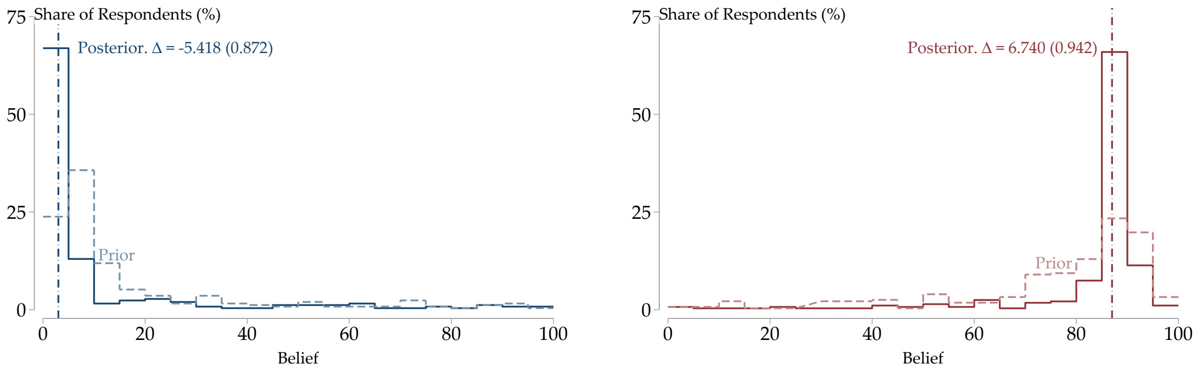


Notes. This figure plots mean willingness to work with WCs against the randomized effective wage, given the specific conviction type and severity. Panel A plots willingness to work with WCs who have a substance-related felony, a property/financial related felony or a violent felony. Panel B plots willingness to work with WCs who have a substance-related misdemeanor, a property/financial related misdemeanor or a violent misdemeanor. The bottom dotted line in Panel B re-plots the estimates for violent felonies. The Δ values estimate the mean difference between demand for each crime type relative to the violent felony crime type. The estimates are based on the experimental sample described in Table 2. The sample includes 1,095 managers from 913 businesses. The estimates are based on the experimental sample described in Table 2. Precise point estimates are reported in columns 5-7 (Panel A) and 8-10 (Panel B) of Appendix Table A.6.

Figure 7: Objective Performance Information

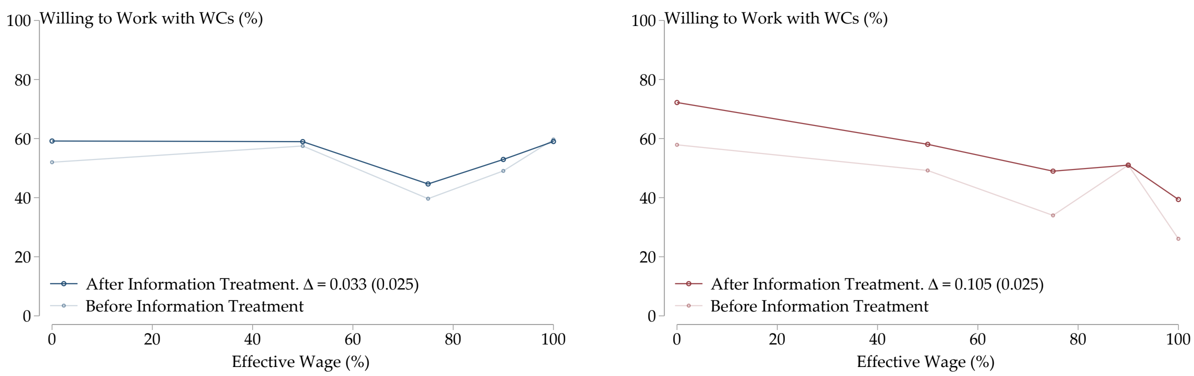
Effects of Receiving Information on Posterior Beliefs

A. Low-Performance B. High-Performance



Effects of Receiving Information on Posterior Willingness to Work With WCs

C. Low-Performance D. High-Performance



Notes. Panels A and B report the posterior belief distributions about WC performance for respondents who were shown objective information about WC performance and for the control group. In these panels, Δ values estimate the mean difference between the posterior and the prior beliefs for the subset that were shown information. Panels C and D report the baseline demand for WCs and the demand after receiving the information treatment only for the respondents who were shown objective information about WC productivity. In these panels, Δ values estimate the mean difference between demand before and after receiving the information treatment. These estimates are based on the subset of 550 managers from 467 businesses who were shown the objective information. Appendix Figure A.4 presents the prior belief distributions.

Table 1: Description of Main Treatments

Treatment Name	Survey Question	Values
Wage Subsidy	If {The Platform} gave you a [wage subsidy] discount for {Platform Workers} with a criminal record, would you permit such {Platform Workers} to perform jobs you post? This means you would only pay [1-wage subsidy] of the wage for those with a criminal record.	0%; 5%; 10%; 25%; 50%; 100%
Crime and Safety Insurance	If {The Platform} could cover damages up to [insurance level] related to theft or safety incurred by workers with a criminal record, would you permit such {Platform Workers} to perform jobs you post?	$1k; $5k; $100k; $5m
Performance History	If {The Platform} required {Platform Workers} with a criminal record to have satisfactorily completed [performance history] job(s), receiving >85% positive reviews (5 stars), would you permit such {Platform Workers} to perform jobs you post?	1 job; 5 jobs; 25 jobs
Clean Record Length	If {The Platform} required users with a criminal record to have maintained a clean record for at least [clean record length] would you permit such users to perform jobs you post?	1 year; 3 years; 7 years
Conviction Type	Please indicate whether you would permit {Platform Workers} with these types of convictions to perform jobs you post. {The Platform} would still give you a [wage subsidy] discount, but no other supplementary policies would apply.	Felony; Misdemeanor; Violent; Substance-Related; Property/Financial
Performance Information Provision	The truth is that {share}% of jobs completed by people with a criminal record resulted in a {rating} on the same or a similar platform – actually better than everyone else.	share: -3%; 87% rating: no-show or a low rating; 5-star rating

Notes. This table summarizes the main experimental treatments. Text in curly brackets is redacted information identifying the Platform. Text in square brackets is a placeholder for the randomized values of each treatment.

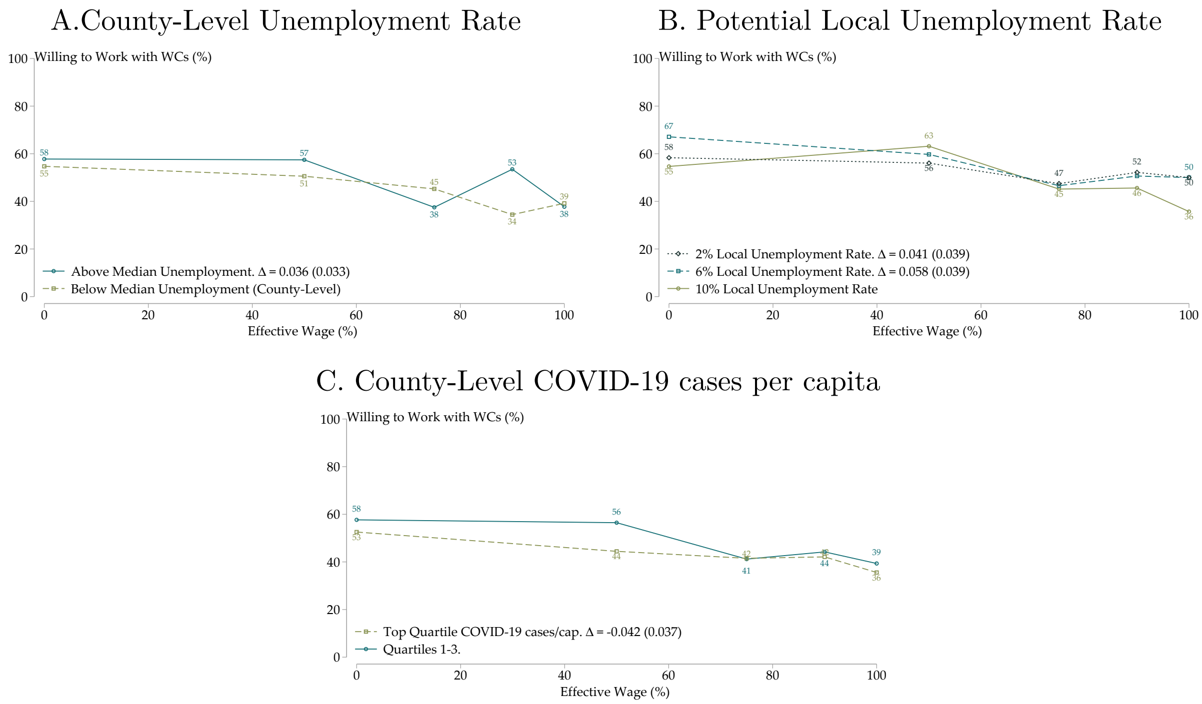
Table 2: Descriptive Statistics

A. Firm Characteristics	Experimental Sample	Infogroup Database
Firm Age	19.0	16.0
Employees	40	02.5
Share Service	0.31	0.37
Share Manufacturing	0.19	0.06
Share Retail	0.15	0.21
Share Transportation & Public Utilities	0.10	0.03
Share Public Administration	0.10	0.02
Share Wholesale Trade	0.09	0.08
Share Finance, Insurance, & Real Estate	0.03	0.07
Share Construction	0.01	0.08
Firms	666	3,260,733
with Industry Information	518	1,245,145
B. Policies Concerning Whether WCs can Work	**Experimental Sample**	**SHRM Survey**
Firm-Wide WC Eligibility Policy	0.45	0.66
WC Positives: Best Candidate	0.46	0.53
WC Positives: Second Chance	0.50	0.38
WC Positives: Financial Incentives	0.08	0.02
WC Negatives: Customer Reaction	0.49	0.30
WC Negatives: Regulation	0.26	0.22
WC Negatives: Performance	0.15	0.04
Firms	900	1,228
C. Manager Characteristics	**Experimental Sample**	
Years of Experience (mean)	7.33	
Share of Mangers in HR	0.14	
Authority to Allow WCs to Work:		
Share with Direct Authority	0.53	
+ Share with Influence on Decision	0.80	
Managers	1,095	

Notes. This table reports descriptive statistics for the experimental sample. Panel A reports statistics for the 571 firms in our sample matched to the Infogroup Historical Business Database (column 1) and all firms in the Infogroup Database (column 2). The industry characteristics are further limited to the 177 firms in our sample with that data available in the Infogroup Database. Panel B reports statistics for all firms in our sample and a nationally representative sample of firms from a nationwide survey of HR professionals commissioned by the Society for Human Resource Management (SHRM).

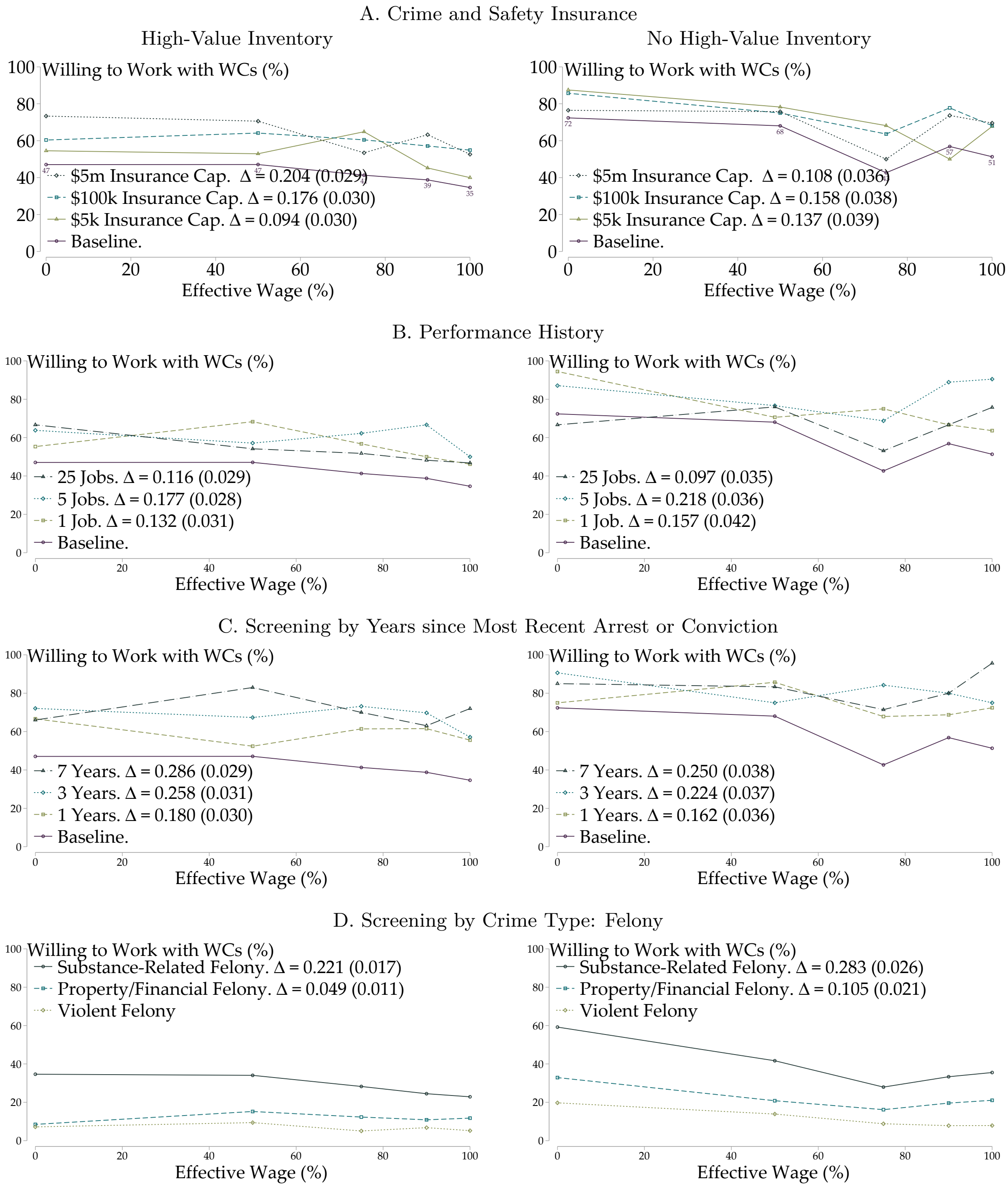
Appendix A. Additional Results

Appendix Figure A.1: Labor Market Conditions and COVID-19 Prevalence



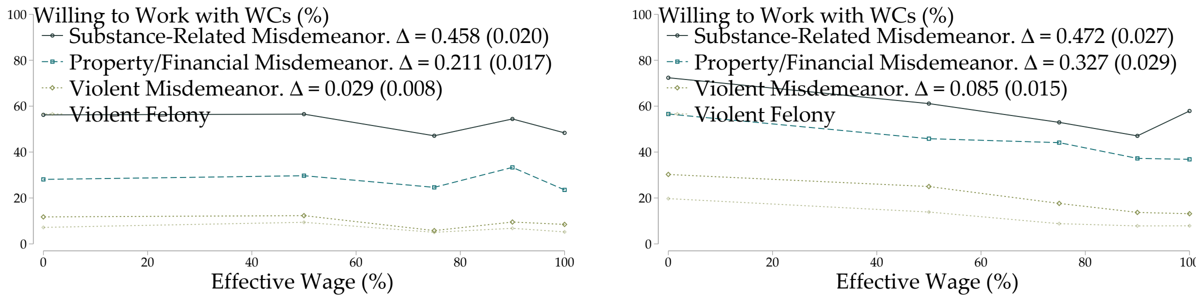
Notes. This figure plots mean willingness to work with WCs against the randomized effective wage. Panel A splits respondents into above-median and below-median groups based on March 2020 unemployment rates in the business's county. The Δ in Panel A represents the average difference between the two curves. Panel B reports willingness to work with under a randomly assigned potential local unemployment rate of 2, 6, or 10%. The Δ in Panel B represents the average impact of the 2 or 6% unemployment rate compared to the 10% unemployment rate. Panel C reports results split based on county level COVID-19 prevalence in March, 2020 when the experiment was distributed. The solid line represents businesses whose county was in the top quartile of COVID-19 rates, and the dotted line represents businesses whose county was in the bottom three quartiles of COVID rates. The estimates in Panels A and B are based on the experimental sample described in Table 2. This sample includes 1,095 managers from 913 businesses. The estimates in Panel C exclude 62 observations from New York City counties due to data constraints.

Appendix Figure A.2: Heterogeneity in Treatment Effects, High-Value Inventory



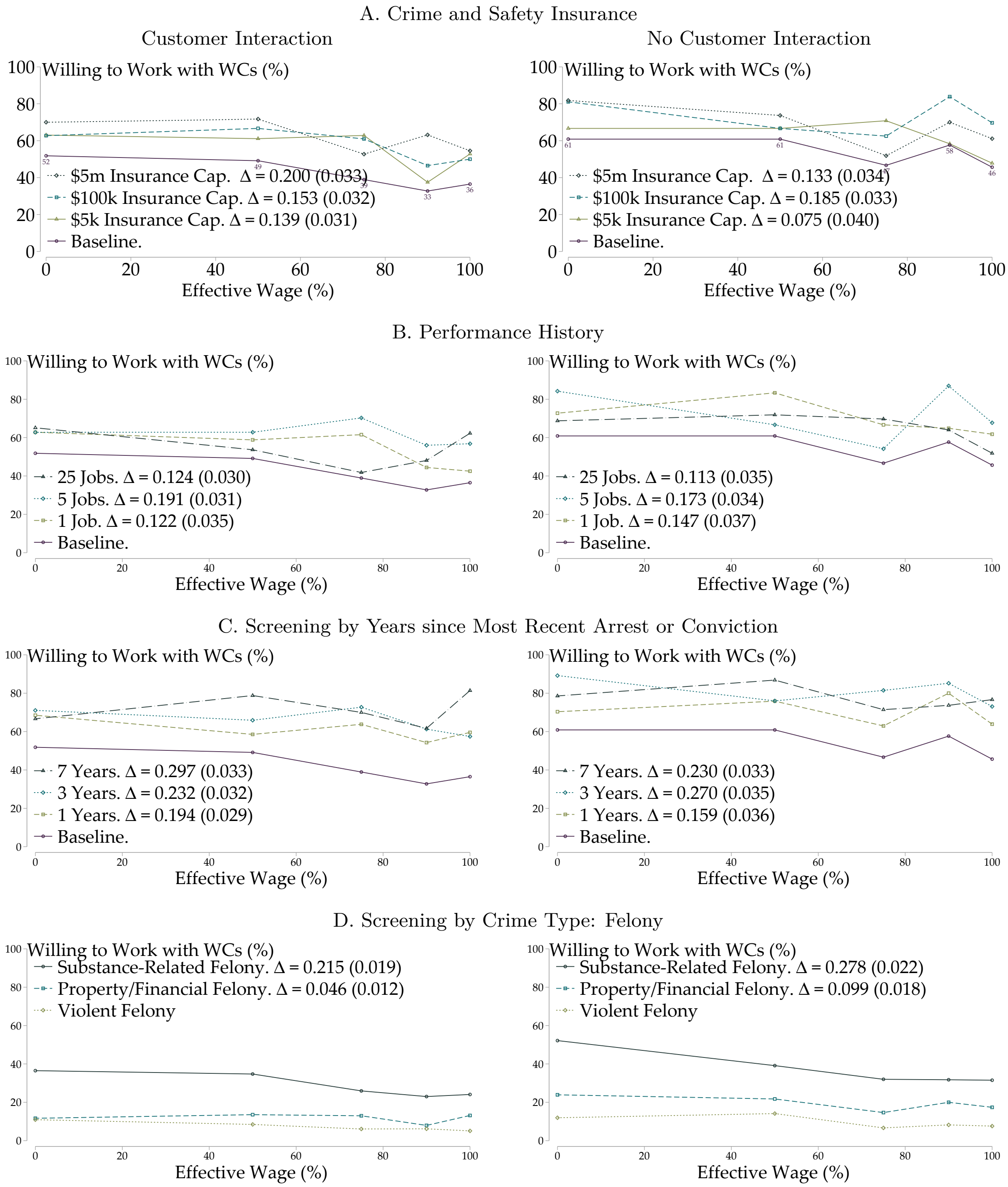
Appendix Figure A.2: Heterogeneity in Treatment Effects, High-Value Inventory

E. Screening by Crime Type: Misdemeanor



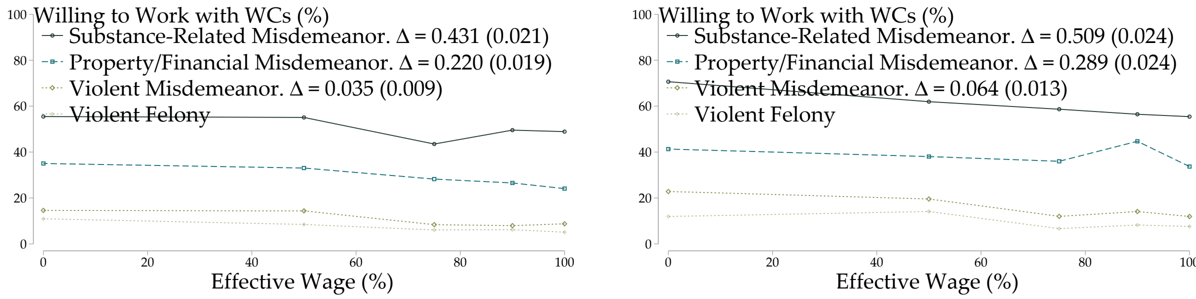
Notes. This figure plots the differential effects of several policies on mean willingness to WCs against the randomized effective wage, split by whether businesses report that their jobs involve customer interaction. The left-hand graph in each panel presents mean willingness to work with for the 59.3% of our sample that reports that their jobs involve customer interaction. The right-hand graph in each panel presents mean willingness to work with for the 40.7% of our sample whose jobs do not involve customer interaction. In Panels A, B, and E, Δ represents the mean effect of each level of each policy (e.g., $5k insurance or 1 completed job) across all subsidy levels as compared to the baseline. In Panels C and D, Δ represents the mean difference between demand for each crime type relative to the violent felony crime type.

Appendix Figure A.3: Heterogeneity in Treatment Effects, Customer Interaction



Appendix Figure A.3: Heterogeneity in Treatment Effects, Customer Interaction

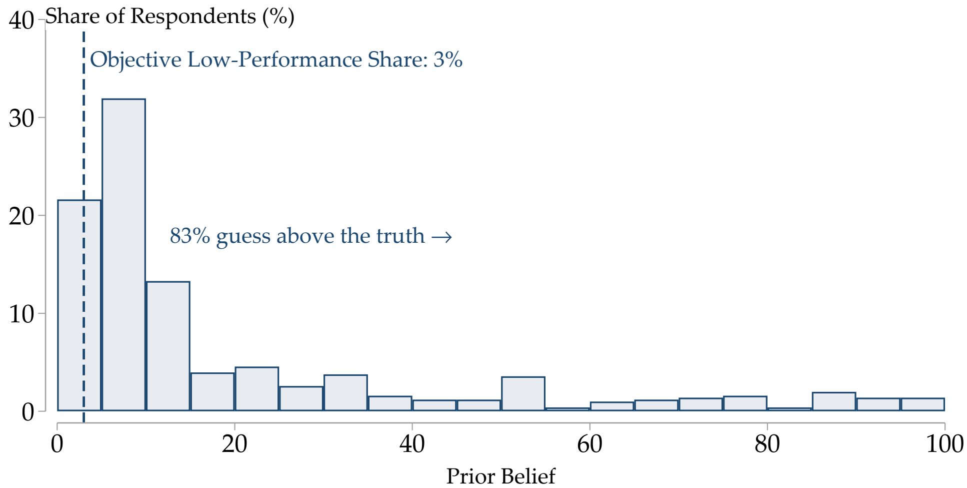
E. Screening by Crime Type: Misdemeanor



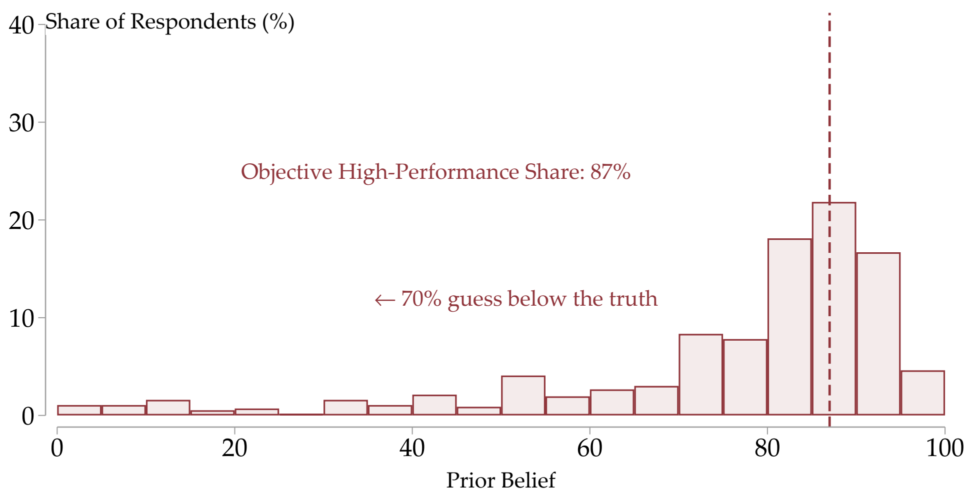
Notes. This figure plots the differential effects of several policies on mean willingness to work with WCs against the randomized effective wage, split by whether businesses report that their jobs involve high-value inventory. The left-hand graph in each panel presents mean willingness to work with WCs for the 68.1% of our sample that reports that their jobs involve high-value inventory. The right-hand graph in each panel presents mean willingness to work with WCs for the 31.9% of our sample whose jobs do not involve high-value inventory. In Panels A, B, and E, Δ represents the mean effect of each level of each policy (e.g. $5k insurance or 1 completed job) across all subsidy levels as compared to the baseline. In Panels C and D, Δ represents the mean difference between demand for each crime type relative to the violent felony crime type.

Appendix Figure A.4: Prior Beliefs about WC Productivity

A. Low-Performance



B. High-Performance



Notes. This figure reports the prior distribution of business beliefs about WC productivity for all respondents. Panel A reports the distribution of prior beliefs about the share of WCs who receive low performance ratings (no-shows and either 1- or 2-star ratings). The dotted vertical line indicates the the true, objective share of WCs who receive low performance ratings. Panel B reports the distribution of prior beliefs about the share of WCs who receive high-performance ratings (5-star ratings). The dotted vertical line indicates the the true, objective share of WCs who receive high-performance ratings. The estimates are based on the experimental sample described in Table 2. This sample includes 1,095 managers from 913 businesses.

Appendix Table A.1: Description of Additional Characteristics

Job Characteristic	Survey Question
Customer Interactions	Do your jobs involve {Platform} users having contact with customers?
High-Value Inventory	At your jobs, is there cash or high-value inventory that {Platform} users could steal?
Hiring Policies	Does your company or organization currently have a hiring policy regarding individuals with a criminal record?
Potential Unemployment Rate	If the unemployment rate were [unemployment rate], meaning the local labor market was [**a** $\in$ {"doing very well", "doing about average", "not doing so well" }] and [**b** $\in$ {"a less than typical", "an average", "a more than typical"}] share of people were looking for jobs, would you permit {Platform Workers} with a criminal record to perform jobs you post?
Performance Information: Prior Beliefs	In 2019, [**a** $\in$ {5%, 85%}] of jobs on the Platform resulted in a [**b** $\in$ {"no-show or low rating (1 or 2 stars)", "5-star rating"}]. What percentage of jobs completed by people with a criminal record do you think would result in a **b** on the Platform or a similar platform? If your guess is within 5% of the truth, we will send you an additional [bonus] reward!

Notes. This table summarizes the main job and firm characteristics used in our analysis, as well as the measure of prior information on WC performance. Text in curly brackets is redacted information identifying the Platform.

Appendix Table A.2: Randomization Assessment
p-values from Regressions of Covariates on Treatment Indicators

A. Firm Characteristics	Wage Subsidy	Crime Insurance	Performance History	Clean Record	Unemp. Rate	Shown Info.
Firm Age	0.157	0.729	0.407	0.233	0.271	0.280
Employees	0.252	0.866	0.101	0.009	0.619	0.424
Share Retail	0.856	0.795	0.320	0.384	0.849	0.317
Share Service	0.059	0.330	0.313	0.635	0.347	0.638
Share Wholesale Trade	0.863	0.730	0.957	0.414	0.400	0.169
Share Nonclassifiable	0.500	0.927	0.366	0.069	0.609	0.400
Share Transportation & Public Utilities	0.279	0.780	0.425	0.561	0.893	0.229
Firm-Wide WC Hiring Policy	0.766	0.669	0.750	0.507	0.435	0.253
Platform Tenure (Years)	0.098	0.067	0.615	0.538	0.212	0.938
Job Vacancy Rate	0.713	0.818	0.300	0.458	0.838	0.926
B. Manager Characteristics						
Manager Works in Human Resources	0.955	0.963	0.826	0.872	0.384	0.935
Manager Years of Experience	0.644	0.361	0.456	0.763	0.809	0.290
C. Platform Characteristics						
Job Involves Customer Interactions	0.745	0.524	0.455	0.360	0.596	0.710
Job Involves High-Value Inventory	0.287	0.449	0.285	0.525	0.428	0.401
D. Modal Job Category						
Fulfillment / Warehousing	0.708	0.800	0.622	0.306	0.221	0.296
General Labor	0.005	0.096	0.728	0.639	0.306	0.148
Event Staff	0.471	0.875	0.623	0.360	0.716	0.781
Delivery	0.814	0.063	0.424	0.910	0.346	0.915
Washing & Cleaning	0.571	0.908	0.782	0.119	0.355	0.919
Firms	913	913	913	913	913	913
Managers	1,095	1,095	1,095	1,095	1,095	1,095

Notes. This table reports balance tests for the estimation sample described in Table 2. Each cell reports the p-value of an F-statistic from a separate regression of the baseline covariates listed in the rows on indicator variables for each value of the treatments listed in the columns. Standard errors are clustered at the firm level. See the Table 1 notes for additional details on the randomized treatments and the Table 2 notes for additional details on the outcomes and sample.

Appendix Table A.3: Labor Demand for Workers with a Criminal Record

	Baseline	Including "Only If It's Hard to Fill My Jobs"	Customer Interactions	No Customer Interactions	High-Value Inventory	No High-Value Inventory
No Subsidy	0.393	0.684	0.365	0.457	0.346	0.513
	(0.032)	(0.032)	(0.042)	(0.051)	(0.039)	(0.055)
10% Subsidy†	0.433	0.685	0.327	0.576	0.388	0.569
	(0.037)	(0.033)	(0.047)	(0.055)	(0.043)	(0.070)
25% Subsidy	0.411	0.694	0.389	0.467	0.413	0.426
	(0.037)	(0.034)	(0.047)	(0.058)	(0.048)	(0.056)
50% Subsidy	0.542	0.766	0.492	0.609	0.471	0.681
	(0.036)	(0.030)	(0.047)	(0.052)	(0.044)	(0.053)
100% Subsidy	0.540	0.753	0.518	0.609	0.471	0.724
	(0.032)	(0.028)	(0.044)	(0.049)	(0.039)	(0.054)
Elasticity	-0.206	-0.071	-0.246	-0.172	-0.157	-0.319
	(0.055)	(0.032)	(0.073)	(0.084)	(0.066)	(0.093)
Mean Effect vs. Baseline	–	0.252	-0.045	0.081	-0.048	0.121
		(0.013)	(0.021)	(0.024)	(0.019)	(0.026)
Mean Effect vs. Omitted Group	–	–	–	0.135	–	0.176
				(0.031)		(0.032)
Firms	913	913	533	392	613	320
Managers	1,095	1,095	636	436	729	343

Notes. This table reports estimates of the effects of wage subsidies on firms' willingness to work with workers with a criminal record. It also reports how these estimates vary across firms with jobs that involve customer interaction or access to high-value inventory. The regressions are estimated on the experimental sample described in Table 2. Column 1 reports the fraction of managers choosing to work with WCs at each subsidy level. Columns 2-3 report this fraction for firms with jobs that do or do not involve interaction with customers. Columns 4-5 report this fraction for firms with jobs that do or do not involve access to high-value inventory. Mean effects are estimated using regressions that include non-interacted controls for the subsidy level. All specifications report standard errors clustered at the firm level. See the Table 1 notes for additional details on the randomized treatments and the Table 2 notes for additional details on the outcomes and sample. † We use different values for low levels of subsidy (5% and 10%). For exposition, we pool the 5 and 10 percent subsidy levels, which results in a uniform number of observations across values displayed under the label *10%*.

Appendix Table A.4: Labor Demand Elasticities

	Linear	Quadratic	Non-Parametric
10% Subsidy†	-0.354	-0.745	-0.927
	(0.094)	(0.341)	
25% Subsidy	-0.301	-0.547	0.287
	(0.080)	(0.221)	
50% Subsidy	-0.202	-0.271	-0.685
	(0.054)	(0.078)	
100% Subsidy	-0.071	-0.031	0.002
	(0.019)	(0.039)	
Average Elasticity	-0.206	-0.272	-0.158
	(0.055)	(0.077)	
Firms	913	913	913
Managers	1,095	1,095	1,095

*Notes.*This table reports alternate estimates of labor demand elasticity. Column 1 reports linear estimates, calculated as $\frac{dH}{dW}\frac{w}{H}$, where $\frac{dH}{dW}$ represents the slope from a linear regression of willingness to work with WCs on effective wage and is constant across rows. w and H represent the midpoint between two effective wage levels of the effective wage and mean willingness to work with WCs, respectively. For example, in the 10% subsidy row, w is the midpoint between a 100% and 90% effective wage. Column 2 reports quadratic estimates, again calculated as $\frac{dH}{dW}\frac{w}{H}$, where $\frac{dH}{dW}$ represents the marginal effect from a regression of willingness to work with WCs on effective wage and effective wage squared. The marginal effect is calculated at the midpoint between two effective wage levels and as such varies across columns. w and H are defined as in column 1. Column 3 reports non-parametric estimates, calculated as the percent change in willingness to work with WCs over the percent change in effective wage between two effective wage levels. The average elasticity in Column 1 reports the elasticity measure shown in Tables XX-XX, calculated as $\frac{dH}{dW}\frac{\bar{w}}{\bar{H}}$, where $\frac{dH}{dW}$ represents the slope from the linear regression of willingness to work with WCs on effective wage, and $\bar{w}$ and $\bar{H}$ represent mean effective wage and willingness to work with WCs across all subsidy levels. The average elasticity in Column 2 $\frac{dH}{dW}$ is instead calculated as the marginal effect at the mean from the quadratic regression. In Column 3, it is the percent change from 0% effective wage to 100% effective wage. See the Table 2 notes for additional details on the sample.

† We use different values for low levels of subsidy (5% and 10%) in two survey arms. For exposition, we pool the 5 and 10 percent subsidy levels which results in a uniform number of observations across values displayed under the label *10%*

Appendix Table A.5: Demand for WCs and Descriptive Statistics

	Wage Subsidy											
	No Subsidy		10% Subsidy		25% Subsidy		50% Subsidy		100% Subsidy			
	Work w/ WCs?		Work w/ WCs?		Work w/ WCs?		Work w/ WCs?		Work w/ WCs?			
	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	p(F-stat)	N
Years Experience of Hiring Manager	7.43	7.28	6.02	7.74	7.16	8.23	7.42	7.37	6.28	8.09	0.028	1,095
	(0.55)	(0.48)	(0.56)	(0.59)	(0.59)	(0.51)	(0.55)	(0.57)	(0.45)	(0.60)		
Firm size (Employees)	1,717	2,011	3,459	2,171	356	2,171	7,333	9,218	689	2,411	0.551	824
	(1,521)	(1,389)	(2,963)	(1,066)	(107)	(1,533)	(4,172)	(3,770)	(341)	(1,095)		
N. Jobs Posted on Platform	245	627	126	1,674	401	325	703	1,176	228	475	<0.001	1,095
	(83)	(134)	(46)	(1,021)	(114)	(90)	(346)	(415)	(99)	(143)		
Industry Share: Service	0.28	0.40	0.28	0.31	0.37	0.41	0.20	0.27	0.37	0.36	0.598	687
	(0.07)	(0.06)	(0.07)	(0.06)	(0.10)	(0.10)	(0.06)	(0.08)	(0.07)	(0.06)		
Industry Share: Manufacturing	0.19	0.06	0.24	0.25	0.13	0.12	0.26	0.21	0.13	0.21	0.252	687
	(0.05)	(0.02)	(0.08)	(0.08)	(0.06)	(0.04)	(0.06)	(0.07)	(0.04)	(0.05)		
Industry Share: Retail	0.20	0.20	0.13	0.12	0.13	0.16	0.15	0.25	0.13	0.10	0.798	687
	(0.06)	(0.05)	(0.05)	(0.04)	(0.06)	(0.05)	(0.06)	(0.07)	(0.05)	(0.04)		
Has Policy Concerning Whether WCs can Work	0.42	0.55	0.37	0.50	0.41	0.54	0.41	0.51	0.36	0.56	<0.001	1,075
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)		
Reasons for Hiring WCs												
Want Best Candidate, Regardless of Criminal History	0.64	0.37	0.61	0.21	0.62	0.22	0.70	0.19	0.62	0.27	<0.001	1,075
	(0.05)	(0.04)	(0.05)	(0.04)	(0.06)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)		
Want to Give People a Second Chance	0.71	0.37	0.67	0.24	0.61	0.25	0.72	0.31	0.68	0.30	<0.001	1,075
	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)		
Incentivized by Tax Rebates & Other Policies	0.11	0.04	0.07	0.05	0.08	0.04	0.10	0.05	0.08	0.06	0.214	1,075
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)		
Reasons for Not Hiring WCs												
Behavior by employees with criminal records	0.65	0.74	0.67	0.68	0.62	0.68	0.70	0.59	0.65	0.67	0.365	1,095
	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)		
Worried about Customer Reaction	0.54	0.57	0.40	0.50	0.46	0.48	0.47	0.45	0.45	0.53	0.576	1,075
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)		
Regulations Making it Difficult or Impossible	0.33	0.22	0.20	0.29	0.34	0.23	0.27	0.27	0.27	0.25	0.098	1,075
	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)		
WC Perceptions, 5 Point Scale												
Confidence a WCs will Perform Well	4.07	3.43	3.99	3.20	4.06	3.31	4.25	3.31	4.27	3.37	<0.001	1,054
	(0.10)	(0.08)	(0.10)	(0.09)	(0.09)	(0.09)	(0.08)	(0.11)	(0.08)	(0.10)		
Concern a WCs will Put Others at Risk	2.35	3.07	2.46	2.99	2.43	2.99	2.19	3.02	2.31	3.19	<0.001	1,054
	(0.12)	(0.08)	(0.11)	(0.09)	(0.13)	(0.08)	(0.10)	(0.11)	(0.10)	(0.11)		
Concern a WCs will Steal or Cause Damage	2.56	3.10	2.47	3.23	2.45	3.13	2.32	3.16	2.39	3.16	<0.001	1,054
	(0.13)	(0.08)	(0.11)	(0.09)	(0.12)	(0.13)	(0.10)	(0.10)	(0.09)	(0.10)		

Notes. Each row presents means of some attribute for each subsidy level, split by whether the respondent is willing to work with a WC at that subsidy level. Standard errors are clustered at the firm level. Column 11 shows the p-value associated with the F-statistic from the test that the means are equal for the hiring and not hiring groups at every subsidy level.Column 12 shows the number of respondents for whom the attribute of interest is available.

Appendix Table A.6: Crime and Safety Insurance, Performance History, and Conviction History

A. Additional Policy Treatment		Crime and Safety Insurance			Performance History		
	Baseline	\$5k†	\$100k	\$5m	1 Job	5 Jobs	25 Jobs
No Subsidy	0.393	0.120	0.178	0.175	0.114	0.217	0.192
	(0.032)	(0.050)	(0.052)	(0.056)	(0.053)	(0.051)	(0.051)
10% Subsidy†	0.433	0.040	0.188	0.205	0.107	0.272	0.092
	(0.037)	(0.060)	(0.057)	(0.053)	(0.054)	(0.062)	(0.053)
25% Subsidy	0.411	0.228	0.204	0.107	0.189	0.228	0.111
	(0.037)	(0.057)	(0.053)	(0.051)	(0.065)	(0.060)	(0.047)
50% Subsidy	0.542	0.096	0.129	0.191	0.136	0.104	0.090
	(0.036)	(0.056)	(0.057)	(0.047)	(0.063)	(0.048)	(0.050)
100% Subsidy	0.540	0.110	0.144	0.190	0.111	0.158	0.117
	(0.032)	(0.045)	(0.046)	(0.050)	(0.054)	(0.045)	(0.052)
Elasticity	-0.206	-0.168	-0.115	-0.208	-0.171	-0.068	-0.136
	(0.055)	(0.076)	(0.075)	(0.082)	(0.089)	(0.076)	(0.082)
Mean Effect	–	0.119	0.168	0.172	0.128	0.188	0.121
vs. Baseline		(0.024)	(0.023)	(0.024)	(0.026)	(0.023)	(0.023)
		[0.046]	[0.010]	[0.010]	[0.046]	[0.002]	[0.041]
Mean Effect	–	–	0.048	0.052	–	0.060	-0.008
vs. Omitted Group			(0.036)	(0.037)		(0.038)	(0.038)
		–	[0.516]	[0.516]	–	[0.516]	[0.753]
Firms	913	292	325	310	283	312	329
Managers	1,095	332	378	385	334	361	400

Appendix Table A.6: Crime and Safety Insurance, Performance History, and Conviction History

B. Selective Screening		Years Since Arrest or Conviction			Felony Type			Misdemeanor Type		
	Baseline	1 Year	3 Years	7 Years	Violent	Property	Drug	Violent	Property	Drug
No Subsidy	0.393	0.221	0.247	0.409	-0.333	-0.248	-0.120	-0.295	-0.115	0.120
	(0.032)	(0.049)	(0.054)	(0.048)	(0.032)	(0.031)	(0.034)	(0.031)	(0.033)	(0.032)
10% Subsidy†	0.433	0.192	0.283	0.245	-0.360	-0.300	-0.167	-0.325	-0.094	0.084
	(0.037)	(0.060)	(0.059)	(0.057)	(0.035)	(0.035)	(0.035)	(0.035)	(0.039)	(0.038)
25% Subsidy	0.411	0.216	0.343	0.282	-0.349	-0.278	-0.134	-0.316	-0.105	0.072
	(0.037)	(0.049)	(0.061)	(0.062)	(0.035)	(0.037)	(0.035)	(0.037)	(0.037)	(0.035)
50% Subsidy	0.542	0.120	0.152	0.289	-0.430	-0.369	-0.178	-0.374	-0.192	0.033
	(0.036)	(0.054)	(0.050)	(0.048)	(0.035)	(0.037)	(0.031)	(0.037)	(0.038)	(0.035)
100% Subsidy	0.540	0.126	0.262	0.166	-0.430	-0.379	-0.123	-0.366	-0.170	0.068
	(0.032)	(0.048)	(0.044)	(0.049)	(0.032)	(0.035)	(0.036)	(0.033)	(0.038)	(0.032)
Elasticity	-0.206	-0.048	-0.109	0.013	-0.116	-0.060	-0.347	-0.183	-0.163	-0.237
	(0.055)	(0.060)	(0.058)	(0.056)	(0.052)	(0.067)	(0.088)	(0.064)	(0.087)	(0.090)
Mean Effect	–	0.175	0.254	0.278	-0.381	-0.315	-0.143	-0.335	-0.136	0.076
vs. Baseline		(0.023)	(0.024)	(0.024)	(0.015)	(0.016)	(0.015)	(0.015)	(0.017)	(0.015)
		[0.003]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.002]	[0.020]
Mean Effect	–	–	0.081	0.104	–	0.066	0.237	0.046	0.245	0.457
vs. Omitted Group			(0.035)	(0.034)		(0.010)	(0.014)	(0.007)	(0.015)	(0.016)
		–	[0.180]	[0.136]	–	[0.003]	[0.000]	[0.003]	[0.000]	[0.000]
Firms	913	314	301	318	913	913	913	913	913	913
Managers	1,095	380	344	371	1,095	1,095	1,095	1,095	1,095	1,095

Notes. This table reports OLS estimates of the effects of different policies on firms' willingness to work with workers with a criminal record. In Panel A, Column 1 reports the baseline fraction of managers choosing to work with WCs at each subsidy level. Columns 2-4 report the additional effect of providing insurance covering damages related to theft or safety up to the indicated level. Columns 5-7 report the additional effect of the requiring that WCs satisfactorily complete the indicated number of jobs. In Panel B, Column 1 again reports the baseline willingness to work with WCs. Columns 2-4 report the additional effect of imposing a minimum time since arrest or conviction before allowing WCs to join the pool of workers. Columns 5-10 report the additional effect of restricting WCs to those with a given crime type. The additional effects are estimated using regressions that include interactions between the subsidy level and the indicated treatment. Mean effects are estimated using regressions that include non-interacted controls for the subsidy level. All specifications report standard errors clustered at the firm level. Westfall-Young adjusted p-values are reported in brackets, grouped by panel. See the Table 1 notes for additional details on the randomized treatments and the Table 2 notes for additional details on the outcomes and sample.

† We use different values for low levels of subsidy (5% and 10%) and crime and safety insurance ($1k and $5k) in two survey arms. For exposition, we pool the 5 and 10 percent subsidy levels and the $1k and $5k insurance levels, which results in a uniform number of observations across values displayed under the labels *10%* and *$5k*, respectively.

Appendix B. Correcting Misperceptions in Beliefs

To see how we make use of business learning, consider two businesses who have the same bias about WC performance, e.g., imagine they underestimate actual performance by 20%. One business is randomized to receive objective information about the performance of WCs and the other is not. We expect the business who did receive the information to end up with higher expectations about WC performance. More specifically, we expect the business to update so that their posterior beliefs approximately match the objective information shown, causing a 20% shock to the treated business's performance beliefs. In Appendix ??, we show that such learning indeed occurs.[1] If this shock to performance beliefs raises the business's desire to work with WCs by 10%, we measure a hiring elasticity of 0.50 with respect to beliefs about performance.

We describe our specification and the results below.

Formally, we follow Cullen and Perez-Truglia (2018) and use the following specification:

$$p_{\text{posterior},i} = \pi_0 + \pi_1(p_{\text{signal},i} - p_{\text{prior},i}) + \pi_2(p_{\text{signal},i} - p_{\text{prior},i}) * \mathtt{Info}_i + \eta H_{\text{prior},i} + \xi_i \quad \text{(B3)}$$
$$H_{\text{posterior},i} = \beta_0 + \beta_1 \hat{p}_{\text{posterior},i} + \beta_2(p_{\text{signal},i} - p_{\text{prior},i}) + \gamma H_{\text{prior},i} + \upsilon_i$$

where the information shock, $(p_{\text{signal},i} - p_{\text{prior},i})$, interacted with the treatment indicator, $\mathtt{Info}_i$, is an instrument for hiring managers' posterior beliefs. $H_{\text{prior},i}, H_{\text{posterior},i} \in \{0, 1\}$ are the hiring manager's prior and posterior willingness to work with WCs, respectively. We express prior and posterior beliefs in log terms throughout this subsection. Following Cullen and Perez-Truglia (2018), Armantier et al. (2016) and Fuster et al. (2018), this log model assumes that the relationship between outcomes and beliefs are linear in log beliefs and symmetric, which we verify in our data.[2] We estimate Equation (B3) separately for respondents assigned to the high- and low-rating treatment arms.

Table ?? provides regression estimates of these results. Panel A provides results for the high-rating treatment arm while Panel B provides results for the low-rating treatment arm. Column 1 presents first-stage estimates of the effect of information on (log) posterior beliefs, Column 2 presents OLS estimates of the relationship between (log) posterior beliefs and hiring decisions, Column 3 presents the main instrumental variable (IV) estimates of the effect of the information treatment on hiring decisions and Column 4 presents reduced form estimates. Column 1 reveals that, on average, treated participants close the gap between their prior beliefs and the truth by 34% and 46% more than the control group, for high and low performance treatment groups respectively.

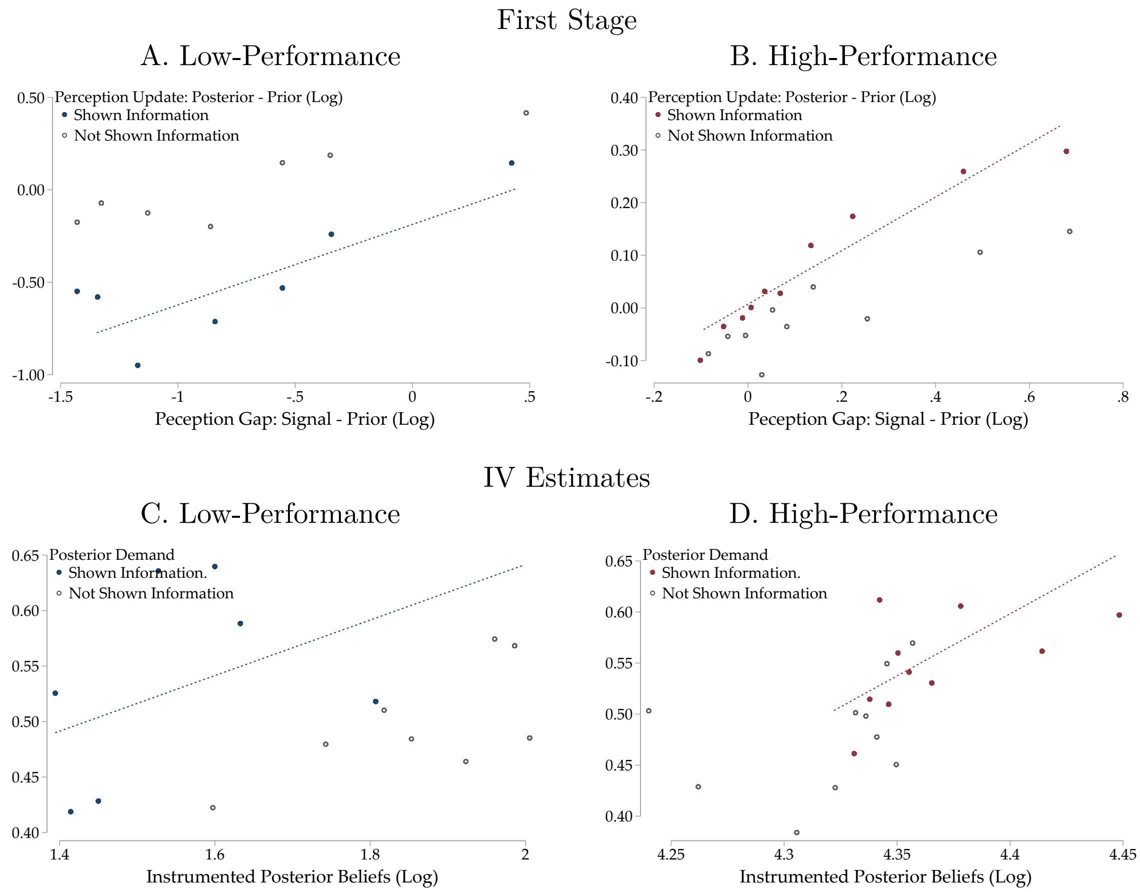
[1] There are several exceptions to this pattern of learning, as to be expected. Subjects may not pay full attention, may not believe the information shown, or may include a typo in their response, all of which would appear in the data as incomplete updating.

[2] Appendix Figure A.4 shows that while hiring managers hold a wide range of prior beliefs about WCs' productivity, they tend to significantly underestimate the productivity of WCs relative to non-WCs. Appendix Figure B.1 shows that treated hiring managers, by and large, eliminated 100% of the error in their initial guesses about WC performance. Control hiring managers also partially eliminated the error in their initial guesses, likely because they were informed that some individuals would receive objective information. We do not expect this partial updating to bias our IV estimates given our direct measures of posterior beliefs for all participants.

The main IV results in Column 3, Panel A, show that the elasticity of hiring with respect to performance beliefs about WCs is 0.81, meaning that a 10% increase in managers' beliefs about WCs' performance leads to an 8.1 percentage point increase in willingness to work with WCs or a 15% increase in hiring.[3] This means that causing managers to update their beliefs by 10% has a similar effect as providing a 100% wage subsidy, and only a slightly larger effect than providing businesses with $5,000 of insurance or requiring WCs to successfully have completed at least 1 prior job. In contrast, Panel B of Table **??** shows that changing perceptions in the low-performance group has no impact on hiring decisions. Our interpretation of this result is that the share of low ratings or no-shows is less salient and less relevant for WC hiring decisions. Consistent with this interpretation, hiring managers also have more dispersed priors about low ratings and no-shows at baseline.

[3]The IV coefficient is 2.5 times larger than the OLS coefficient of 0.33 in Column 2, as is common in many information provision experiments (Gerber et al., 2020), consistent with substantial attenuation bias due to measurement error in beliefs. Such measurement error likely reflects that predicting performance is unfamiliar to many businesses. It may also reflect inattention.

Appendix Figure B.1: IV Estimates: Randomized Provision of WC Performance Information



Notes. This figure reports binned scatter-plot estimates of the impact of high- and low-performance information on business beliefs and hiring decisions. Panels A-B report first stage results. We plot the difference between the reported performance beliefs at the end of the experiment and prior beliefs against the perception gap. For graphical exposition, we plot the belief update rather than the posterior belief because this allows a simpler interpretation of updating from information: observations along the 45 degree access imply the manager updated completely from her prior to a posterior that exactly matches the information shown. Panels C-D reports IV results. we plot the willingness to work with WCs at the end of the experiment against the fitted posterior belief predictions from the first stage regression. Panels A and C present results for being shown information on the fraction of no-shows and either 1- or 2-star ratings; Panels B and D present results for being shown information on the fraction of 5-star ratings results for being shown information on the fraction of no-shows and either 1- or 2-star ratings. See Section 5 of the text for additional details.