Measuring Labor Demand for Workers with a Criminal Conviction*

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PRELIMINARY AND INCOMPLETE.

Abstract

We employ a discrete choice field experiment on a large on-demand staffing platform serving thousands of traditional firms to estimate the labor demand for workers with a criminal conviction (WCCs). Thirty-nine percent of firms are willing to hire WCCs at baseline, with higher levels of demand when vacancies are hard to fill and for jobs that do not involve customer interaction or high-value inventory. Wage subsidies modestly increase the level of demand for WCCs above these levels, with significantly larger increases from crime and safety insurance, performance screening, including only workers with a less serious conviction, and providing objective information on the productivity of WCCs. Based on the experiment's findings, the platform modified its user-interface so that firms can post jobs that do not require a criminal background check but are covered by crime and safety insurance.

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1. Introduction

Employers are significantly less likely to interview or hire workers with a past criminal conviction (WCCs) compared to otherwise similar workers without a conviction (e.g., Pager, 2003; Holzer et al., 2006; Holzer, 2007; Agan and Starr, 2017). In 2008, for example, the average unemployment rate among WCCs—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 WCCs 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). Survey work suggests that wage subsidies or certificates of validated work performance history may be cost-effective ways to increase employer demand for WCCs (e.g., Holzer, 2007; Hunt et al., 2018), but the hypothetical (and hence, low-stakes) nature of these surveys makes it difficult to know whether employers are expressing their true preferences or just their aspirations.

In this paper, we employ a discrete choice field experiment on a nation-wide on-demand staffing platform to estimate the actual labor demand for WCCs under different policy counterfactuals. The context for our study is a large anonymous online labor platform (hereafter, "the Platform"), which thousands of traditional firms use to source workers for temporary staffing, often in large numbers. Like peer platforms, up to 30% of Platform applicants interested in employment are currently screened out by a criminal background check.

The Platform provides an ideal setting to investigate the actual labor demand for WCCs for several reasons. First, traditional firms use the Platform to fill a wide range of entry-level jobs. These jobs include entry-level roles in sectors that are traditionally more open to hiring WCCs, 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 WCCs (e.g., Holzer et al., 2004; Raphael, 2010; Yang, 2017; Schnepel, 2018). Second, the Platform's labor market is simple, with firms listing required qualifications and the Platform assigning workers that meet the criteria. This simplified labor market means that we can ask firms to make incentive-compatible choices over *actual hiring decisions*, as opposed to over callback or interview decisions considered in past work. This distinction is potentially important, as the decision of who to call back or interview may differ substantially from the decision of who to hire (Jarosch and Pilossoph, 2019). Finally, the Platform collects daily data on individual worker performance at all jobs, including the performance of some WCCs that completed several jobs while their criminal background check was being processed. These data allow us to measure the objective performance of both WCCs and non-WCCs in identical jobs and provide this information to a subset of firms.

In the experiment, over 1,000 hiring managers on the Platform were asked whether their firm would permit WCCs to be assigned to their jobs under different randomized conditions. Randomization occurred on the availability and level of wage subsidies, crime and safety insurance, past performance history, and potential local unemployment levels. Hiring managers were also

asked questions specific to the type of conviction, and a subset of managers were provided with objective information on the productivity of WCCs compared to non-WCCs. The hiring managers were truthfully informed that their responses may be implemented and could therefore affect whether or not WCCs would be matched to their jobs in the future, broadly building on the methodologies developed in Low (2017), Mas and Pallais (2017), and Kessler et al. (2020) to generate incentive-compatible responses in field experiments. We extend these methodological advances by working with a real corporation to develop and implement new policies supported by such incentive-compatible responses.

We find that a sizable share of firms, 39%, are willing to hire WCCs without additional incentives or conditions. The level of demand, still with no 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 firms perceiving risks related to customer safety or inventory theft. We also find that the share of firms willing to hire WCCs increases to 68% if the Platform is having a hard time filling a job, consistent with firms being more likely to consider non-traditional workers when jobs are hard to fill. In contrast to our results using this targeted and context-specific measure of willingness to hire in tight labor market conditions, we find that the level of demand is largely unaffected by randomly assigned potential labor market conditions or non-randomly assigned actual local labor market conditions. The level of demand also did not significantly change during the experiment as the COVID-19 crisis intensified and large numbers of workers were permanently laid off or furloughed.

Turning to the main treatments, we find that the share of firms willing to hire WCCs further increases by approximately 2.1% for every 10% increase in the offered wage subsidy. There are significantly larger increases, however, from a range of alternative policies. Crime and safety insurance covering damages up to $5,000, for example, increases the level of demand for WCCs by 12 percentage points, approximately equivalent to the effects of an 80% subsidy according to a linear extrapolation of our experimental estimates. Requiring WCCs to have satisfactorily completed one prior job on the Platform increases the level of demand by 13 percentage points, roughly equivalent to the effect of an 80% subsidy, and only including workers with less serious or misdemeanor convictions increases the level of demand by 24 to 46 percentage points compared to violent felony convictions, greater than the effect of a 100% wage subsidy.[1]

The final option we consider is providing hiring managers with objective information on the performance of WCCs on the Platform. We exploit the fact that some WCCs inadvertently

[1]Our estimates are broadly consistent with prior work in related contexts. Pallais (2014) finds, for example, that there are large benefits of randomly providing past performance history for novice workers on the online platform oDesk. 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 felony conviction on call-back rates (Agan and Starr, 2017). There is also a large literature studying the effects of "Ban the Box" policies that prohibit firms from asking about an individual's criminal history on a job application, with the overall evidence indicating that these policies tend to increase hiring discrimination against Black men (Bushway, 2004; Holzer et al., 2006; Stoll, 2009; Shoag and Veuger, 2016; Agan and Starr, 2018; Doleac and Hansen, 2020). We are unable to test for such spillover effects in our setting, as firms are unable to screen by race on the Platform.

access the Platform before their background screening results are known, allowing us to compare the objective performance ratings of WCCs and non-WCCs in their first jobs on the Platform. We find that hiring managers underestimate the share of high-performance ratings received by WCCs and overestimate the share of low-performance ratings that WCCs receive. Providing objective information on the true share of high-performance ratings received by WCCs leads to more accurate beliefs and increases WCC 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 WCC hiring by a statistically insignificant 1.5 percentage points.[2]

Under reasonable assumptions, our estimates imply that a range of policies may significantly increase demand for WCCs at a fraction of the cost of wage subsidies. Performance screening, for example, can achieve substantial gains in the share of firms willing to hire WCCs at near-zero cost if WCCs are initially matched with firms willing to waive this requirement, or at the small one-time cost of subsidizing the first set of matches until there is sufficient performance history. Providing objective information on the average productivity of WCCs can similarly increase the share of firms hiring WCCs at essentially zero additional cost to the Platform. Including WCCs with less serious or misdemeanor convictions also requires no new costs for the Platform, although they would need to continue paying for background checks to determine the type of conviction. Even actuarially fair crime and safety insurance, the most expensive alternative we consider, can increase the demand for WCCs at one-tenth or less the cost of wage subsidies under various assumptions of the probability of damages due to WCC misbehavior.

Beyond demonstrating that a range of policies can increase the demand for WCCs, our paper provides new evidence on why employers may be less likely to hire WCCs compared to otherwise similar non-WCCs. Employers may see WCC status as a potential marker of either lower expected productivity or a higher probability of a very negative event for the employer. For example, an employer may worry that a WCC would pack boxes more slowly than a non-WCC (i.e., lower productivity) or that they would be more likely to get in a fight with a coworker or customer (i.e., higher downside risk). There are also a set of minor infractions (e.g., petty theft) and major infractions (e.g., violence) that an employer may be concerned about which have different implications for the design of effective alternatives to the background screening. Our results can inform the policy discourse by providing evidence on the relative importance of these forces.

We find the greatest empirical support for the idea that WCC status matters for employers' hiring decisions because it raises the perceived likelihood of modest on-the-job risks and decreases the perceived likelihood of performance excellence. First, the large demand response to jobs with high-value inventory suggests that many firms are sensitive to the downside risk of hiring WCCs, and in particular the possibility of theft. Second, the large demand response to

[2]Our estimates are sizable compared to other information treatment experiments. Bursztyn et al. (forthcoming), for example, 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%.

an insurance policy with a relatively small cap of $5,000, and the similarity between demand responses to insurance offerings with $5,000 and $5 million caps, supports the hypothesis that WCC status raises the perceived risk of minor infractions occurring on-the-job. Third, the demand for a WCC from the full pool of WCCs is substantially higher than the demand for a WCC that has a felony or any violent crime in the past, suggesting that employers believe a typical WCC is unlikely to have committed these more serious infractions. All three findings are consistent with the view that the perceived prevalence of modest on-the-job risks rises with WCC status. The positive response to objective information on the average productivity of WCCs also suggests that other firms view WCCs as less productive on average than otherwise similar non-WCCs. The differential demand response to performance information about the rate of no-shows versus the rate of excellence suggests that misperceptions about excellence are most detrimental to the demand for WCCs.[3]

Based on our findings, the Platform modified its user-interface so that thousands of employers posting jobs could have the option of hiring WCCs. The Platform also provides the modest level of crime and safety insurance that our study finds boosts demand for WCCs in a cost-effective manner, and excludes WCCs with violent or sexual felony convictions where our study suggests particularly low demand. Together, these policies are meant to address the concerns related to left-tail risk (e.g., the potential for a theft or violent act on the job) that our study suggests may be a key underlying reason that employers request a criminal background screening. The new policy opens new questions for future research, including the evolution of demand for WCCs as employers gain experience working with WCCs, and long-term employment opportunities created for workers that are matched as a result of this policy change.

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 workers with a criminal conviction with and without wage subsidies and explores heterogeneity. Section 4 presents results for our primary experimental interventions, Section 5 presents results from the information experiment embedded in our survey, 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 firms use to source workers for temporary staffing, often in large numbers. Firms use the Platform to fill a wide range of entry-level jobs in sectors that report being more willing to hire WCCs, such as

[3]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 firms from the perceived downside potential of hiring disadvantaged workers for short-term positions.

general labor and transportation, as well as entry-level roles in customer-facing or administrative sectors that are traditionally more averse to hiring WCCs (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 labor demand for WCCs.

The Platform's labor market is simple, another advantage for our study as it allows us to ask firms to make incentive-compatible choices over hiring decisions. Firms post job listings that include a short description, the hourly wage, and any qualifying criteria. For example, some listings require that workers have experience driving a truck or are comfortable with heavy lifting. Firms do not decide whether to hire individual 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. The firm must pay a cancellation fee of 50% of the wage bill to unmatch from the worker after this point, with less than 1% of matches being canceled in practice. By asking firms to make decisions about what workers they would allow in their pool, we are therefore asking firms 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).

The majority of workers who are hired once are hired repeatedly by the same employer. Workers can also be rehired as W2 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.

The Platform collects data on worker performance from firms, a feature that allows us to provide firms with information about the performance of WCCs. At the end of each job, the firm'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% of the overall ratings are 5-stars and approximately 5% are 1- or 2-stars. No-shows comprise an additional 4% of the overall ratings. Given the bi-modal nature of the ratings, we define good performance as a large percentage of perfect 5-star ratings and poor performance as a large percentage of low-ratings and 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 employers might care more about mitigating the risk of a bad outcome than about average performance or ability to perform well.

Like many other online labor platforms (e.g., Uber, Lyft), up to 30% of Platform applicants

are currently screened out by a criminal background check. The Platform is interested in expanding work opportunities for individuals with a criminal conviction for several reasons. The first is that the Platform's performance data suggests that WCCs can contribute productively to the marketplace and expand the pool of available workers. The second reason is that the criminal background check is among the Platform's largest per-user expenses, and moving to a no-background check equilibrium would allow the Platform to reduce costs. The final reason is that the state of Washington recently introduced legislation to limit the use of background checks, leading to a broader conversation at the Platform on the most appropriate use of criminal background checks. The Platform is thus actively searching for alternatives to criminal background checks and agreed to partner with us in a randomized experiment to understand the potential barriers to hiring WCCs in their context.

2.2. Experimental Design

In the experiment, we worked with the Platform to ask hiring managers whether their firm would permit WCCs to be matched to their jobs under different randomly-assigned conditions. Randomization occurred at the firm level to ensure that hiring managers at the same firm were not given conflicting options. Starting on March 6, 2020, emails were sent to all 7,450 hiring managers who had been active in listing positions on the platform in the last 16 months.Hiring managers were contacted up to five times between March 6 and April 11. 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." Respondents were offered either a $35 or $50 Amazon gift card for completing the survey. The emails did not mention WCCs, and hiring managers were not aware that they were part of a randomized study. In total, 1,095 hiring managers from 913 firms completed the survey experiment, or 14% of the hiring managers contacted. Conditional on opening the survey, the completion rate was 86%.

Following a series of short introduction questions, participating hiring managers were shown 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.

Participating hiring managers were then asked about their willingness to hire WCCs under different randomized conditions. Managers could respond by saying "Yes" they would hire WCCs, "No" they would not hire WCCs, or that they would only hire WCCs "if it's hard to fill [their]

jobs." These questions are incentive-compatible because the respondents are aware that their responses may affect whether the Platform assigns WCCs to their jobs. Recall that after the hiring manager agrees that a pool of workers meet the criteria to accept their jobs, these are the workers that will ultimately show up to carry out the job. There is no additional screening that occurs once a worker receives a job posting from the platform and accepts it. Thus, these questions can be regarded as expressing firms' labor demand or actual hiring response under different conditions.

The experiment began by asking hiring managers about their willingness to accept WCCs under one of several randomly assigned wage subsidy levels to establish the baseline level of demand for WCCs. The randomly assigned subsidy level was maintained through all subsequent questions. Next, hiring managers were asked about their willingness to accept WCCs under different additional conditions, such as if insurance or additional performance screening were offered in addition to the subsidy. After that, managers were asked a series of descriptive questions about the hiring practices at the firm and the types of jobs posted on the Platform. The survey experiment concluded with an information treatment experiment motivated by the large dispersion in prior beliefs about the performance of WCCs on the Platform, which is described in detail in Section 5. The remainder of this section summarizes the most important details of the survey, also detailed in Table 1 and Appendix Table A.1.

Wage Subsidies and the Baseline Level of Demand. The baseline level of demand for WCCs is measured by asking hiring managers whether their firm would permit WCCs to be matched to their jobs under one of several randomly assigned wage subsidy levels:

> If the Platform gave you a [wage subsidy] discount for users with a criminal record, would you permit such users 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 users would still receive the full pay amount after the discount (the Platform would pay the difference).

The wage subsidy levels were 0%, 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. Prior work suggests, for example, that wage subsidies may have small or even negative effects on the labor demand of disadvantaged workers, in part because these subsidies may further stigmatize such workers (Burtless, 1985; Katz, 1996). We might therefore expect that there will be small or even negative effects of the 5% or 10% wage subsidies if stigma effects are particularly important in our context. For exposition, we pool the 5% and 10% subsidy levels, which results in a uniform number of observations across values displayed.

Crime and Safety Insurance. The effect of crime and safety insurance is measured by asking

hiring managers if, at a given subsidy level, their firm would permit WCCs to be matched to 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 workers with a criminal record, would you permit such Platform users 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.[4] 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. The effect of screening based on performance history on the Platform is measured by asking hiring managers if, at a given subsidy level, their firm would permit WCCs to be matched to their jobs under one of several randomly assigned job histories:

> If the Platform required users of the Platform with a criminal record to have satisfactorily completed [performance history] job(s), receiving more than 85% 5-star reviews, would you permit such users 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.

Screening Based on Conviction History. The effect of selectively screening based on conviction history is measured by asking hiring managers if, at a given subsidy level, their firm would permit WCCs to be matched to 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)

[4]The \$5 million cap is not artificially high in this context. 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.

a property/financial misdemeanor, (5) a violent misdemeanor, or (6) a substance-related misdemeanor. Hiring managers were asked about each crime type, allowing us to estimate the demand for workers convicted of each of these crime types. Hiring managers were also asked if, at a given subsidy level, their firm would permit WCCs to be matched to their jobs if they committed their last offense at least 1, 3, or 7 years ago, with these values randomly chosen with 1/3 probability each. These randomized values were chosen because the probability of criminal re-offending is particularly high in the first two years post-incarceration, while background checks are often limited in asking about criminal convictions in the last 7 years.

Objective Performance Information. We measure how providing hiring managers with objective performance information about average WCC productivity affects hiring demand. To provide objective information, we exploit the fact that some WCCs inadvertently access the Platform before their background screening results are known, allowing us to compare the performance ratings of WCCs and non-WCCs in their first jobs on the Platform.

Information was also collected about baseline beliefs about WCC performance, as well as how employers update those beliefs after seeing information, for three reasons. First, understanding the degree of misperception among employers with respect to WCC performance teaches us about the salient signals contained in WCC status, and sheds light on why employers request information about WCC status. Second, we want to precisely estimate the relationship between performance beliefs and hiring demand as opposed to the relationship between a particular performance-message and hiring demand. Thirdly, we want to be able to describe the impact of performance information in terms of its impact on beliefs so that the results can be more easily applied to other settings where policy makers can measure "updating performance beliefs" but cannot measure underlying hiring demand. For example, by framing our results in terms of beliefs, we can report an elasticity, with a 10% increase in beliefs about the average performance of WCCs, demand rises by X%. In other settings, policy makers may design their own messages to shift beliefs.

Prior beliefs about WCC performance were collected in an incentive-compatible manner by using a guessing game about the relative performance of WCCs on the Platform and rewarding accuracy. We used an objective measure of WCC performance to reward accuracy, exploiting the fact that 265 WCCs 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, the following question was asked about high-performance:

> In 2019, 86% of jobs on the Platform resulted in a 5-star rating. What percentage of jobs completed by people 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!

The other half of the managers were asked 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 people 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!

Half of these participants were then randomly assigned 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 people 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 people 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.

All participants were then asked, regardless of whether or not they were shown the new objective information, to report their posterior beliefs about the performance of WCCs by replicating the same guessing game question. Finally, participants were allowed to revise their answer to the very first question about hiring WCCs after eliciting posterior beliefs. Since participants are allowed to revise their willingness to hire WCCs, we can learn how the information about performance impacted hiring decisions.

Heterogeneity by Labor Market Conditions. We explore heterogeneity by labor market tightness, exploiting that hiring managers could choose to match with WCCs only when they are having difficulty filling their position. For all of the questions on the survey 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 firm's context. In addition, we also 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 hire WCCs if the local unemployment rate were to be at a certain level, randomized between 2%, 6%, or 10%. Finally, we estimate results separately for firms located in counties with above- and below-median unemployment rates as of March 2020 (e.g., Los Angeles County with 6.6% unemployment vs. San Francisco County with 3.1% unemployment) and for firms 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, hiring managers were asked about the typical jobs they post, including whether there are any customer interactions or access to high-value inventory, thereby allowing us to explore heterogeneity by job type. Managers were also asked whether their company or organization has a hiring policy related to WCCs.

2.3. Motivating Framework

The experimental design described above is motivated by a stylized theoretical framework of how employers decide whether or not to hire a WCC worker. The framework formalizes the idea that employers may see WCC status as a potential marker of either expected productivity or the probability of a very negative event for the employer. For example, an employer may worry that a WCC may pack boxes more slowly than a non-WCC (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 show how the effectiveness of alternative policies used in our experiment can shed light on the primary uses of WCC status by employers at the time of hiring.

Consider a single firm deciding whether or not to hire a single WCC. The firm's expected profits from hiring the WCC 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 WCC (e.g., the rate at which the worker packs boxes), w is the WCC's wage, b is the probability of a bad event occurring as a result of the WCC'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 firm has an unobserved shadow value, θ, of not hiring the WCC (e.g., there is some probability that the firm can fill the slot instead with a non-WCC). The firm chooses to hire the WCC, $H = 1$, when $\pi > \theta$. The first prediction is that wage subsidies increase demand for WCCs 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 employer has strong alternative options to hiring WCCs, the shadow value of labor in our framework, θ, rises. The employer chooses to hire the WCC, $H = 1$, when the value reaped from hiring is greater than the alternative, $\pi > \theta$. Hence we expect that demand for WCCs is lower when the employer faces favorable labor market conditions, and their job is easy to fill.

Heterogeneity by Job Characteristics. Firms 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 WCC demand if downside risk is a relevant factor for firms. 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, employers may also be concerned that the general presentation of a worker with a past criminal conviction 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 WCC hiring as long as downside risk is a relevant factor for firms. The effect of insurance on hiring should be larger for firms with a greater probability of, or larger costs from, a bad event. This would include firms 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 WCC successfully complete a prior job can be viewed primarily, but not exclusively, as increasing the expectation about productivity, y, for that worker. While screening could also decrease the perceived probability of a bad event 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 WCCs if either productivity or downside risk is a relevant factor for firms. If firms have negatively biased beliefs about y and b for WCCs, providing objective performance information regarding WCC performance can also increase demand for WCCs.

Screening Based on Conviction History. Expected productivity y may be higher and both the probability of a bad event b and the cost of that bad event k may be lower for WCCs convicted of less serious crimes or crimes committed less recently. This leads to higher demand for WCCs convicted of less serious or less recent crimes compared to WCCs convicted of more serious or more recent crimes if either productivity or downside risk is a relevant factor for firms.

2.4. Descriptive Statistics and Randomization Assessment

The experimental sample is comprised of the 1,095 hiring managers from 913 firms that completed the survey experiment. 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. Firms 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). Firms 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 WCC hiring policies from our survey 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, firms in our experimental sample are only somewhat less likely to have a firm-wide WCC hiring policy (45% vs. 66%) and to indicate that they want to hire the best candidate for the job regardless of criminal history (46% vs. 53%). Slightly more firms in our sample indicated that they would want to hire WCCs to help give individuals a second chance (50% vs. 38%) or for financial incentives (8% vs. 2%), and a similar number of firms in both samples are concerned about local or state regulations that make hiring WCCs difficult (26% vs. 22%).

Appendix Table A.2 shows that the randomization was also balanced in our experimental sample. We regress fifteen firm 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 five% level and only five are significant at the ten% 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 WCCs

In this section, we measure the labor demand for WCCs using the randomized wage subsidies. We first analyze the effects of the wage subsidies on the willingness to hire WCCs for all jobs, before turning to its effects for different types of jobs and local labor market conditions. We present our main results graphically, providing corresponding regression tables in the Web Appendix.

3.1. Baseline Results

Panels A and B of Figure 1 plot mean willingness to hire at each subsidy level. Panel A uses our main, pre-registered, baseline definition of willingness to hire, wherein we code firms as willing to hire WCCs if they responded "Yes" to the relevant question, and unwilling to hire WCCs if they responded "No" or "Only if it's hard to fill my jobs." To provide a targeted, context-specific measure of willingness to hire in a tight labor market, Panel B uses a modified version of this definition, where we group the "Only if it's hard to fill my jobs" responses with the "Yes"

responses, as discussed below.[5] The horizontal axis plots the effective wage, equal to 100% of the wage minus the given wage subsidy. The vertical axis plots the fraction of firms that respond that they are willing to hire WCCs at the given effective wage.

Appendix Table A.3 presents the same results as Figure 1 in regression form, using the following specification that includes one observation per respondent:

$$Hire_i = \sum_{k \in K} \beta_k \cdot Subsidy_{ik} + \xi_i \tag{1}$$

where $Hire_i$ is firm i's WCC hiring decision, $Subsidy_{ik}$ is a set of indicator variables for the assigned wage subsidy in the set K = {0%; 10%; 25%; 50%; 100%}, and ξ_i is an error term. We include all possible wage subsidy levels and omit the constant term. Therefore, the β_k coefficients represent regression-weighted estimates of mean willingness to hire at each subsidy level. We omit hiring manager subscripts for notational simplicity but cluster standard errors by firm in this and all other specifications, reflecting that treatments are randomized by firm.

Figure 1 also includes an estimate of the demand elasticity, ϵ^D, for our main and alternate definitions of willingness to hire. To estimate ϵ^D, we first calculate the following the linear specification with one observation per respondent:

$$Hire_i = \gamma_0 + \gamma_1 \cdot EffectiveWage_i + u_i \tag{2}$$

where we include $EffectiveWage_i$, defined as a percentage equal to $100\% - Subsidy_i$, as a pseudo-continuous variable. We then estimate ϵ^D using $\epsilon^D = \hat{\gamma}_1 \cdot \frac{\bar{w}}{\bar{h}}$, 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 elasticity for the main results.

We find that a highly substantial 39% of firms are willing to hire WCCs with no subsidy. With a full subsidy, meaning that there are no out-of-pocket costs for the firm, this jumps to 56%, which is 44% (17 percentage points) above the no subsidy hiring rate.[6] The baseline elasticity of demand is equal to -0.21, meaning that for every 10% increase in the wage subsidy, about 2.1% more firms are willing to hire WCCs. Our ϵ^D= -0.21 is broadly in line with elasticities estimated for general populations, though somewhat lower in magnitude than other estimates.[7]

[5]Our pre-analysis plan specified that our main results would be based on coding "Yes" as 1 and "No" or "Only if it's hard to fill my jobs" as 0, but that we would also consider robustness where "Only if it's hard to fill my jobs" is coded as a 1.

[6]The point estimates shown in Panel A of Figure 1 are slightly non-monotonic, but there are no points where demand is significantly higher at a higher price. For example, the hiring rate is 44% at the 10% subsidy, whereas it is 41% at the 25% subsidy, but these hiring rates are statistically indistinguishable. In contrast, we can often reject that demand is lower at higher prices, e.g., we find that demand is significantly higher with a 50% subsidy relative to a 25% subsidy. The simplest interpretation of the results is that the WCC labor demand curve is consistently downward sloping, but that our non-parametric estimates include significant sampling error due to the relatively small number of firms in each point.

[7]Labor demand elasticities estimated using industry data typically range between -0.15 and -0.75 (Hamermesh, 1996), so ours is in line with these. Our elasticity is smaller in magnitude than those estimated using aggregate data, such as Acemoglu et al. (2004), who estimate a labor demand elasticity for women of -1.0 to -1.5, consistent with firms potentially having less elastic demand for WCCs than non-WCCs (or for the general population). Our elasticity

3.2. Heterogeneity in Demand by Labor Market Conditions

In Figure 1, Panel B, we consider respondents who ask to be matched to WCCs "Only if it's hard to fill my jobs". These respondents are willing to hire WCCs, albeit only when they are personally facing tight labor market conditions for the role. Under such conditions, demand for WCCs rises an additional 25 percentage points on average. Without any additional subsidy, the share of employers willing to match with WCCs jumps from 39% to 68%, representing a 74% increase in demand for WCCs. Unsurprisingly, the elasticity of demand with respect to wages falls when jobs are hard to fill, by nearly two-thirds, as employers become less responsive to wages in an attempt to fill roles.

We also explore demand as a function of local labor market measures that are more commonly found in the literature, namely local unemployment rates, which capture more general features of the local labor market while being one step removed from conditions facing a particular employer hiring for a particular role.

In Appendix Figure A.1 we show demand split by three ways of measuring labor market tightness conditions: Panel A shows demand by above and below median county-level unemployment; Panel B shows demand under the potential unemployment rate that we stipulate, either 2%, 6% and 10% local unemployment randomly assigned; Panel C shows demand by above and below median county-level COVID-19 cases, a driver of unemployment largely orthogonal to other local conditions.

Panels A, B, and C show that there is no economically significant change in willingness to hire WCCs based on actual local unemployment, randomized unemployment level criteria, or intensity of the COVID-19 pandemic. In unreported results, we similarly find no effect if we split in other ways (e.g., above mean instead of median) or if we focus on the local unemployment rate for workers with a high-school degree or less.

Taken together, we interpret these results as suggesting that firms' interest in WCCs does appear to reflect how hard it is for firm to fill jobs. However, for the firms we study (as well as likely other firms), the difficulty in filling jobs may not be primarily captured by the local unemployment rate. For example, a firm with warehousing jobs may experience large swings over time in labor needs (e.g., due to differences over time in product demand). The results also suggest that the significant labor demand we document for WCCs is unlikely to evaporate in weaker local labor markets or one where the community is recovering from the COVID-19 pandemic.

3.3. Heterogeneity by Job Characteristics

Following our motivating framework described above, Panels A and B of Figure 2 split our sample based on two salient job characteristics. Panel A divides our main sample in to two groups based on whether firms report that their jobs involve customer interaction and Panel

is in line with the own price elasticities of roughly -0.3 in Borjas (2003), and is a bit lower than the elasticity of -0.5 discussed as reasonable for low-wage workers in Katz (1996).

B divides based on whether firms report that their jobs involve access to cash or high-value inventory. Columns 3-6 of Appendix Table A.3 present regression estimates of mean willingness to hire split by the same job characteristics. Both panels plot mean willingness to hire for each groups. Each panel indicates what percent of our sample reports the relevant job characteristic. We also report an estimate of the difference between the two demand curves, Δ, in each panel. Δ provides a summary of how much demand varies by job characteristic. We calculate Δ using the data underlying both curves and the following specification:

$$\text{Hire}_i = \Delta \cdot \text{JobCharacteristic}_i + \sum_{k \in K} \lambda_k \cdot \text{Subsidy}_{ik} + \epsilon_i \tag{3}$$

As predicted by the motivating framework in Section 3, Panel A of Figure 2 shows that firms with jobs that involve access to cash or high-value inventory are 18 percentage points less willing to hire WCCs, pointing to a perceived risk of theft. Panel B shows employers hiring for jobs involving customer interaction are 13 percentage points less likely to hire WCCs, and demand is lower at every subsidy level. This suggests that some firms may refrain from hiring WCCs due to a perceived risk to their reputations with customers.

4. Crime and Safety Insurance and Targeted Screening

This section measures the effects of crime and safety insurance, performance screening, and screening based on conviction history on the labor demand for WCCs, holding fixed the subsidy level established in the first part of the survey experiment.

4.1. Crime and Safety Insurance

Figure 3 builds on Panel A of Figure 1 by adding three additional lines that represent mean willingness to hire at each subsidy level given a randomly assigned crime and safety insurance policy that covers damages up to $5,000, $100,000, or $5 million. Figure 3 also reports Δ, the mean effect of each level of insurance across subsidy levels compared to baseline. Columns 2-4 of Appendix Table A.6 provide regression estimates of the same results and additionally present estimates of elasticity of demand. The regression results use the following fully-interacted, stacked specification where each hiring manager has one observation for their baseline hiring decision and one observation for their decision under the randomly assigned insurance level. This allows both the level and elasticity of demand to change with each treatment:

$$\text{Hire}_{it} = \sum_{k \in K} \beta_k \cdot \text{Subsidy}_{ik} + \sum_{z \in Z} \sum_{k \in K} \delta_{kz} \cdot (\text{Subsidy}_{ik}) \cdot (\text{Insurance}_{itz}) + \eta_{it} \tag{4}$$

where Hire_{it} is the hiring decision of firm i with and without the insurance treatment t. Subsidy_{ik} represents indicator variables for the assigned wage subsidy and does not depend on the randomized treatment t. Insurance_{itz} represents indicator variables for the randomly assigned

insurance level in the set $Z = \{\$5{,}000; \$100{,}000; \$5{,}000{,}000\}$ that are set to zero for the baseline hiring decision before the insurance option is offered. In this specification, as in Equation (1), the β_k coefficients represent the regression-weighted estimates of mean willingness to hire at each subsidy level without insurance. The δ_{kz} coefficients represent the additional impact of providing insurance level z to a firm with wage subsidy k.

Figure 3 shows that providing crime and safety insurance significantly increases the level of demand for WCCs. With no subsidy, insurance that covers damages up to $5,000 increases demand for WCCs by 12 percentage points. Averaged over all of the subsidy levels, the mean effect of insurance up to $5,000 is to increase demand for WCCs by 12 percentage points (31%). This is equivalent to the effects of a 80% wage subsidy, based on a linear extrapolation of our baseline estimates. This result is striking given that the $5,000 cap was chosen to be equal to that of the hardly-used U.S. Federal Bonding program, raising the possibility that the Bonding program's low usage reflects non-demand-based reasons (e.g., that firms don't know about it or its use it stigmatized).

Insurance covering greater damage has a somewhat larger effect. An insurance policy with a cap of $100,000 increases the share of firms willing to hire WCCs by 17 percentage points (45%) with no subsidy. Averaged over all the subsidy levels, it increases demand for WCCs by 17 percentage points. Providing $5m in insurance yields similar effects to providing $100,000. Overall, the results suggest that firms are particularly concerned about moderate damages from WCC hiring (e.g., due to moderate theft), and also point to some concern about more severe tail risk events.

4.2. Screening Based on Performance History

Figure 4 plots the baseline mean willingness to hire at each subsidy level, as in Figure 3 described above, as well as lines representing mean willingness to hire WCCs who have satisfactorily completed either one, five, or twenty-five previous jobs on the platform. Figure 3 also reports Δ, the mean effect of each number of completed jobs compared to baseline. Columns 5-7 of Appendix Table A.6 present corresponding regression estimates using a similar specification as used for the analysis of the insurance treatment.

Figure 4 shows that screening by performance history also substantially increases demand for WCCs. With no subsidy, employers are 10 percentage points (27%) more willing to hire WCCs when WCCs have successfully completed at least one prior job, compared to the baseline with no performance screening. Across all subsidy levels, the mean effect of requiring WCCs to have completed one prior job is an increase in demand by 13 percentage points. This is roughly equivalent to the effects of a 80% wage subsidy or a $5,000 crime and safety insurance policy.

Firm demand for WCCs increases by a modest amount if WCCs are required to have completed more than one prior job satisfactorily. Requiring WCCs to have completed five or twenty-five prior jobs boosts demand by 19 and 13 percentage points respectively, relative to the baseline.

These results suggest that firms see WCCs as heterogeneous in their suitability for hire, and

that they believe that even a single positive or negative review may serve as a reliable screening tool. These effects are consistent with similar work in other contexts. For example, Pallais (2014) hires inexperienced workers on the online platform oDesk for a data entry job and provides detailed reviews for a randomly selected group of workers. She finds that providing a single job's worth of experience along with a positive review has large positive benefits for future employment and wages on the platform.

4.3. Screening Based on Conviction History

Figures 5 and 6 plot the baseline mean willingness to hire at each subsidy level, again like the above figures, as well as lines representing mean willingness to hire WCCs at each subsidy level given that it has been one, three, or seven years since the individual was convicted. Columns 2-4 of Appendix Table A.6 report the regression version of these results using a similar specification as used for the other randomized treatments. Panels A and B of Figure 6 similarly plot mean willingness to hire WCCs at each subsidy level given the specific crime type and severity. This figure also presents estimates of Δ, representing the mean impact of each crime type on willingness to hire relative to the baseline crime type of violent felony. Each survey participant was asked about their willingness to hire someone who fits each crime type, holding their subsidy level fixed. Hence, the observation counts underlying the mean willingness to hire by crime type in Figure 6 are larger than in other panels where a random subset of participants were asked about each condition. Panel A plots demand for WCCs with felony convictions, while Panel B considers misdemeanor convictions. Columns 4-9 of Appendix Table A.6 report the regression version of these results, along with estimates of demand elasticities.

Figures 5 and 6 show that offering firms the opportunity to screen based on conviction history can also substantially boost WCC labor demand. Screening WCCs so that they are only permitted to accept jobs if it has been at least one year since their conviction increases demand substantially. With no subsidy, it increases demand by 21 percentage points (54%). The mean effect across all subsidy levels compared to baseline is a 17 percentage point increase (44% percent). These results are greater than the effects of a 100% wage subsidy. Effects are even larger if it has been at least five or seven years since the conviction. These results suggest that firms prefer candidates with older convictions. This could reflect concern that recently released WCCs may have higher risk of recidivism or downside events (given that the hazard of recidivism is downward-sloping), but could also indicate concern that recently released WCCs are less productive and have higher rates of absenteeism.

Firms are consistently most willing to hire WCCs with a substance-related conviction and least willing to hire WCCs with a violent conviction. In Panel A, with no subsidy, firms are 21 percentage points (350%) more willing to hire a WCC 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, firms are 41 percentage points (410%) more willing to hire WCCs 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 conviction history can have large effects on WCC hiring, at a level equal to or much greater than the effect of a 100% wage subsidy. This is broadly aligned with other research findings in this area. For example, Holzer et al. (2007) find that employers report being more willing to hiring 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 felony conviction on call-back rates (Agan and Starr, 2017).

4.4. Heterogeneity in Treatment Effects by Job Characteristics

Appendix Figures A.2 and A.3 explore heterogeneity in treatment effects across the same two job characteristics considered in Panels A and B of Figure 2, again following the motivating framework. Appendix Figure A.2 compares the treatment effects if we split the sample by whether the firms' jobs involve high-value inventory and Appendix Figure A.3 compares the treatment effects if we split the sample by whether the firms' jobs involve customer interaction.

Across the five panels of both figures, it is again clear that the firms with customer-facing jobs or high-value inventory are substantially less willing to hire WCCs. Panel A of Appendix Figure A.2 also shows that the impact of insurance on hiring is larger for high-value inventory firms, consistent with our conceptual framework. The $5m insurance, the most generous policy, boosts hiring by 10 percentage points in jobs without high-value inventory, but by twice as much (21 percentage points) in jobs with high-value inventory. We strongly reject that the elasticity of hiring with respect to the insurance level is the same across jobs with and without high-value inventory ($p < 0.001$). In our motivating framework, this prediction follows because insurance is more valuable to firms with a greater chance or cost from a loss. The impact of screening on performance history or years since conviction is similar across firms with and without high-value inventory. The same pattern holds for customer facing jobs.

5. Objective Performance Information and Correcting Misperceptions in Beliefs

In this section, we measure the effect of providing objective performance information about average WCC productivity on the labor demand for WCCs, again holding fixed the subsidy level established in the first part of the survey experiment. We are also able to measure the effect of correcting misperceptions in beliefs by exploiting the interaction of cross-firm variation in prior beliefs and our randomized information intervention.

5.1. Objective Performance Information

Panels A and B of Appendix Figure A.4 plot the distribution of beliefs about low performance (i.e., share of time the person gets one or two stars, or is a no show) and high performance (i.e., share of 5-star ratings), where both sets of prior beliefs are elicited using our incentive-compatible guessing game described in Section 2. Appendix Figure A.4 shows that while hiring managers hold a wide range of beliefs about WCCs' productivity, they tend to significantly underestimate the productivity of WCCs relative to non-WCCs. On average, hiring managers underestimate the likelihood of a WCC earning a 5-star rating by 12% and overestimate the likelihood of a low-performance rating by 14%.[8]

Figure 7 plots the distribution of posterior beliefs and the willingness to hire WCCs at each subsidy level following our randomized information intervention.

Panels A and B of Figure 7 show that information causes hiring managers to positively update their beliefs about WCC performance. Objective information about the 5-star ratings of WCCs causes hiring managers to raise their posterior beliefs about their rate of high-performance by 6.6 percentage points on average, equivalent to an increase of 0.35 standard deviations. Objective information about the share of low-ratings reduces hiring managers' posterior belief about the rate of low-performance by 5.8 percentage points on average, equivalent to a decrease of 0.25 standard deviations. We note that individuals in the control group, who are not shown information, still update their beliefs to some extent (XX percentage points boost in beliefs about high performance and XX percentage point drop about low performance), most likely because they receive an announcement from the Platform that some survey participants will be shown objective information, and this message in and of itself may signal that the information is positive. Our reduced form estimates of the binary impact of receiving objective information are therefore likely to underestimate the true impact of providing information. We return to this issue below when we use the interaction of cross-firm variation in prior beliefs and our randomized information intervention to estimate the unbiased effect of the relationship between beliefs and hiring demand.

Panel C of Figure 7 similarly show that providing information about high-performance increases hiring by 7.1 percentage points, equivalent to a rise of 0.14 standard deviations. We interpret the 7.1 percentage point treatment effect of high-performance information on hiring as economically substantial. The treatment effect is on par with that of providing $5,000 of insurance or of requiring WCCs to have successfully completed at least 1 prior job.

By comparison, Panel D of Figure 7 shows that providing information about low-performance only increases the share of employers willing to hire WCCs by 1.5 percentage points, and the increase is not statistically significant. The muted effect of low-performance rating, which primarily reflects no-shows, suggests that employers are less concerned with absenteeism when deciding

[8]The degree of misperception can also be driven by measurement error. For example, if some respondents mistyped their guess or randomly guessed a number between 0 and 100, the direction of the bias would exaggerate the extent to which participants underestimate WCC performance.

whether to hire WCCs and more concerned that WCCs might not satisfactorily meet their performance standards.

5.2. Correcting Misperceptions in Beliefs

We are also able to measure the effect of correcting misperceptions in beliefs by exploiting the degree to which employers update their beliefs, or learn about WCC performance. To see how we make use of employer learning, consider two employers who have the same bias about WCC performance by, e.g., underestimating the actual performance by 20%. One employer is randomized to receive objective information about the performance of WCCs and the other is not. We expect the employer who did receive the information to end up with higher expectations about WCC performance. More specifically, we expect the employer to update so that his or her posterior beliefs approximately match the objective information shown, causing a 20% shock to the treated employer's performance beliefs.[9] If this shock to performance beliefs raises the employers desire to hire WCCs by 10%, we measure a hiring elasticity of 0.50 with respect to beliefs about performance. Below we describe the specification and results.

Formally, we follow Cullen and Perez-Truglia (2018) and use the information shock, $(p_{signal,i} - p_{prior,i})$, interacted with the treatment indicator, T_i, as an instrument for hiring managers' posterior beliefs. 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.[10] $H_{prior,i}, H_{posterior,i} \in \{0, 1\}$ are the hiring manager's prior and posterior willingness to hire WCCs, respectively.

$$\begin{aligned} p_{posterior,i} &= \pi_0 + \pi_1(p_{signal,i} - p_{prior,i}) + \pi_2(p_{signal,i} - p_{prior,i}) * T_i + \eta H_{prior,i} + \xi_i \\ H_{posterior,i} &= \beta_0 + \beta_1 \hat{p}_{posterior,i} + \beta_2(p_{signal,i} - p_{prior,i}) + \gamma H_{prior,i} + \epsilon_i \end{aligned} \tag{5}$$

We estimate Equation (5) separately for respondents assigned to the high- and low-rating treatment arms.

Appendix Figure **??** provides a visual depiction of how participants in the low performance and high-performance group update their beliefs. In Panel A and C, the horizontal axis of the bin scatter plots the prior perception gap (truth minus prior belief), and the vertical axis plots the beliefs update (posterior minus prior belief). Observations along the 45 degree line indicate subjects that eliminated 100% of the error in their initial guess about WCC performance. Treated individuals, by and large, fall along this 45 degree line while participants shown no information

[9] 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, may include a typo in their response, all of which would appear in the data as incomplete updating.

[10] See the Appendix for a graphical depiction of the relationship between log prior perception gap and log posterior beliefs update, and hiring plotted against log posterior beliefs. This is also a common specification in the literature (e.g., Cullen and Perez-Truglia 2018; Fuster et al. 2018; Senik 2004.)

fall below the 45 degree line, indicating they only partially eliminate the error.[11] Column 1 in both panels of Appendix Table A.7 provides a regression estimate of the first-stage in both the high and low-performance groups. On average, treated participants close the gap between their prior and the truth by 34 percentage points and 46 percentage more than the control group, for high and low performance treatment groups respectively.

Column 3 in Appendix Table A.7 presents the main estimates of the effect of the information treatment on managers' willingness to hire WCCs. In Panel A, the estimated effect is roughly 0.81, meaning that a 10% increase in managers' beliefs about WCCs' performance leads to an 8.1 percentage point increase in willingness to hire.[12] This means that causing managers to update their beliefs by 10% has a similar effect as providing a 50% wage subsidy, and only a slightly smaller effect than providing firms with $5,000 of insurance or requiring WCCs to successfully have completed at least 1 prior job. In contrast, Panel B of Appendix Table A.7 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 WCC hiring decisions. Consistent with this interpretation, hiring managers also having more dispersed priors about low ratings and no-shows at baseline.

6. Robustness and External Validity

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

Social Desirability Bias. Another concern is that firms may feel a social desirability bias to express interest in hiring WCCs out of a desire to appear socially conscious. Several aspects of our study are ideally suited for addressing this concern. First, unlike past surveys, our study is based on firms making real incentive-compatible choices about whether they would like to have WCCs matched to their jobs and showing up to work. Recall that the only opportunity to screen out candidates once matched would be to cancel the job and pay half of agreed-to wages; moreover, the employer could not expect to know which matched worker had a past conviction and which did not, making it especially hard to reverse the decision in our survey. An employer that believes it is a poor choice to hire WCCs would go against his own economic interests by expressing a social desire to do so. Second, the experiment took place in an email from the Platform. Unlike studies where participants may do things to please the researchers, subjects did

[11] Individuals in the control group, who are not shown information, still update their beliefs to some extent because they are informed that some individuals will receive objective information, but we do not expect this to bias our estimates given our direct measures of posterior beliefs for all participants. We would not expect this to bias our results because we elicit posterior beliefs and posterior demand from the control group, effectively allowing us to net-out any information shock or time-passing effects experienced by the control group.

[12] 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 firms. It may also reflect inattention.

not know that there were researchers to potentially please.

Differential Attrition. One concern is that only 86% of hiring managers completed the entire survey experiment, and one might worry that employers who are less interested in hiring WCCs (or less affected by various policies) are less likely to complete the experiment and enter our estimation sample. To address how this may affect the level of demand, Appendix Figure B.3 presents our baseline labor demand curve under two extreme assumptions. The lower bound assumes that attriters never hire WCCs whereas the upper bound assumes that attriters always hire WCCs. The demand curve is similar at the upper or lower bound, suggesting that differential attrition does not affect the level of demand in our demand curve. To examine how differential attrition could affect non-wage treatment effects (and the level of demand), we exploit the fact that a sizable share of the attrition takes place later in the survey after most of the key treatment questions have been answered. If instead we merely require respondents to have completed the initial treatment questions, our results are qualitatively unchanged.

Hiring Authority. Another concern is that not all hiring managers would have the authority to make decisions about WCCs. As seen in our summary statistics, a large majority of managers report that they would have full authority or some authority to hire WCCs. Our results are qualitatively robust to restricting to these managers.

External Validity. We chose our setting because it was uniquely suited to estimate the demand for WCCs across many large traditional firms hiring for entry-level positions and flexible low-skill work. We expected this to offer a large and concentrated pool of appropriate jobs for WCCs re-entering the workplace, while at the same covering a wide range of roles across many industries, allowing us to observe role-specific barriers to hiring. When extrapolating to other settings, it is important to keep in mind that the labor contracts through the Platform are temporary and may thus lower the barrier to hiring WCCs. While anecdotally many convert to long-term positions, we do not directly observe this transition and cannot speak to the duration of employment, nor can we speak to the evolution of demand after employers gain experience working with WCCs. Our finding that the level of customer-interactions and presence of high-value inventory affect demand for WCCs 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 estimate the labor demand for WCCs. We find that 39% of firms are willing to hire WCCs without additional incentives, increasing by 2.1% for every 10% increase in the offered wage subsidy. The level of demand is higher for positions that do not involve customer interactions or high-value inventory, and is also higher higher if jobs are hard to fill. Crime and

safety insurance, performance screening, including only workers with a less serious conviction, and providing objective information on the productivity of WCCs all significantly increase the level of demand for WCCs.

These results are important because crime and safety insurance, performance screening, including only workers with a less serious conviction, and providing objective information on the productivity of WCCs can all likely be scaled up at a fraction of the cost of wage subsidies. While a comprehensive cost analysis is beyond the scope of this paper, we consider a back-of-the-envelope calculation that takes into account the direct costs of increasing demand for WCCs for each of our main treatments in Appendix C. These calculations reveal that all of these policies can significantly increase demand for WCCs at a fraction of the cost of wage subsidies. Performance screening, for example, can achieve notable gains in the share of firms willing to hire WCCs at near-zero cost because a large number of employers are willing to hire and provide WCCs with their first performance review, opening up the door to employers that require that first review. Providing objective information on the average productivity of WCCs can similarly increase the share of firms hiring WCCs at essentially zero additional cost to the Platform. Scaling back the background screening to exclude only those candidates with the most severe crimes requires no new costs for the Platform. Crime and safety insurance can increase the demand for WCCs at one-tenth or less the cost of wage subsidies under realistic assumptions of the probability of damages due to WCC misbehavior. These calculations suggest that all of the options we consider are substantially more cost-effective than wage subsidies, at least in this context.

Cost-effective alternative policies to the criminal background check that directly address employers' concerns are especially important in light of evidence that simply prohibiting firms from asking about an individual's criminal history ("Ban the Box") may increase hiring discrimination against Black men (Bushway, 2004; Holzer et al., 2006; Stoll, 2009; Shoag and Veuger, 2016; Agan and Starr, 2018; Doleac and Hansen, 2020). Based on our findings, the Platform modified its user-interface so that thousands of employers posting jobs could have the option of hiring WCCs. The Platform also currently provides the modest level of crime and safety insurance that our study finds boosts demand for WCCs in a cost-effective manner, and has decided to exclude WCCs with violent or sexual felonies, consistent with our findings that concern over left-tail risk (and not just productivity conccerns) is an important underlying reason for requesting a criminal background screening, even for short-term jobs. The platform-wide policy opens new questions for future research, including the evolution of demand for WCCs as employers gain experience working with WCCs, and long-term employment opportunities created for workers that are matched as a result of this policy change.

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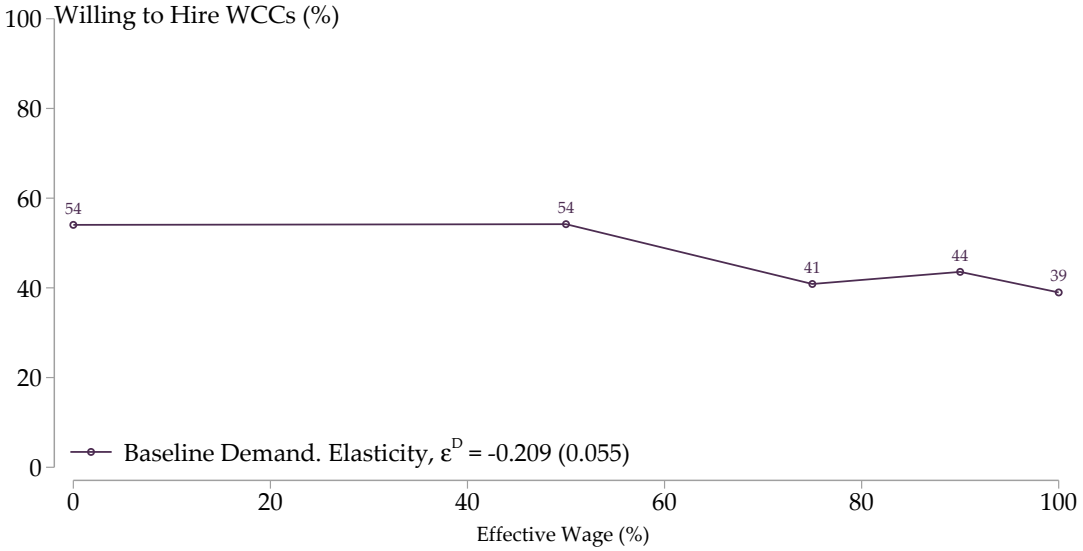
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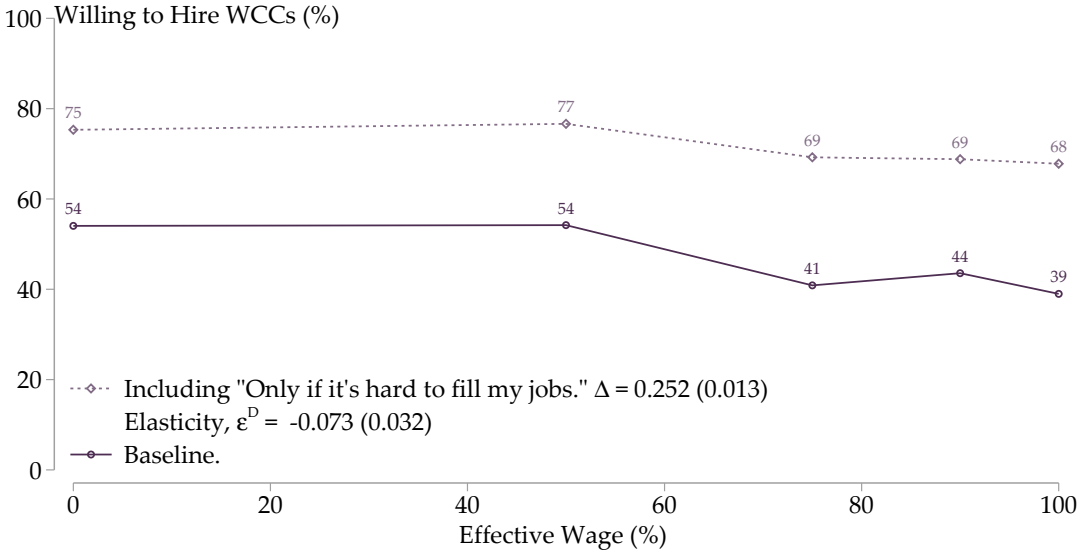
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Figure 1: Labor Demand for Workers with a Criminal Conviction

A. Baseline Definition of "Willing to Hire"

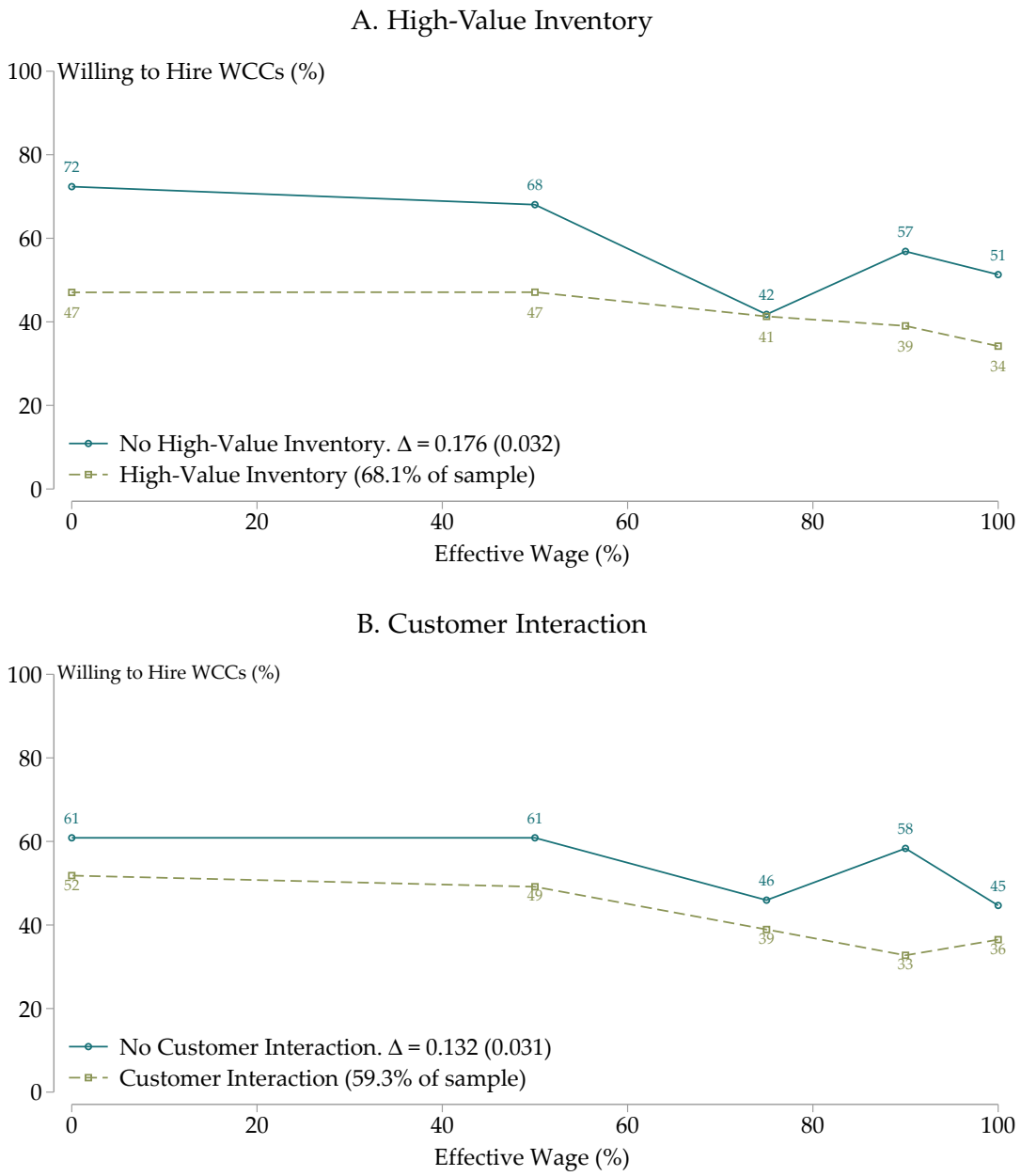


B. Including "Only If It's Hard to Fill My Jobs"



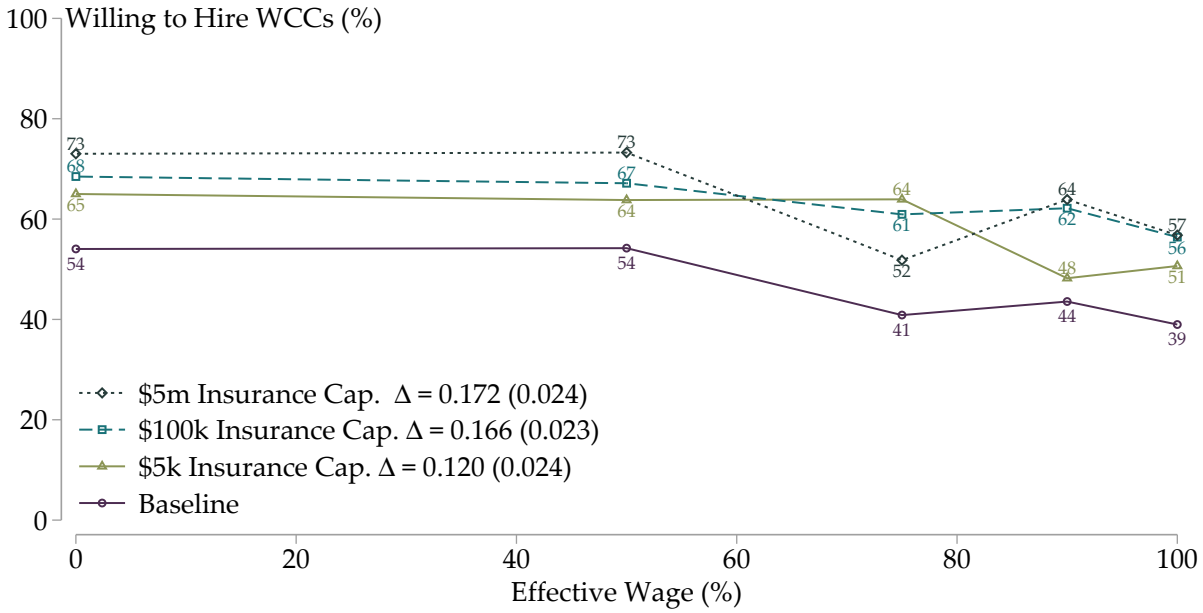
Notes. This figure plots mean willingness to hire workers with a criminal conviction against the randomized effective wage. The effective wage is calculated as the share of the wage remaining after the subsidy is applied, or $100 - \texttt{subsidy rate}$. Respondents are asked if they are willing to hire a WCC 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 hire WCCs. This is our baseline definition of willingness to hire 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 hire WCCs. 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 firms. 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



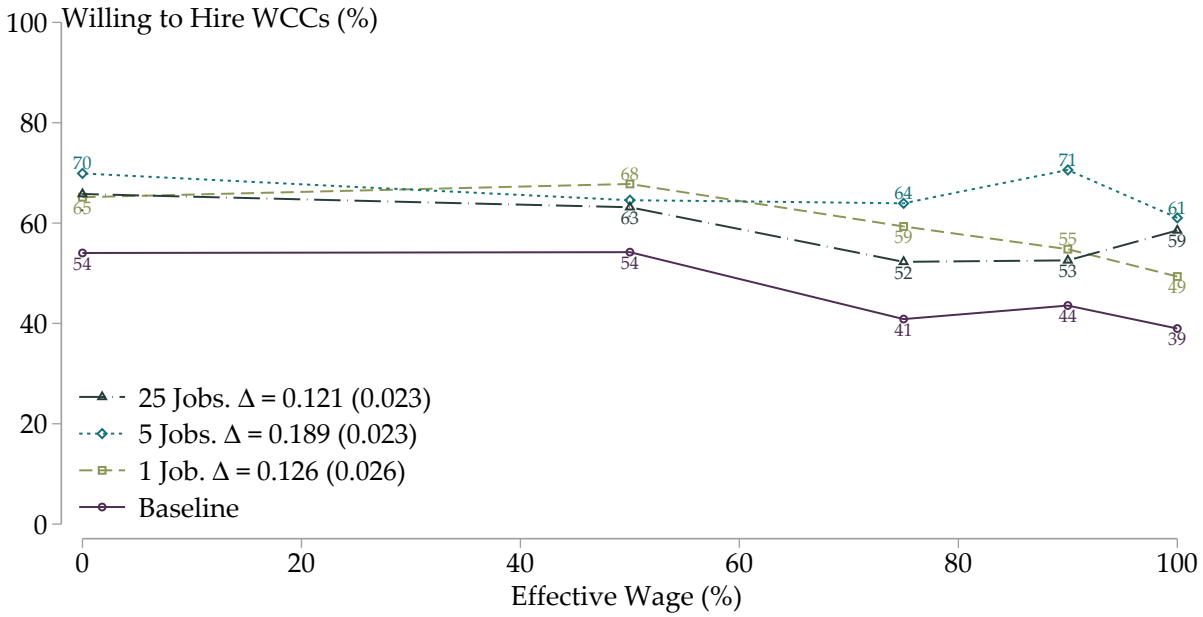
Notes. This figure plots mean willingness to hire workers with a criminal conviction against the randomized effective wage. Panel A reports results separately for firms who report that their jobs do or do not involve high-value inventory. Panel B reports results separately for firms who report that their 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 firms. 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



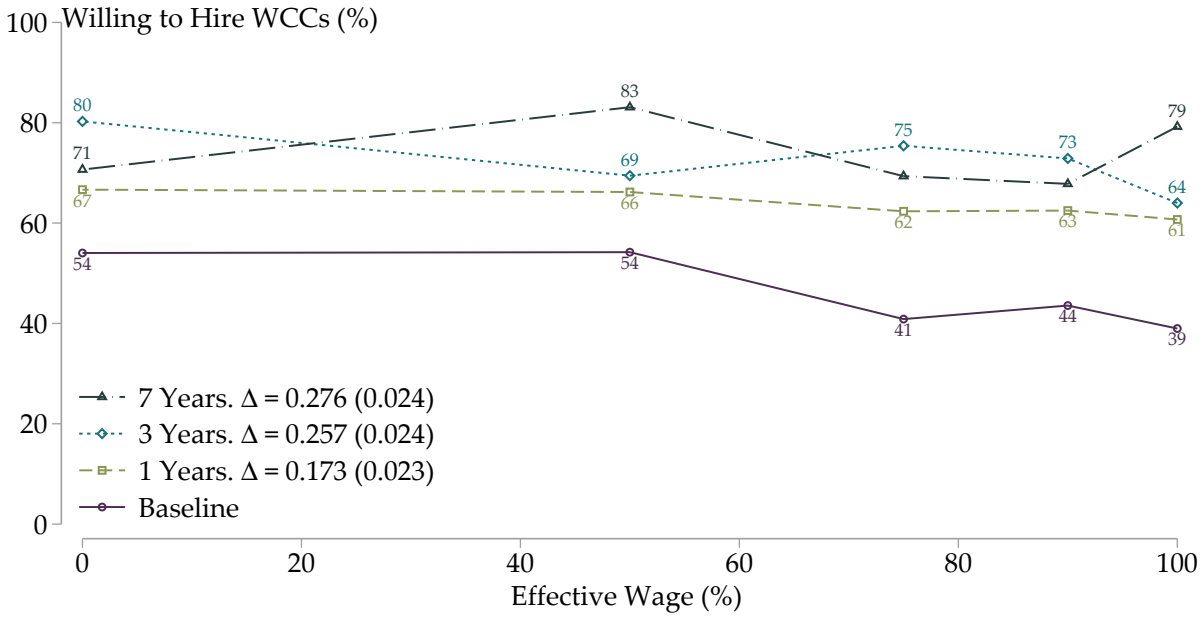
Notes. This figure plots mean willingness to hire workers with a criminal conviction against the randomized effective wage. The dotted baseline curve displays baseline hiring rates shown in Panel A of Figure 1. The three upper curves display the effect of the Platform providing a crime and safety insurance 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 firms. Precise point estimates are reported in columns 2-4 of Table A.6.

Figure 4: Screening Based on Performance History



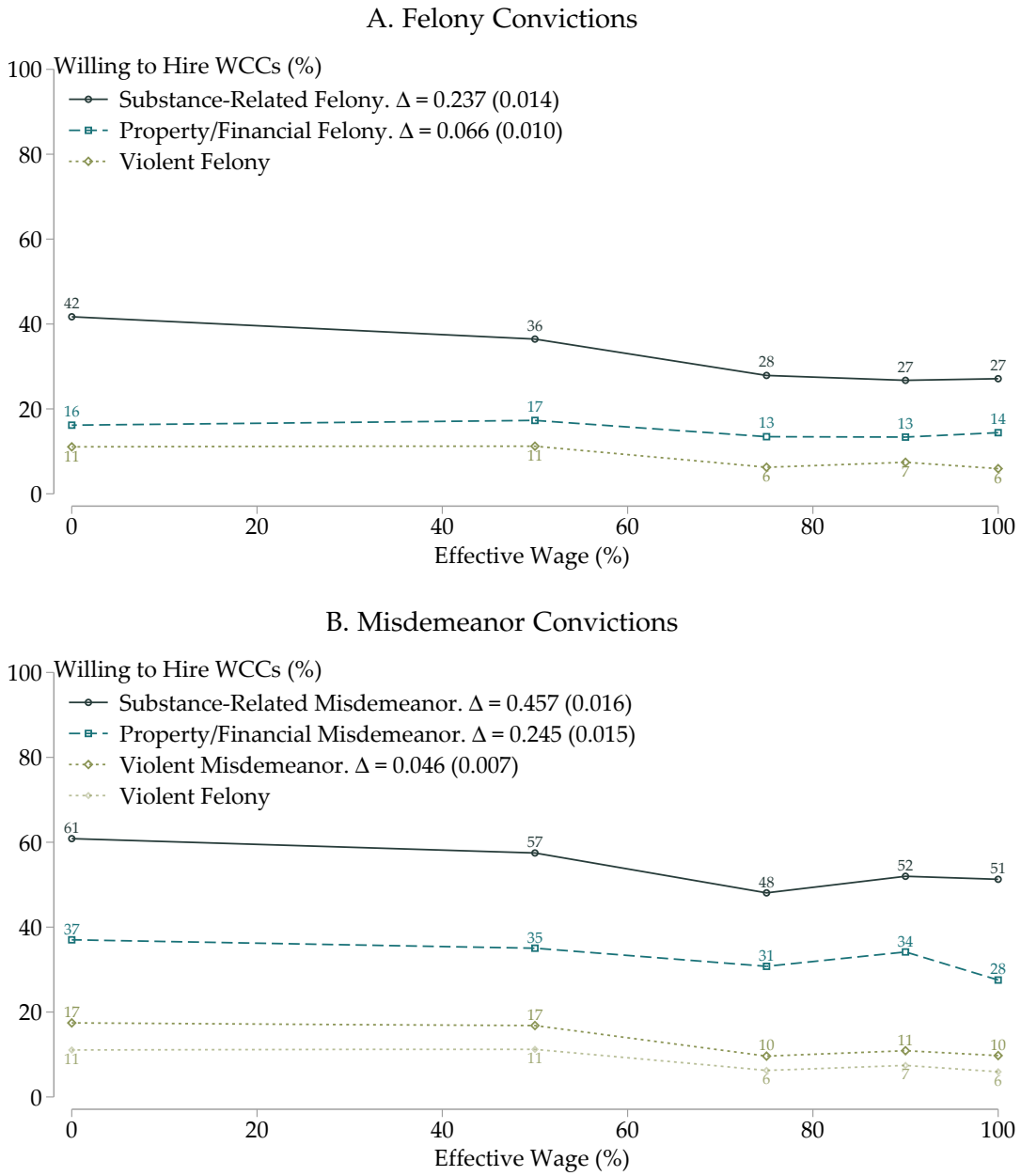
Notes. This figure plots mean willingness to hire workers with a criminal conviction against the randomized effective wage. The dotted baseline curve displays baseline hiring rates shown in Panel A of Figure 1. The three upper curves display the effect of the individual having satisfactorily completed 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 firms. Precise point estimates are reported in columns 2-4 of Table A.6.

Figure 5: Screening Based on Years Since Conviction



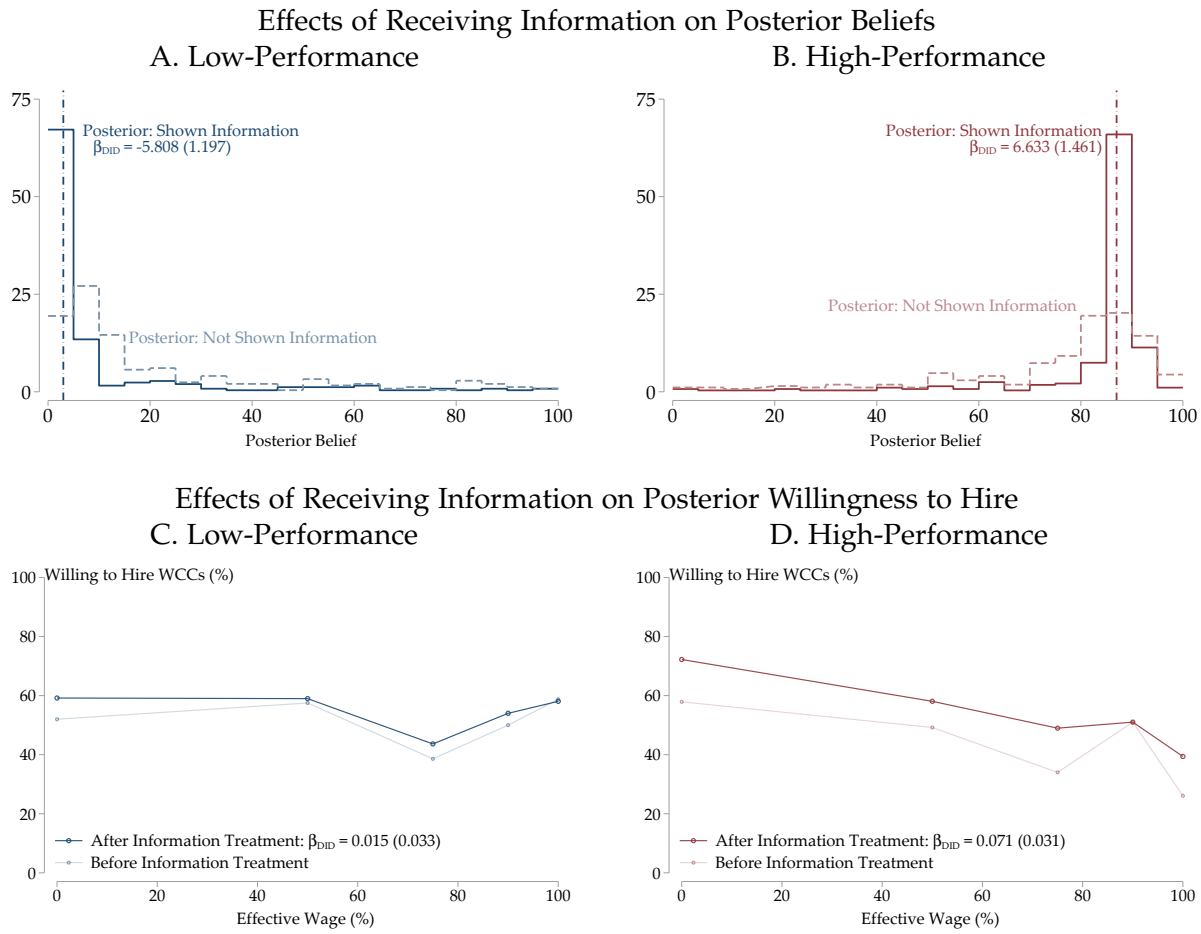
Notes. This figure plots mean willingness to hire workers with a criminal conviction against the randomized effective wage. The dotted baseline curve displays baseline hiring rates shown in Panel A of Figure 1. The three upper curves display the effect of it having been one, three, or seven years since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

Figure 6: Screening Based on Crime Type



Notes. This figure plots mean willingness to hire workers with a criminal conviction against the randomized effective wage, given the specific crime type and severity. Panel A plots willingness to hire WCCs who have a substance-related felony, a property/financial related felony or a violent felony. Panel B plots willingness to hire WCCs 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 firms. 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 Table A.6.

Figure 7: Objective Performance Information



Notes. Panels A and B report the posterior belief distributions about WCC performance for respondents who were shown objective information about WCC performance and for the control group. Panels C and D report the baseline demand for WCCs and the demand after receiving the information treatment only for the respondents who were shown objective information about WCC productivity. The estimates in Panels A and B are based on the experimental sample described in Table 2. This includes 1,095 managers from 913 firms. The estimates in Panels C and D include the subset of XXX managers from XXX firms 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
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
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

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	31.1	23.6
Employees	3,093	21.3
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. WCC Hiring Policies	Experimental Sample	SHRM Survey
Firm-Wide WCC Hiring Policy	0.45	0.66
WCC Positives: Best Candidate	0.46	0.53
WCC Positives: Second Chance	0.50	0.38
WCC Positives: Financial Incentives	0.08	0.02
WCC Negatives: Customer Reaction	0.49	0.30
WCC Negatives: Regulation	0.26	0.22
WCC Negatives: Performance	0.15	0.04
Firms	900	1,228

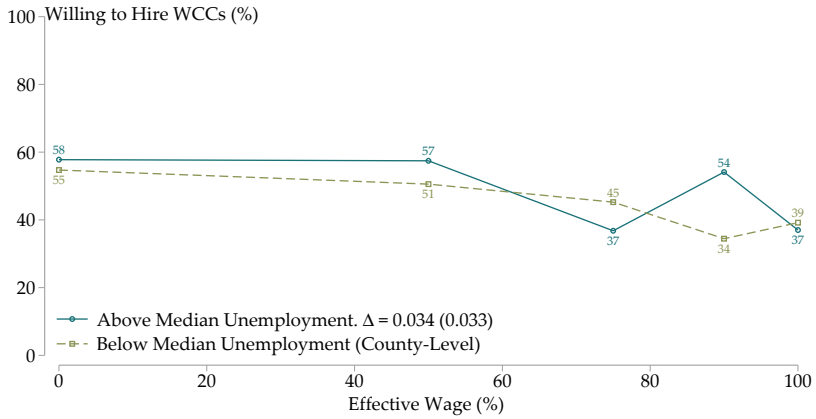
C. Manager Characteristics	Experimental Sample
Years of Experience (mean)	7.33
Share of Mangers in HR	0.14
Share with Full Authority to Hire WCCs	0.53
Share for whom Opinion on Hiring WCCs Would Matter, but not Final Decision-Maker	0.27
Share without Authority to Hire WCCs	0.20
Managers	1,095

Notes. This table reports descriptive statistics for the experimental sample. Panel A reports statistics for the 666 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 518 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).

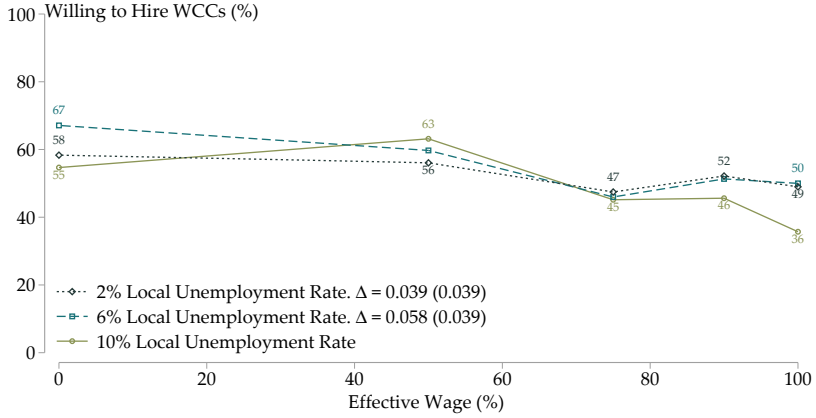
Appendix A. Additional Results

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

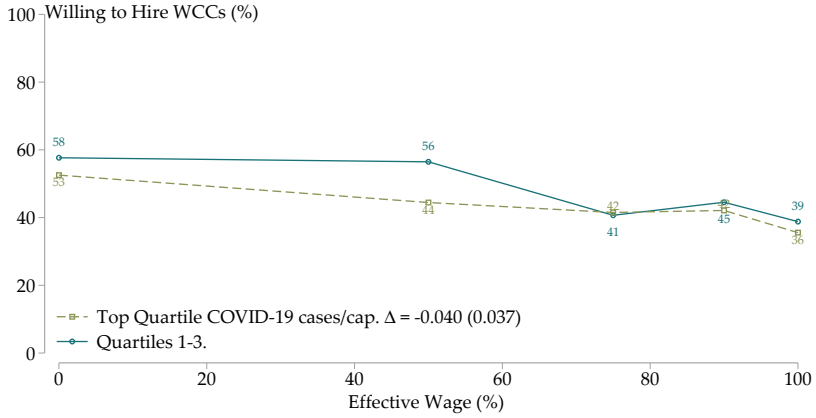
A.County-Level Unemployment Rate



B. Potential Local Unemployment Rate

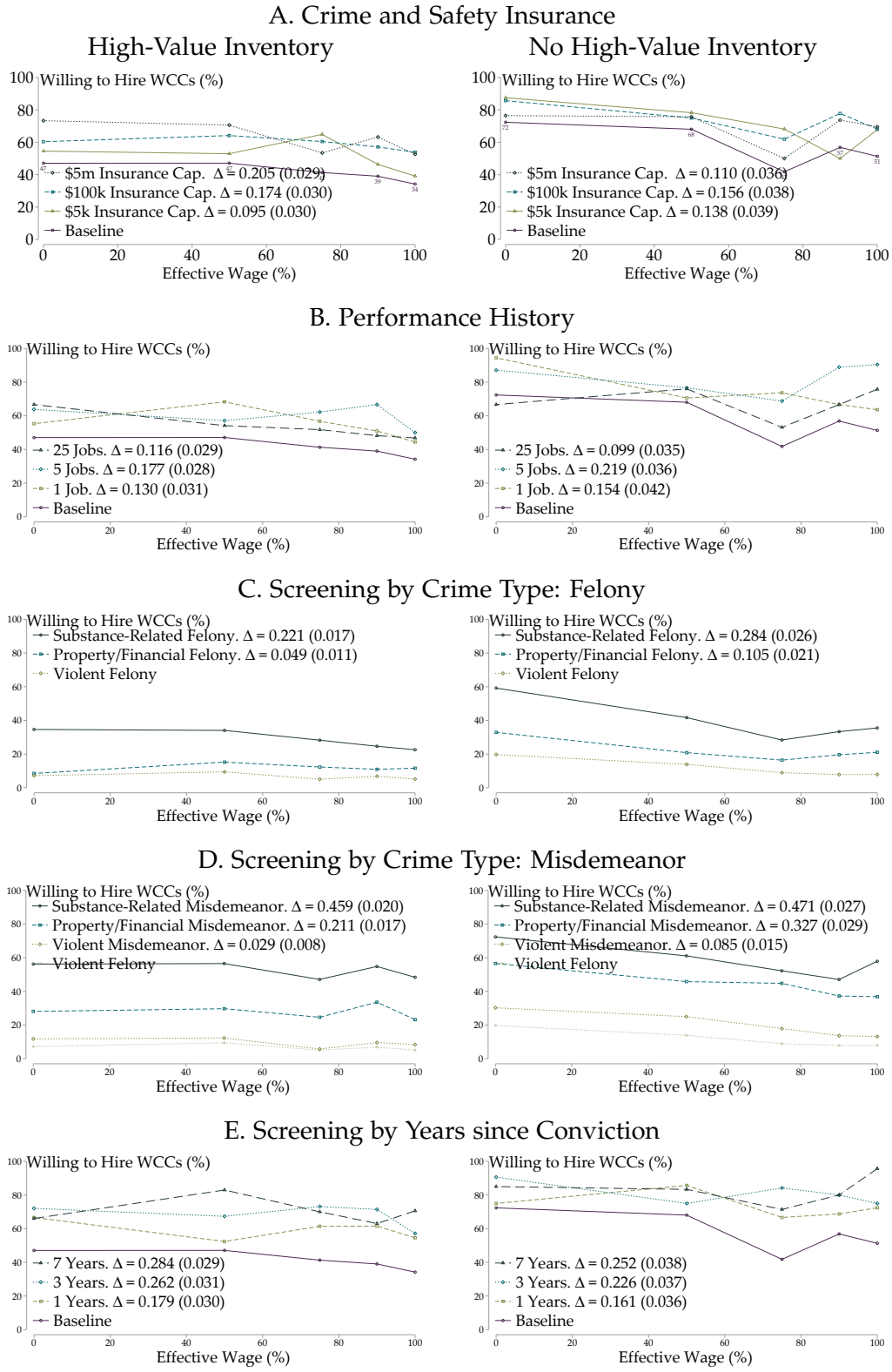


C. County-Level COVID-19 cases per capita



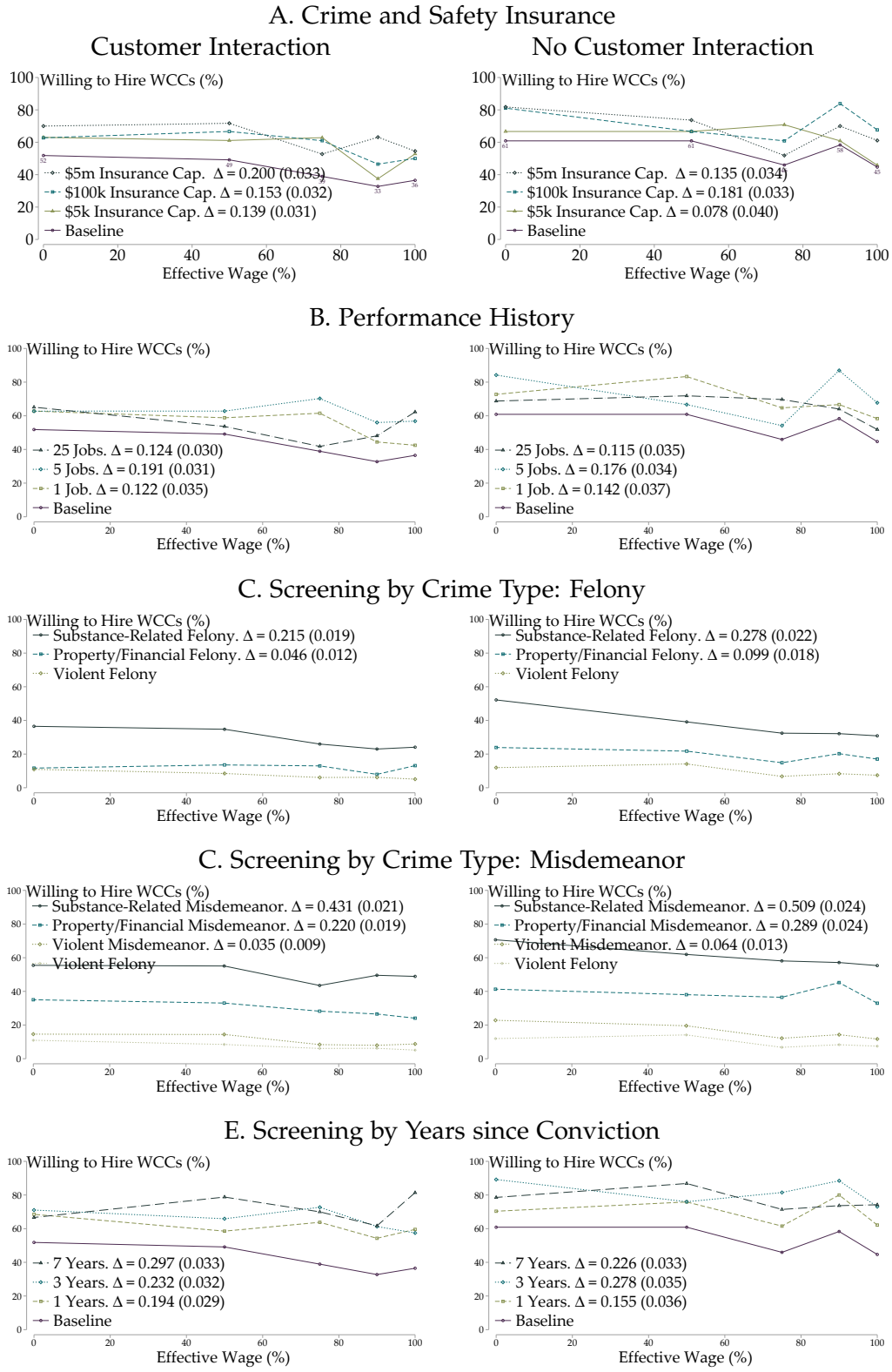
Notes. This figure plots mean willingness to hire WCCs against the randomized effective wage. Panel A splits respondents into above-median and below-median groups based on March 2020 unemployment rates in the firm's county. The Δ in Panel A represents the average difference between the two curves. Panel B reports willingness to hire 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 survey was distributed. The solid line represents firms whose county was in the top quartile of COVID-19 rates, and the dotted line represents firms 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 firms. 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



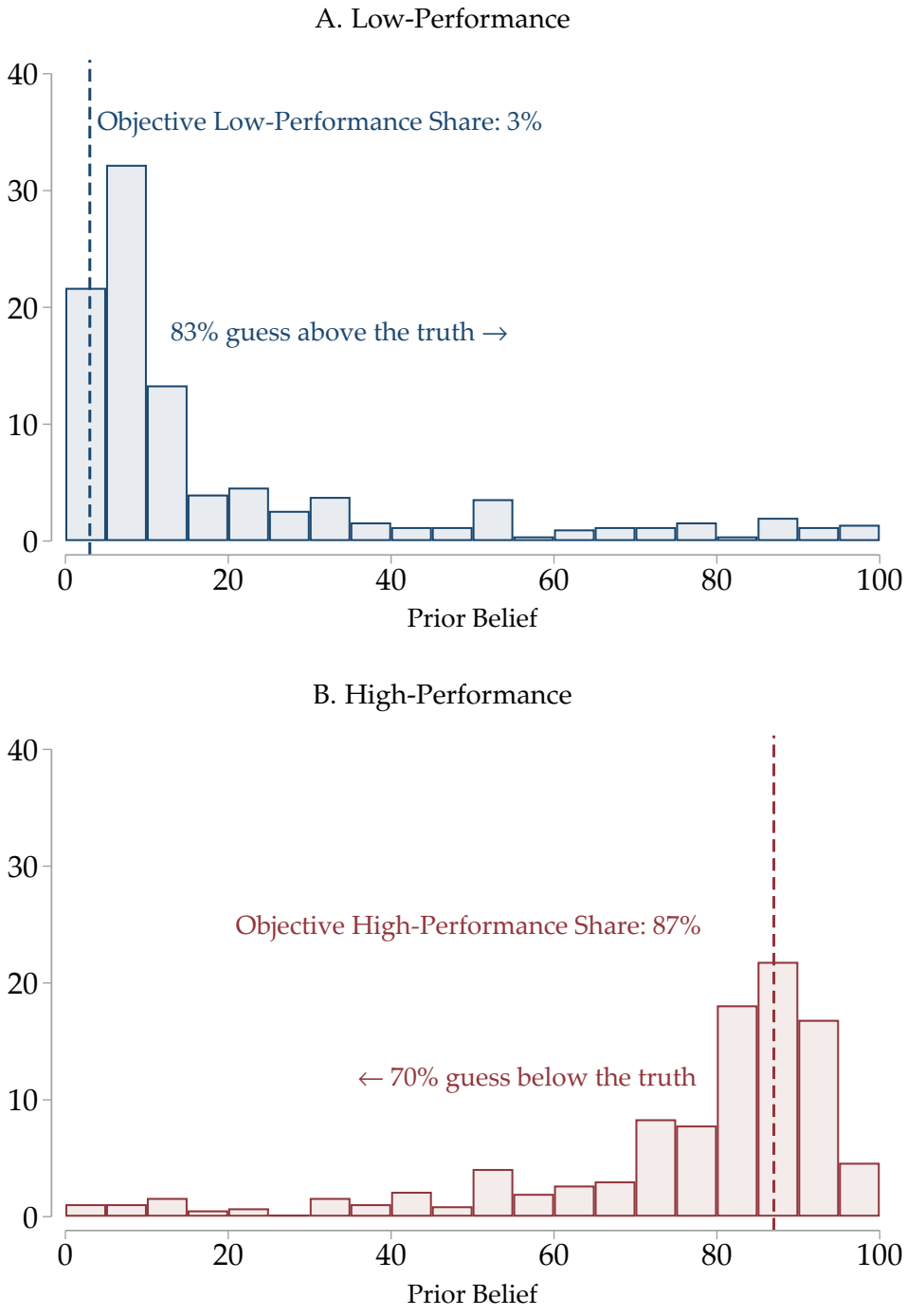
Notes. This figure plots the differential effects of several policies on mean willingness to hire workers with a criminal conviction against the randomized effective wage, split by whether firms report that their jobs involve customer interaction. The left-hand graph in each panel presents mean willingness to hire 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 hire 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



Notes. This figure plots the differential effects of several policies on mean willingness to hire workers with a criminal conviction against the randomized effective wage, split by whether firms report that their jobs involve high-value inventory. The left-hand graph in each panel presents mean willingness to hire 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 hire 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 WCC Productivity



Notes. This figure reports the prior distribution of firm beliefs about WCC productivity. Panel A reports the distribution of prior beliefs about the share of WCCs 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 WCCs who receive low performance ratings. Panel B reports the distribution of prior beliefs about the share of WCCs who receive high performance ratings (5-star ratings). The dotted vertical line indicates the the true, objective share of WCCs 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 firms.

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?
Local 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?
State of Emergency	We can split responses based on whether they were submitted before or after the President declared a National Emergency concerning the novel coronavirus disease (COVID-19) outbreak on March 13, 2020.
High/Low COVID Counties	We have county-level coronavirus data available, and can split counties into population-weighted quartiles, by county-level cases per capita. To identify counties in which the outbreak is most severe, we distinguish between counties in the top quartile and the bottom three quartiles. We exclude the counties that make up New York City.
Prior Beliefs	We elicit beliefs regarding the manager's prior beliefs regarding either the share of WCCs who recieve a 4-star or 5-star rating ("High-Performance") or the share of WCCs who receive a 1-star or 2-star rating or are a no-show ("Low-Performance").

Notes. This table summarizes the main job and firm characteristics used in our analysis. 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	0.605	0.419
Share Retail	0.865	0.795	0.320	0.376	0.857	0.311
Share Service	0.058	0.330	0.313	0.632	0.344	0.632
Share Wholesale Trade	0.944	0.875	0.957	0.658	0.651	0.325
Share Nonclassifiable	0.504	0.907	0.366	0.072	0.633	0.385
Share Transportation & Public Utilities	0.272	0.768	0.425	0.557	0.890	0.243
Firm-Wide WCC Hiring Policy	0.731	0.645	0.750	0.537	0.435	0.279
Platform Tenure (Years)	0.090	0.081	0.615	0.563	0.212	0.912
Job Vacancy Rate	0.729	0.832	0.301	0.463	0.825	0.922
B. Manager Characteristics						
Manager Works in Human Resources	0.928	0.928	0.826	0.890	0.329	0.824
Manager Years of Experience	0.648	0.379	0.475	0.782	0.828	0.315
C. Platform Characteristics						
Job Involves Customer Interactions	0.740	0.524	0.455	0.356	0.561	0.663
Job Involves High-Value Inventory	0.324	0.421	0.261	0.531	0.422	0.413
D. Modal Job Category						
Fulfillment / Warehousing	0.599	0.795	0.562	0.338	0.339	0.280
General Labor	0.005	0.096	0.728	0.654	0.321	0.143
Event Staff	0.424	0.933	0.574	0.420	0.762	0.691
Delivery	0.799	0.063	0.426	0.917	0.336	0.921
Washing & Cleaning	0.569	0.907	0.779	0.123	0.358	0.923
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 Conviction

	Baseline	Including "If Hard to Hire"	Customer Interactions	No Customer Interactions	High-Value Inventory	No High-Value Inventory
No Subsidy	0.390	0.678	0.365	0.447	0.342	0.513
	(0.032)	(0.032)	(0.042)	(0.050)	(0.038)	(0.055)
10% Subsidy†	0.436	0.688	0.327	0.583	0.390	0.569
	(0.037)	(0.033)	(0.047)	(0.055)	(0.044)	(0.070)
25% Subsidy	0.409	0.692	0.389	0.459	0.413	0.418
	(0.037)	(0.035)	(0.047)	(0.058)	(0.048)	(0.057)
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	-0.209	-0.073	-0.247	-0.179	-0.160	-0.322
	(0.055)	(0.032)	(0.073)	(0.084)	(0.066)	(0.093)
Mean Effect vs. Baseline	–	0.252	-0.044	0.080	-0.047	0.121
		(0.013)	(0.021)	(0.024)	(0.019)	(0.026)
Mean Effect vs. Omitted Group	–	–	–	0.132	–	0.176
				(0.031)		(0.032)
Firms	913	913	533	392	614	319
Managers	1,095	1,095	636	436	730	342

Notes. This table reports estimates of the effects of wage subsidies on firms' willingness to hire workers with a criminal conviction. 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 hire WCCs 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†	-0.359	-0.756	-1.054
	(0.094)	(0.341)	
25% Subsidy	-0.305	-0.554	0.352
	(0.080)	(0.221)	
50% Subsidy	-0.205	-0.275	-0.702
	(0.054)	(0.078)	
100% Subsidy	-0.072	-0.031	0.002
	(0.019)	(0.039)	
Average Elasticity	-0.209	-0.276	-0.162
	(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 hire 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 hire, respectively. For example, in the 10% subsidy row, w is the midpoint between a 100% and 90% effective wage. Column 2 reports quadratric estimates, again calculated as $\frac{dH}{dW}\frac{w}{H}$, where $\frac{dH}{dW}$ represents the marginal effect from a regression of willingness to hire 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 hire 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 hire on effective wage, and $\bar{w}$ and $\bar{H}$ represent mean effective wage and willingness to hire 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: Labor Demand for WCCs by Local Unemployment Rate

		Willing to Hire: Baseline		
	Baseline	10%	6%	2%
No Subsidy	0.447	-0.033	0.110	0.100
	(0.050)	(0.047)	(0.059)	(0.044)
10% Subsidy†	0.583	0.066	0.123	0.132
	(0.055)	(0.082)	(0.066)	(0.073)
25% Subsidy	0.459	0.062	0.070	0.085
	(0.058)	(0.076)	(0.067)	(0.074)
50% Subsidy	0.609	0.242	0.207	0.171
	(0.052)	(0.064)	(0.068)	(0.076)
100% Subsidy	0.609	0.157	0.281	0.194
	(0.049)	(0.066)	(0.063)	(0.060)
Elasticity	-0.209	-0.230	-0.244	-0.119
	(0.055)	(0.094)	(0.092)	(0.083)
Mean Effect vs. Baseline	–	0.026	0.084	0.065
		(0.024)	(0.023)	(0.023)
Mean Effect vs. Omitted Group	–	–	0.058	0.039
			(0.039)	(0.039)
Firms	913	304	306	317
Managers	1,095	340	379	376

Notes. This table reports OLS estimates of the effects of potential local employment rates on firms' willingness to hire workers with a criminal conviction. Column 1 reports the baseline fraction of managers choosing to hire WCCs at each subsidy level. Columns 2-4 report the additional effect of potential local unemployment rates. 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. 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%) 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.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	0.390	0.117	0.174	0.178	0.104	0.221	0.196
	(0.032)	(0.049)	(0.052)	(0.056)	(0.052)	(0.051)	(0.051)
10% Subsidy†	0.436	0.046	0.186	0.203	0.112	0.270	0.090
	(0.037)	(0.061)	(0.057)	(0.053)	(0.054)	(0.062)	(0.053)
25% Subsidy	0.409	0.231	0.201	0.109	0.185	0.231	0.114
	(0.037)	(0.057)	(0.054)	(0.051)	(0.066)	(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	-0.209	-0.169	-0.122	-0.209	-0.179	-0.068	-0.136
	(0.055)	(0.076)	(0.076)	(0.082)	(0.089)	(0.077)	(0.082)
Mean Effect	–	0.120	0.166	0.172	0.126	0.189	0.121
vs. Baseline		(0.024)	(0.023)	(0.024)	(0.026)	(0.023)	(0.023)
		[0.038]	[0.010]	[0.010]	[0.038]	[0.002]	[0.037]
Mean Effect	–	–	0.045	0.052	–	0.063	-0.005
vs. Omitted Group			(0.036)	(0.037)		(0.038)	(0.038)
		–	[0.455]	[0.455]	–	[0.455]	[0.886]
Firms	913	292	324	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 Conviction			Felony Type			Misdemeanor Type		
	Baseline	1 Year	3 Years	7 Years	Violent	Property	Drug	Violent	Property	Drug
No Subsidy	0.390	0.217	0.250	0.402	-0.331	-0.246	-0.119	-0.292	-0.114	0.123
	(0.032)	(0.049)	(0.054)	(0.047)	(0.032)	(0.030)	(0.034)	(0.031)	(0.033)	(0.032)
10% Subsidy†	0.436	0.189	0.293	0.243	-0.361	-0.302	-0.168	-0.327	-0.094	0.084
	(0.037)	(0.060)	(0.058)	(0.058)	(0.035)	(0.035)	(0.035)	(0.035)	(0.039)	(0.038)
25% Subsidy	0.409	0.215	0.345	0.285	-0.346	-0.274	-0.130	-0.313	-0.101	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	-0.209	-0.053	-0.105	0.008	-0.117	-0.061	-0.349	-0.183	-0.165	-0.236
	(0.055)	(0.060)	(0.058)	(0.056)	(0.052)	(0.067)	(0.088)	(0.064)	(0.087)	(0.090)
Mean Effect	–	0.173	0.257	0.276	-0.380	-0.314	-0.142	-0.334	-0.135	0.077
vs. Baseline		(0.023)	(0.024)	(0.024)	(0.015)	(0.016)	(0.015)	(0.015)	(0.017)	(0.015)
		[0.006]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.024]
Mean Effect	–	–	0.086	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)
		–	[0.167]	[0.142]	–	[0.006]	[0.000]	[0.006]	[0.000]	[0.000]
Firms	913	314	300	318	913	913	913	913	913	913
Managers	1,095	380	343	372	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 hire workers with a criminal conviction. In Panel A, Column 1 reports the baseline fraction of managers choosing to hire WCCs 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 WCCs satisfactorily complete the indicated number of jobs. In Panel B, Column 1 again reports the baseline willingness to hire WCCs. Columns 2-4 report the additional effect of imposing a minimum time since conviction before allowing WCCs to join the pool of workers. Columns 5-10 report the additional effect of restricting WCCs 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 Table A.7: High- and Low-Performance Information

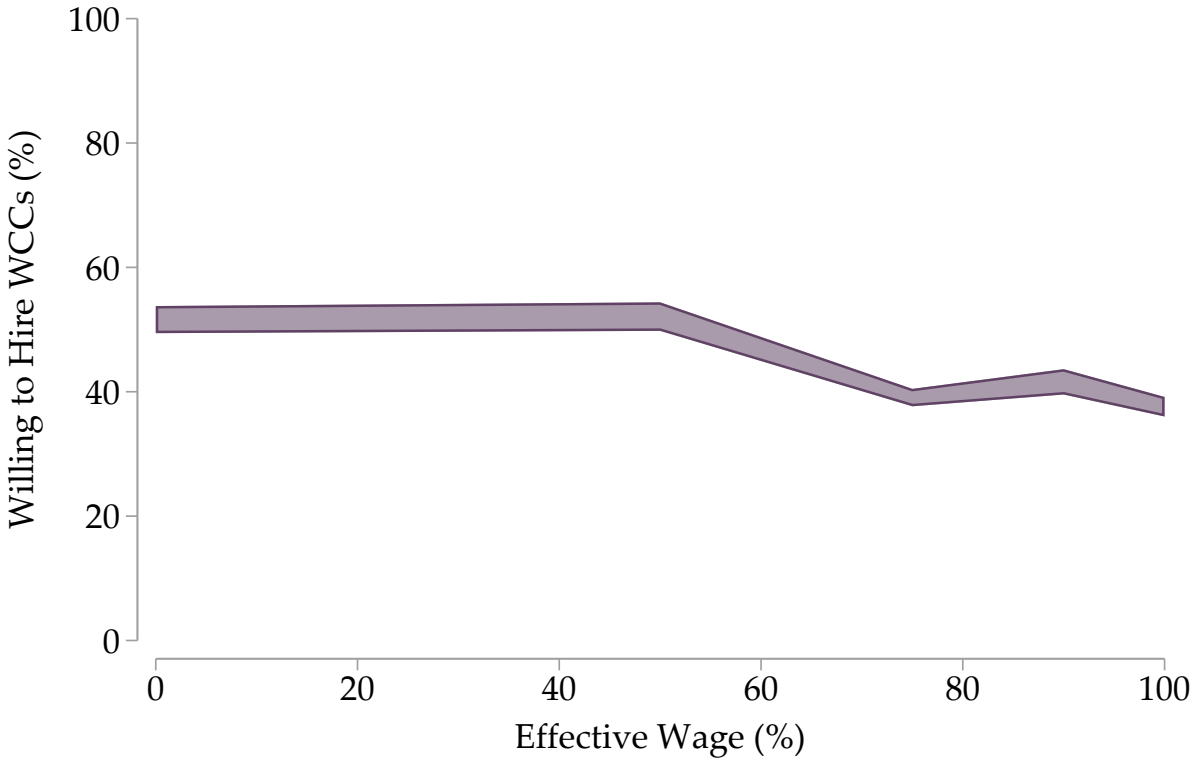
A. Impact of High-Performance Information	First Stage	OLS	IV	Reduced Form
Shown Info × (Signal - Prior Belief)	0.337			0.273
	(0.0806)			(0.131)
ln(Posterior Belief)		0.338	0.811	
		(0.108)	(0.410)	
Mean: Dependent Variable	4.36	0.51	0.51	0.51
Kleibergen-Paap: Weak Identification F-Stat			17.50	
Firms	491	491	491	491
Managers	559	559	559	559

B. Impact of Low-Performance Information	First Stage	OLS	IV	Reduced Form
Shown Info × (Signal - Prior Belief)	0.451			0.0310
	(0.0475)			(0.0328)
ln(Posterior Belief)		-0.0336	0.0687	
		(0.0295)	(0.0736)	
Mean: Dependent Variable	1.68	0.52	0.52	0.52
Kleibergen-Paap: Weak Identification F-Stat			90.15	
Firms	429	429	429	429
Managers	501	501	501	501

Notes. This table reports estimates of the impact of high- and low-performance information on firm beliefs and willingness to hire. Panel A reports results for managers who were shown information on the fraction of 5-star ratings. Panel B reports results for managers who were shown information on the fraction of no-shows and either 1- or 2-star ratings. Column 1 reports first stage estimates of the effect of information on posterior beliefs. Column 2 reports OLS estimates of the cross-sectional relationship between posterior beliefs and hiring decisions. Column 3 reports IV estimates of the causal impact of a change in posterior beliefs on hiring decisions. Column 4 reports reduced form estimates of the effect of information on hiring decisions. See Section 5 of the text for additional details.

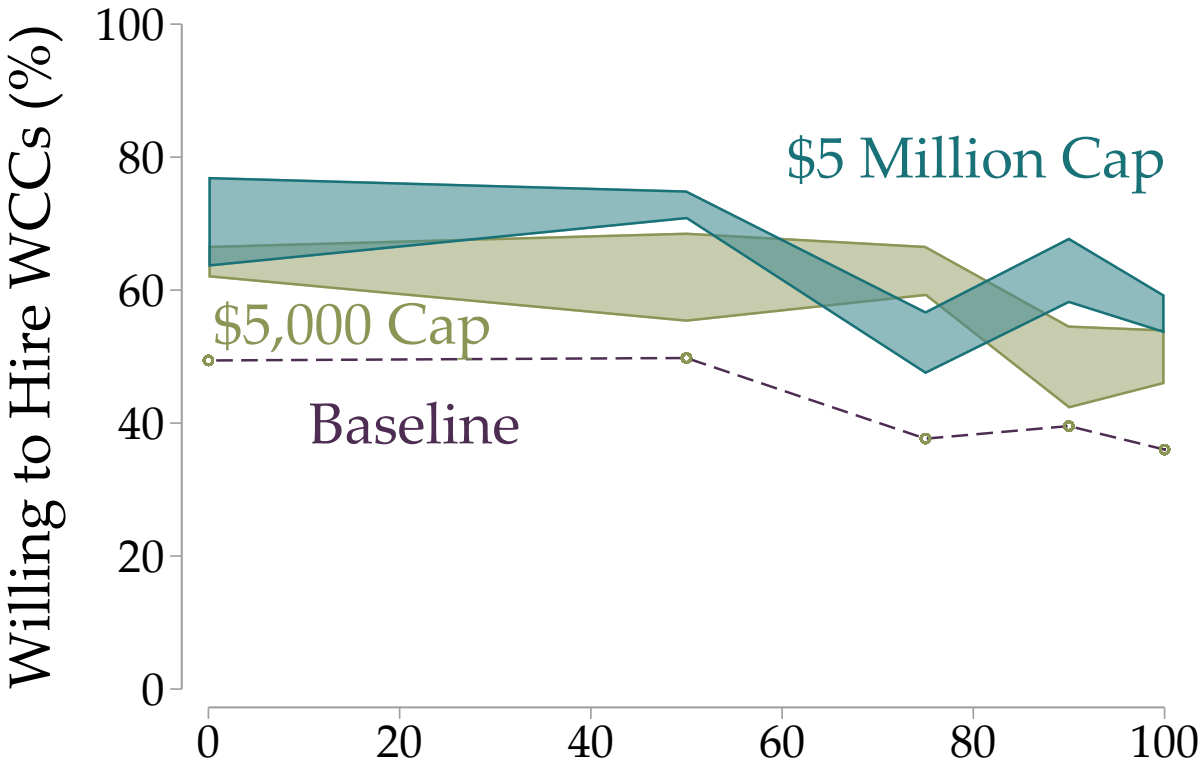
Appendix B. Attrition Robustness Check

Appendix Figure B.1: Bounds on Demand for Workers



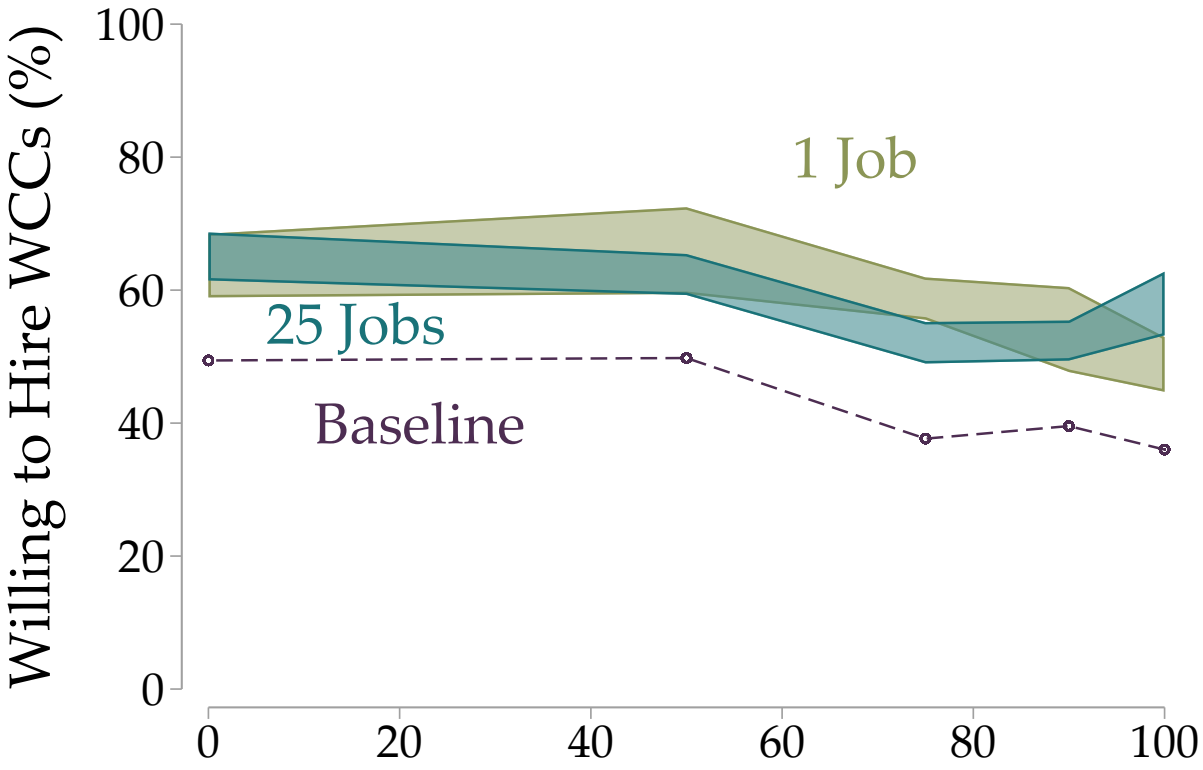
Notes. This figure plots mean willingness to hire WCCs against the randomized effective wage. The upper bound is estimated by assuming that respondents who do not answer would hire WCCs at any subsidy level. The lower bound is estimated by assuming that they would not hire WCCs at any subsidy level.

Appendix Figure B.2: Bounds on the Effect of Insurance on Demand for Workers



Notes. This figure plots mean willingness to hire WCCs against the randomized effective wage. The upper bound is estimated by assuming that respondents who do not answer would hire WCCs at any subsidy level. The lower bound is estimated by assuming that they would not hire WCCs at any subsidy level.

Appendix Figure B.3: Bounds on the Effect of Performance History on Demand for Workers



Notes. This figure plots mean willingness to hire WCCs against the randomized effective wage. The upper bound is estimated by assuming that respondents who do not answer would hire WCCs at any subsidy level. The lower bound is estimated by assuming that they would not hire WCCs at any subsidy level.

Appendix Table B.1: Survey Completion and Attrition

	Respondents	Unique Firms
All Responses (Through May 3, 2020)	1,306	1,062
First Question: Role on Platform	1,256	1,015
How Many Years of HR Experience	1,198	958
Would You Have the Authority to Hire a WCC†	1,175	941
Baseline Willingness to Hire with Subsidy	1,114	926
Willingness to Hire with Insurance	1,109	923
Willingness to Hire if Restricted to *type of crime*	1,095	913
What are Existing HR Hiring Policies and/or Practices	1,075	900
Additional Job Information (Customer Facing and High-Value Inventory)	1,072	898
Willingness to Hire with a Subsidy (Information Treatment)	1,060	889
Subjective Expectations of WCC Performance	1,054	885
Complete and Submit Survey	1,044	875

Notes. † first mention of WCCs in Survey.

Appendix Table B.2: Baseline Demand, Adjusting for Attrition

Respondents who don't answer	Don't Hire	Do Hire
No Subsidy	0.360	0.441
	(0.030)	(0.031)
10% Subsidy†	0.396	0.489
	(0.034)	(0.036)
25% Subsidy	0.377	0.446
	(0.035)	(0.038)
50% Subsidy	0.498	0.582
	(0.034)	(0.034)
100% Subsidy	0.494	0.575
	(0.031)	(0.030)
Elasticity	-0.207	-0.206
	(0.057)	(0.055)
Firms	1,015	1,015
Managers	1,213	1,213

Appendix Table B.3: Crime and Safety Insurance, Bounds on Treatment Effects

A. Policy Treatment	Crime and Safety Insurance					
	$5k†		$100k		$5m	
	Assume respondents who don't finish		Assume respondents who don't finish		Assume respondents who don't finish	
	Never Hire	Only Hire with Ins.	Never Hire	Only Hire with Ins.	Never Hire	Only Hire with Ins.
No Subsidy	0.099	0.181	0.151	0.251	0.175	0.233
	(0.047)	(0.049)	(0.048)	(0.048)	(0.053)	(0.056)
10% Subsidy†	0.026	0.151	0.179	0.242	0.185	0.283
	(0.055)	(0.060)	(0.055)	(0.056)	(0.050)	(0.051)
25% Subsidy	0.214	0.290	0.181	0.252	0.097	0.192
	(0.055)	(0.055)	(0.050)	(0.054)	(0.047)	(0.052)
50% Subsidy	0.054	0.189	0.117	0.207	0.209	0.252
	(0.053)	(0.055)	(0.056)	(0.051)	(0.046)	(0.047)
100% Subsidy	0.125	0.172	0.130	0.219	0.141	0.276
	(0.045)	(0.045)	(0.046)	(0.044)	(0.049)	(0.047)
Elasticity	-0.208	-0.144	-0.120	-0.119	-0.170	-0.209
	(0.082)	(0.074)	(0.082)	(0.070)	(0.088)	(0.076)
Mean Effect	0.105	0.195	0.150	0.234	0.161	0.245
vs. Baseline	(0.023)	(0.024)	(0.023)	(0.022)	(0.022)	(0.023)
	[0.100]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Mean Effect	–	–	0.045	0.039	0.056	0.050
vs. Omitted Group			(0.035)	(0.034)	(0.035)	(0.035)
	–	–	[0.500]	[0.700]	[0.500]	[0.400]
Firms	325	325	357	357	346	346
Managers	366	366	419	419	428	428

Appendix Table B.4: Performance History, Bounds on Treatment Effects

B. Policy Treatment	Performance History: 1 Job		5 Jobs		25 Jobs	
	Assume respondents who don't finish		Assume respondents who don't finish		Assume respondents who don't finish	
	Never Hire	Only Hire with Hist.	Never Hire	Only Hire with Hist.	Never Hire	Only Hire with Hist.
No Subsidy	0.087	0.169	0.213	0.274	0.172	0.268
	(0.049)	(0.052)	(0.050)	(0.050)	(0.048)	(0.049)
10% Subsidy†	0.081	0.209	0.247	0.337	0.098	0.159
	(0.050)	(0.051)	(0.061)	(0.059)	(0.051)	(0.051)
25% Subsidy	0.179	0.242	0.179	0.318	0.113	0.175
	(0.064)	(0.063)	(0.055)	(0.055)	(0.046)	(0.044)
50% Subsidy	0.096	0.227	0.111	0.180	0.095	0.156
	(0.059)	(0.057)	(0.047)	(0.049)	(0.049)	(0.050)
100% Subsidy	0.095	0.191	0.137	0.234	0.120	0.193
	(0.052)	(0.053)	(0.043)	(0.044)	(0.050)	(0.050)
Elasticity	-0.180	-0.172	-0.058	-0.067	-0.148	-0.124
	(0.092)	(0.083)	(0.082)	(0.071)	(0.088)	(0.078)
Mean Effect	0.104	0.205	0.171	0.261	0.120	0.191
vs. Baseline	(0.024)	(0.025)	(0.022)	(0.023)	(0.022)	(0.022)
	[0.100]	[0.000]	[0.000]	[0.000]	[0.100]	[0.000]
Mean Effect	–	–	0.067	0.056	0.016	-0.014
vs. Omitted Group			(0.036)	(0.035)	(0.037)	(0.035)
	–	–	[0.500]	[0.400]	[0.600]	[0.700]
Firms	321	321	345	345	360	360
Managers	376	376	400	400	437	437

Appendix C. Cost Effectiveness of Different Policies

This section discusses the potential costs of increasing demand for WCCs.

Wage Subsidies. A full wage subsidy increases demand for WCCs by roughly 20 percentage points. Given that the relationship between subsidy and hiring is roughly linear, increasing hiring by 10 percentage points requires a wage subsidy of roughly 50%. A typical hourly wage on the Platform is $15 per hour, meaning that it costs approximately $60 per worker per day to increase the share of firms hiring WCCs through wage subsidies.

Crime and Safety Insurance. By comparison, we find that $5,000 in crime and safety insurance increases WCC hiring by roughly 10 percentage points. Given that $5,000 is low relative to Platform revenues, we assume that the Platform self-insures. We are not aware of any comprehensive data regarding the rate at which WCCs occur liability for firms. However, insurance appears highly cost-effective at reasonable rates of assumed offense levels. For example, if we assume that WCCs have a 1 in 1,000 daily chance of incurring $5,000 in damages, the cost of providing insurance to increase WCC hiring by 10 percentage points is $5 per worker per day. If the probability is 1 in 200, the cost is $25 per worker per day.

Targeted Screening. Performance screening, including only workers with a less serious conviction, and providing objective information on the average productivity of WCCs can achieve these gains at even lower costs. Performance screening, for example, can increase the share of firms hiring WCCs at near-zero cost if WCCs are initially matched with firms willing to waive this requirement, or at the one-time cost of subsidizing the first set of matches until there is sufficient performance history. Including only workers with a less serious conviction can also be done at no additional cost to the Platform or firms given that background checks already include the nature of the conviction. Providing objective information on the average productivity of WCCs can also increase the hiring of WCCs at a negligible cost, given that such information already exists or can be newly generated at the one-time cost of subsidizing another set of matches.

Worker-Focused Policies. Research on improving employment outcomes for WCCs has generally focused on the workers themselves, considering such policies as job counseling, job and skills training, cognitive-behavioral therapy, and various "wraparound" services aimed at integrated WCCs into the community (e.g., housing assistance). Unfortunately many of these policies fail to boost WCC employment and earnings (Doleac, 2018, 2019).

One recent exception is Cook et al. (2015), who study a comprehensive program integrating skill training, counselling, and other services to help recently released WCCs gain employment. Their program, estimated to cost about $5,000 per participant, increased median earnings of WCCs in their first year out by $2,200. Therefore, the ratio of program costs to earnings benefits was roughly 2:1. At the end of 12 months, there was little difference in employment between treated and control individuals. This is substantially less cost effective than even the 50% wage

subsidy we study, for which the ratio of program costs to earnings benefits is 1:2. Providing $5,000 in insurance has a far more favorable ratio, equal to 1:24 if we assume a daily risk of 0.001. Suppose, for example, a person workers N jobs in a year, where s/he would expect to earn about $120 per day or 120N. The annual direct cost is $5,000*.001*N=5N, yielding a cost-benefit ratio of 1:24.

Summary. This cost-benefit analysis is highly stylized, focusing on increasing employment and earnings. Given these parameters, we find that wage subsidies can increase demand for WCCs, but that other policies including crime and safety insurance and targeted screening options appear much more cost effective. Policymakers may have other valuable policy objectives, including reducing recidivism and improving well-being of formerly incarcerated individuals. Still, the results suggest that firm-side policies, like the ones we study, may be an important part of a policymaker's toolkit. Their potential cost-effectiveness is attractive and is worthy of future study.

Appendix Table C.1: The Cost Effectiveness of Different Treatments for Increasing the Share of Firms Hiring WCCs by 10 Percentage Points

Policy	Cost per worker-day
50% wage subsidy	\$60
\$5k Insurance, $p = 0.001$	\$5
\$5k Insurance, $p = 0.01$	\$50
Require 1 past job. Assume Platform hires, $E(\pi) = 0$	Free
Group information	Free

Notes: p is the assumed daily probability of a bad event where the full insurance policy is paid out. We assume a wage of \$15 per hour. We ignore administrative costs.