The Mark or Trace of a Criminal Record:

A Survey Experiment of Race and Criminal Record Signaling

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ABSTRACT

Employment discrimination from a criminal record is a salient social fact, evidenced by a robust body of experimental research. In Part 1 of this study, we analyze prior criminal record hiring experiments—comprising in-person audits, online audits, and opt-in surveys—to describe patterns over time in employer receptivity to applicants of different races with criminal records. In Part 2, we use a novel experimental survey of 1,080 employers to measure how differences in the signaling of a criminal record impact the criminal record-employment relationship. Our results reveal a substantial hiring penalty for an official criminal record (conveyed by a background check report), with a smaller but still significant penalty for an unofficial criminal record (an internet search engine "hit"). The experiment also shows that the official criminal record penalty is significantly larger for White applicants than Black applicants. Although the latter finding was counter to expectations informed by prior studies, it is less surprising considering our Part 1 findings, which reveal a closing racial gap in the criminal record penalty over the last 20 years. We discuss how broader legal, social, and technological changes, as well as changes in methodologies, impact our understanding today of criminal records, race, and employment.

1. INTRODUCTION

Among the most enduring forms of stigma in the labor market is an official record of involvement with the criminal legal system. It is fair to characterize it as a form of extralegal punishment, even when the contact is limited to arrest with no further action (Schwartz and Skolnick 1962; Uggen et al. 2014). It is so stigmatizing that job applicants with criminal records are faced with the prospect of concealment, or at the very least carefully managing disclosure of their criminal record (Harding 2003; Lindsay 2022). At the same time, the seeming universality of background checks makes non-disclosure nearly impossible, making employers key discretionary decision-makers regarding hiring and criminal records (Bushway and Kalra 2021).

The experimental audit is the research design that has provided the clearest documentation of the stigmatic effects of a criminal record for employment, with the most impactful work by Devah Pager (Pager 2003; Pager, Bonikowski, and Western 2009; Pager, Western, and Sugie 2009). While there are many variations of an audit, it has been the chief design for the study of hiring discrimination in general (Gaddis 2018; Neumark 2018). An audit involves submitting applications or resumes to posted job openings and recording the response from employers. Key information about the applicants can be randomly varied, so when two applicants with otherwise identical credentials have different "callback" rates, that difference can be credibly attributed to the characteristic that distinguishes them. Related to an audit, a growing number of studies have experimentally surveyed employers directly by presenting them with portfolios of (hypothetical) job applicants and asking them about their willingness to hire the individual(s) in question.

Importantly, variations on study design also mean the criminal record is signaled in different ways, sometimes via the job application itself ("checking the box"), through in-person disclosures to hiring managers, or through more subtle signals, such as listing a probation officer as a reference or employment in a prison industry. Beyond the confines of the audit, major changes in employer background screening also have implications for how employers interpret and receive criminal record information in the real world (Jacobs 2015). While commercial background checking has continued its rapid, now nearly universal, adoption by employers, there are also more channels of "informal" publicly available criminal record information, such as through a routine internet search (Lageson 2020). In our study, we seek to examine the "mark" of a criminal record—an officially sanctioned, stigmatizing public label—as well as the "trace" of a criminal record—the existence of a digital record in an online search engine, which may or may not be validated by an official criminal history. Unlike the more commonly studied mark of a criminal record that appears on a formal background check or is clearly disclosed by an applicant to a prospective employer, the trace of a criminal record is more ambiguous. It might convey an official state sanction, a remnant of an official sanction (e.g., an expunged record not updated in private databases), or mere intimation of criminality (e.g., a mugshot or non-conviction record).

Understanding the effect of "marks" and "traces" of a criminal record in today's digital environment, however, is best understood within the context of prior studies of criminal record discrimination. In what follows, we provide a study in two parts. In Part 1, we provide a quantitative summary of prior criminal record hiring experiments covering six decades. We emphasize shifts over time in how employers and/or survey respondents respond to criminal records. We describe how changes in technology, changes in study design, and broader contextual factors (such as regulatory changes, public opinion, and the scope and availability of criminal records) might lead to meaningful shifts in our expectations about how the intersection of a criminal record and applicant race impacts hireability.

In Part 2, we present results from a novel survey experiment of employers to test the effect of different record signals (an "official" background check vs. an "unofficial" criminal record hit from a Google search result), and whether or how this varies by the race of the job seeker. Our experimental survey uses a 2 x 2 x 2 design and randomizes a criminal record (yes vs. no), applicant race (Black vs. White), and a Google hit for a criminal history (yes vs. no), allowing us to explore additive as well as two- and three-way interactive effects of the manipulations.

Thus, Part 1 of the study details historical trends in how criminal record and racial discrimination has been measured across dozens of studies, which allows us to better contextualize the findings of our novel survey experiment in Part 2. Taken together, the studies make a clear contribution regarding how shifts in methodology and the broader institutional and social landscape impact our understanding of criminal records, race, and experimental research today.

2. EXPERIMENTAL APPROACHES TO STUDYING CRIMINAL RECORD DISCRIMINATION IN HIRING

Experimental audits are an oft-used methodology in the social sciences for measuring employer attitudes toward job candidates with criminal records, routinely measured by an employer's positive assessment (including callbacks or favorable survey-based assessments) to a variety of applicant signals (including type and severity of record and race or gender of applicant). The history of the experimental employer audit begins with the 1962 publication of Richard Schwartz and Jerome Skolnick's investigation into how employers in upstate New York assess a job candidate with no criminal record against a job candidate who presented a variety of judicial system outcomes from an assault accusation, including a conviction and sentence, a trial and acquittal, or a trial and acquittal with a judicial letter certifying a not-guilty finding. Though relying on non-random assignment of employers to study conditions, this early study showed that a criminal record indeed mattered for employment, even when there was a not-guilty finding.

For the next forty years, with the notable exception of a pair of studies by Finn and Fontaine (Finn and Fontaine 1983; 1985), criminal record hiring experiments were practically non-existent. This changed dramatically with the publication of Devah Pager's (2003) seminal audit of employers in Milwaukee, which showed significant discrimination based on a criminal record, particularly for Black job applicants. Interest in audit studies was piqued; a series of follow-up studies by Pager and colleagues showed similar patterns in New York City (Pager, Bonikowski, and Western 2009), and by other researchers in Milwaukee (Wells 2013), Phoenix (Decker et al. 2015; Ortiz 2014) and Minneapolis-St. Paul (Uggen et al. 2014). These audit studies all shared similar design characteristics, most notably an in-person test where a "confederate" hired by the research team physically traveled to job sites to apply for jobs and signaled a criminal record by "checking the box" on a job application or by verbal disclosure to a hiring manager. These studies varied in the type of record tested; for instance while the original audit by Pager (2003) tested felony-level convictions, the audit by Uggen et al. (2014) tested misdemeanor non-convictions. These studies showed the persistence of racially biased criminal record-based discrimination even amidst variations in the record itself.

Technology began to change both the hiring and audit methodology landscape. Most significantly, the advent of online application portals meant researchers could now test a much larger N of employers at significantly lower cost. For instance, Galgano (2009) submitted 600 total resumes via careerbuilder.com in an online audit in Chicago, Ortiz (2014) paired an in-person audit of 60 employers with an online audit of 515 employers in a test of discrimination for female applicants with criminal records, as did Decker et al. (2015) for male applicants, with 57 employers audited in person and 518 audited online. A number of online audits followed, including in Raleigh-Durham (Cundiff 2016), Columbus (Leasure 2019; Leasure and Andersen 2016; 2017; 2020), New Jersey and New York (Agan and Starr 2018), Oakland (Mobasseri 2019), Cleveland (Leasure and Kaminski 2021; Leasure and Zhang 2021) and Prince William County (Ripper 2022). Several studies also leveraged the online format to reach numerous cities within a single state (Cerda-Jara, Elster, and Harding 2020) or across several states (Lindsay 2021).

The rise of panel survey services also offered new avenues for testing employer assessments of criminal records. Whereas some studies survey individuals in personnel management classes or human resource organizations (Finn and Fontaine 1983; 1985; Kukucka, Applegarth, and Mello 2020), companies such as Qualtrics and YouGov now offer entire samples composed of opt-in survey respondents who have worked as a hiring manager. These advances in respondent availability means the audit model can now easily be adapted to a survey format, with the added bonus of respondents having real-life hiring experience (DeWitt and Denver 2020; Santos, Jaynes, and Thomas 2023; Sugie, Zatz, and Augustine 2020). These studies present respondents with hypothetical portfolios and ask them to rate their willingness to hire the person in question. That said, surveys may differ significantly from audits due to social desirability bias of the respondents; audit studies that have subsequently interviewed audited employers show they are more likely to profess willingness to hire applicants with records than they are when independently audited (Lageson, Vuolo, and Uggen 2015; Pager and Quillian 2005). On the other hand, audits are constrained by real-world conditions, including labor markets and applicant pools. Thus, questions remain regarding similarities between in-person audits, online audits, and opt-in surveys.

Contextual shifts in hiring and criminal records

Beyond recent shifts in experimental methodologies, broader shifts have also likely impacted how employers assess criminal records. First, state and local ordinances "banning the box" have likely caught the attention of employers and potentially changed the way they respond to criminal records. These policies also emerged amidst a backdrop of mass criminalization and a rise in background screening, increasing the likelihood that employers now routinely encounter criminal records while screening applicants. Second, public opinion of the criminal legal system has shifted in the past few years, at least partially in response to high-profile police violence. Third, the type, volume, and availability of criminal record information available to employers in the digital age has likely impacted how employers consider and weigh records.

The Rise of Criminal Records, Background Checks, and Fair Chance Hiring Policies

The number of people who have a criminal record that appears on a background check has increased in the age of mass criminalization (Brame et al. 2012; Shannon et al. 2017; Wakefield 2022). The simultaneous rise of background checking (McElhattan 2022) means that as more people were assigned the criminal label, more institutional actors expanded inquiries into such information, creating patterns of discriminatory outcomes. Perhaps in response, the past two decades have also seen significant policy efforts to reduce the stigma of a criminal record in employment contexts. In 1998, Hawaii became the first state to implement "Ban the Box," a policy that requires employers to remove questions on a job application that inquire about a criminal history. Since then, 37 states and over 150 cities and counties have adopted similar laws (Avery and Lu 2021). Advocates frame these laws as combating racial discrimination and as "smart on crime" measures to increase economic opportunities for people with a criminal history (Emsellem and Rodriguez 2015)—an estimated 1 in 3 Americans (The Sentencing Project 2022). While Ban the Box prevents employers from asking about criminal histories at the application stage, employers may conduct a background check at later stages of hiring (Raphael 2021).

It is not clear whether these policies broadly increase access to employment. Agan and Starr's (2018) audit was conducted pre- and post-Ban the Box and found that, lacking criminal record information at the application stage, employers were more likely to discriminate against Black applicants. The authors draw upon statistical discrimination theory to argue that employers relied on racial stereotypes regarding Black criminality in assessing candidates who could not otherwise "prove" they did not have a criminal record. That said, these policies likely have had some impact on hiring processes (Schneider et al. 2022).

It is also possible that audit study findings have directly shaped policy, and by extension, employer behavior. In 2012, the EEOC issued enforcement guidance on the consideration of arrest and conviction records in employment decisions under Title VII of the Civil Rights Act, warning employers against treating candidates with records differently based on their race or maintaining a criminal record policy that has a disparate impact by race (e.g. implementing a blanket exclusion of people with arrest records, given racial disparities in arrest rates) (U.S. Equal Employment Opportunity Commission 2012). In developing this guidance, the EEOC cited audit research, detailing Devah Pager's work in an extended footnote (see EEOC 2012, footnote 55). In this sense, experimental audits may have had a causal effect on policy making, which in turn may impact future audit results due to a changing employment law landscape.

Public Opinion, Race, and the Criminal Legal System

Building from the 2014 police killing of Michael Brown in Ferguson, public attention and critique of the criminal legal system has increased (Gross and Mann 2017). The uprisings and protests of 2020 further solidified public interest in the criminal legal system following the murder of George Floyd by Minneapolis police officers. Some of America's largest employers were quick to respond: McDonalds issued a video proclaiming that Trayvon Martin, Michael Brown, and George Floyd were "one of us" (McDonalds 2020). Jamie Dimon, chief executive of JPMorgan Chase, kneeled with staff at a branch of the United States' largest bank, adopting the protest pose of former quarterback Colin Kapernick (Sumagaysay 2021). America's 50 biggest public companies collectively committed $49.5 billion to addressing racial inequality (Jan, McGregor, and Hoyer 2021). These public conversations at the highest levels of corporate America may have swayed employers' views of criminal records, at least in the short term. For example, an experimental audit by Kirk and Rovira (2022) on race and employment found that a White applicant advantage in employer callbacks and requests for an interview receded during the protests and unrest following the killing of George Floyd, even to the point of producing a Black advantage.

Changes in Criminal Record Scope and Availability

A third factor that may impact employers' views of criminal records is the widening availability of criminal record information. While employers historically relied on a single source of information, such as by consulting with the local court or hiring a criminal background screener, employers can now conduct a simple internet search to learn information about a candidate's record (Lageson 2020). This new landscape of information may temper employer assessments of a criminal record at the earliest stage of hiring; for example, employers may be aware that unregulated information sources may be messy or incomplete, may overlook a record in lieu of a personal interview, or may rely on hiring instincts rather often ambiguous legal system information (Lageson et al. 2015). Employers may also work in an environment where final hiring decisions are outsourced to Human Resources, rather than carrying the responsibility to independently assess the record themselves (Lageson et al. 2015), or in Fair Chance Hiring contexts, delay a hiring decision based on a criminal record until an offer of employment is made (Rose 2021), even if the criminal record is revealed through a quick Google search. In general, internet searches of job candidates are extremely common—trade publication surveys of employers have shown that up to 98% of hiring managers conduct some type of online background research on applicants (McKeon 2020).

Whether these "unofficial" signals of criminality matter for hiring remains unclear, though some limited evidence provides preliminary guidance. Sugie, Zatz, and Augustine (2020) conducted an experimental survey where employers were presented with both official (criminal record) and unofficial (social media posts) signals reflective of the same behavior (drug use). The authors found that official signals were evaluated more negatively by employers, suggesting that the institutional marker of a formal record triggers additional stigma above that of the act of using drugs alone.

In what follows, we provide a quantitative summary of six decades of experimental research to develop a more robust understanding of trends over time. This assessment, in turn, sets the stage for our own survey experiment that explicitly tests one contextual shift in hiring—the availability of criminal records on the internet. Taken together, both parts of our study deepen our understanding of how research methodologies and broader shifts are changing the state of knowledge around hiring, criminal records, and race in America.

3. PART 1: QUANTITATIVE SUMMARY OF PRIOR CRIMINAL RECORD HIRING EXPERIMENTS

We begin our study with a quantitative description of criminal record discrimination from prior experimental studies. We sought to code the callback rates from published and unpublished criminal record hiring experiments conducted in the United States. These include in-person audits, online audits, and opt-in surveys, where the key manipulation was the presence or absence of some kind of criminal record. We limit our attention to the US because of the enduring salience of racial discrimination (Quillian et al. 2017), the public availability of criminal records, and the inclusion of race and criminal record in our own experimental study. The Supplementary Information for this article further describes our methodology.

We identified 32 studies (or study arms) from which we were able to obtain 144 estimates of the difference in callback by a criminal record, and 23 studies from which we could obtain 77 estimates of the Black/White racial difference in callback.[1] For criminal record discrimination, we computed the difference in callback rates as a percentage difference between same-race groups differing in the presence or absence of a criminal record, calculated as follows:

$$100 \times \left(\frac{\bar{Y}_{record}}{\bar{Y}_{no.record}} - 1 \right)$$

[1] The information in Pager et al. (2009a) does not allow calculation of a same-race estimate of the criminal record penalty. This is because the groups comprise White applicants with a criminal record, Black applicants without a criminal record, and Latino applicants without a criminal record. They do, however, allow calculation of the racial disparity in callback.

For racial discrimination, we modified the formula to compute the difference in callback rate among Black applicants as a percentage of the callback rate among White applicants:

$$100 \times \left(\frac{\bar{Y}_{black}}{\bar{Y}_{white}} - 1\right)$$

Because opt-in surveys rely on ordinal willingness-to-hire outcomes rather than binary callback outcomes, we first normed the means on a 0-100 scale so they resemble an approximate callback percentage. This is the percent of maximum possible score (POMP) of Cohen et al. (1999), and is calculated as:

$$\bar{Y}_{norm} = 100 \times \left(\frac{\bar{Y} - Y_{min}}{Y_{max} - Y_{min}}\right)$$

The normed means, now roughly equivalent to callback percentages, are then plugged into the percent difference formula.

In Table 1, in the top panel, we provide summaries of the percentage difference in callback formed by comparing applicants with a criminal record to their same-sex, same-race, and equivalently-credentialed counterparts without a criminal record. In addition to averaging all these estimates, we condition on the race/ethnicity of the job seeker to judge whether the magnitude of the criminal record penalty meaningfully differs. We also report adjusted differences that condition on study design, as the baseline callback percentage is much higher in opt-in surveys where hiring managers are reviewing just one or a handful of hypothetical applicants rather than a large pool of actual job applicants. In the bottom panel, we provide summaries of Black/White racial discrimination, which compare Black applicants to their White counterparts possessing the same criminal record, sex, work experience, and educational credentials. For all estimates, we report both unweighted mean percent differences as well as weighted mean percent differences, where the weights derive from the total number of applications informing each difference.

*** Table 1 about here ***

As seen in the first row of the table, the impact of a criminal record on hiring is easily discernible: the likelihood of callback is more than one-third lower for applicants with a criminal record relative to their counterparts without a criminal record (unweighted mean = –38.0%). Interestingly, the magnitude of the criminal record penalty is very consistent across applicant race/ethnicity. On average, the (unweighted) criminal record penalty is –35.0% among Black job applicants, –33.6% among Latino job applicants, and –34.9% among White job applicants. When judging the magnitude of the criminal record penalty across the three distinct study designs, there is modest variation, but the callback differences are still substantial. For example, applicants with a criminal record are 38.7% less likely to receive a callback in in-person audits, 33.6% less likely in online audits, and 45.6% less likely in opt-in surveys.

To probe the estimates of hiring discrimination by criminal record further, in Figure 1, we supplement the means from Table 1 with the trend in callback difference by year of data collection and study design, along with the marginal distribution of callback by study design. Note that weighted estimates are shown, but the line of best fit is based on unweighted estimates. For reference, we plot the corresponding estimates from our survey experiment with Xs (X_R for an official criminal record; X_G for an unofficial criminal record indicated via Google hit), but reserve discussion until a later point. The estimates are horizontally jittered by up to two years so they do not overly cluster in the graph.[2]

[2] As a side note to Figure 1, we can discern growth in experimental hiring studies over a 60-year period as well as adoption of new experimental designs. With a casual glance, one can quickly pick out the estimates from Schwartz and Skolnick (1962) and Finn and Fontaine (1983, 1985). Renewed interest in criminal record hiring discrimination

*** Figure 1 about here ***

Figure 1 shows the difference in callback for applicants with a record (as a percentage of the callback rate of applicants without a criminal record) is quite clearly less than zero, with isolated exceptions.[3] Across all study estimates, the criminal record penalty is –67.7% at the 5th percentile and –4.8% at the 95th percentile (not shown), suggesting there is variation across studies but little reason to indicate a criminal record is anything other than a liability in labor markets. We also see from the line of best fit that the impact of a criminal record on callback has apparently grown smaller over time. The coefficient on the trend is 0.45, which is the average yearly increment in the criminal record penalty (in percent difference metric) over the 60-year period under consideration: from 1960 to 2020, the criminal record penalty declined by about 27 percentage points (0.45 x 60), as predicted by the line of best fit. When adjustment is made for study design, the slope of the trend is 0.38, suggesting a small portion of the apparent decline in criminal record discrimination over time is a design artifact, while the largest share likely reflects a potentially real change in employer behavior toward applicants with criminal records.

In Figure 2, we explore the intersection of criminal record and race by plotting the difference in callback for applicants with a criminal record versus no record, separately for Black and White applicants.[4] There is an interesting racial difference in the trend of the criminal record

was inspired by Pager (2003) and accelerated with replication efforts and widespread adoption of the online audit and opt-in survey.

[3] The anomalously large positive percent difference in 2012 (+52.5%) is not an error. It comes from the in-person audit of Oriz (2014), who reports an 18.0% callback rate among White women with a criminal record, versus 11.8% among White women without a criminal record (100 x [0.180 / 0.118 – 1] = 52.5%). The smaller positive percent difference in 2014 (+11.0%) comes from the online audit of Mobasseri (2019), who reports an 18.2% callback rate among Black men with a criminal record, compared to 16.4% among Black men without a criminal record.

[4] The estimates shown in Figure 2 are a subset of what is already shown in Figure 1, where the race of the applicant is either Black or White. Figure 2 begins with the estimates published in Pager (2003), since the earlier criminal

penalty over time. For Black job applicants, the line of best fit is upward sloping, indicating the criminal record penalty has gotten smaller over the past 20 years—the coefficient on the trend is 1.32 per year. By comparison, for White job applicants, the line of best fit is downward sloping at a rate of –0.45 per year over the period shown. This suggests the size of the criminal record penalty has shrunk over time for Black applicants, and has grown modestly over time for White applicants. In the 2000s, criminal record hiring experiments tended to find that Black applicants experienced a larger penalty from a criminal record than their White peers, and by a considerable margin. But this seems to have shifted in recent years, such that the criminal record penalty is roughly equal for Black and White applicants—note the end points of the two lines of best fit. If there is any non-zero difference of note, it is quite possible that White applicants nowadays tend to experience the larger criminal record penalty.

*** Figure 2 about here ***

Returning to Table 1, in the lower panel, we next examine the evidence concerning racial discrimination in callback from prior criminal record hiring experiments. These compare callback rates of Black and White job applicants with otherwise identical profiles—same criminal record, same sex, same work experience, and same educational credential. Negative estimates indicate a Black disadvantage in callback, while positive estimates indicate a Black advantage. Across all estimates, there is a sizable racial disparity that disadvantages Black applicants (unweighted mean = –25.1%). Yet there is also a great deal of heterogeneity connected to experimental design. On average, the (unweighted) estimates suggest Black

record hiring experiments were focused only on the impact of a criminal record and did not vary the race of the applicant.

applicants are 49.5% less likely to receive a callback in in-person audits, 18.8% less likely in online audits, but 5.5% more likely in opt-in surveys. Thus, while the magnitude of criminal record discrimination is not strongly linked to study design, both the direction and magnitude of racial discrimination is very strongly linked with study design. We caution, however, that the estimates from opt-in surveys are based on a single study (DeWitt and Denver 2020).

In Figure 3, we plot the Black/White callback differences by year of data collection and study design. The slope for the line of best fit is 2.2, which can be taken to mean that racial discrimination in criminal record hiring experiments has declined substantially over the period shown—by just over two percentage points per year. However, the marginal distribution of callback by study design provides a clear indication of design sensitivity. Adjusting for study design, in fact, the slope of the line of best fit is reduced to 0.6.[5] Thus, the lion's share of the apparent decline in racial discrimination in callback is attributable to changes over time in experimental design. Important for the current study is that the few available estimates from opt-in surveys cluster around 0%, indicating racial parity, and if anything, suggest a Black advantage in callback.

*** Figure 3 about here ***

Taking stock of what we have learned over six decades of criminal record hiring experiments in the US, the criminal record penalty remains large and persistent. However, there is some indication it has grown smaller over time, and this is not attributable to changes in study design. The criminal record penalty also remains large for Black and White job seekers, but the

[5] There is also evidence of curvilinearity in the callback differences in Figure 3, although this is fully accounted for by study design.

long-term trend suggests slow convergence in magnitudes. To wit, early studies tended to find a larger criminal record penalty for Black job applicants, but more recent studies tend to find no racial difference in magnitudes—and possibly a larger penalty for White job applicants. The pattern concerning racial disparity is quite stark by comparison. There has been a pronounced decline in racial discrimination over time, but this is almost fully attributable to the adoption of new designs for criminal record hiring experiments. Opt-in surveys, although few in number, provide evidence of no apparent racial discrimination, and possibly a reversal of the form of racial discrimination historically observed in criminal record hiring experiments. We use these assessments to motivate our own opt-in survey experiment, described next.

4. PART 2: SURVEY EXPERIMENT

We have offered general theories that might explain the trends captured in our quantitative assessment of past studies, including the influence of Fair Chance Hiring and Ban the Box policies, shifts in public opinion and awareness regarding race and the criminal legal system, and changes in information technology that make accessing criminal record information easier than ever—and perhaps less consequential. We note further that changes in study design make some contribution, as the trends are muted when we adjust for whether the design is an in-person audit, online audit, or opt-in survey. Accordingly, we now test the effect of a criminal record through an increasingly popular method (the opt-in survey) and as communicated in a way that reflects changes in technology: an official "mark" on a criminal background check, or an unofficial "trace" from an online search engine "hit" for the applicant's name. Following prior literature, we also test whether employer survey respondents assess this criminal record differently by applicant race.

The survey experiment asked former hiring managers to assess a set of applicant materials for a position as a front desk clerk at a hotel in Cleveland. The respondents consist of 1,080 YouGov panel respondents who have hired employees in the past.[6] If selected to participate, respondents were randomly assigned to one of eight conditions. These conditions derived from a 2 x 2 x 2 design, which determined the application materials a respondent reviewed, based on a different combination of the following: (1) an official criminal record (criminal conviction on a background screening report vs. no record reported on background screening report); (2) applicant race (Black vs. White); and (3) an unofficial criminal record (criminal record conveyed via Google search results vs. no record signaled on Google search results).

Survey respondents selected for the study were told they were hiring a front clerk position at a hotel and received the following direction: *You are working as the Hiring Manager for Westside Hotel in Cleveland, Ohio. The following applicant has applied for the job. Please view their application materials, then answer a set of questions regarding whether or not they should be hired.* Respondents then reviewed three documents presented on a single screen: (1) a resume; (2) a criminal background report with applicant photo; and (3) a screenshot of Google search results. After reviewing the materials for at least 90 seconds, respondents were presented with a series of survey questions. All study materials are included in the Supplement, but we briefly describe how race and criminal record were communicated in these materials and how we measured willingness to hire.

[6] Respondents were selected after answering a screening question: "Have you been involved in making hiring decisions (either for your employer or personally as a business owner) anytime in the last 5 years?" Respondents were compensated 2,000 points on the YouGov platform. More information about YouGov's participants can be accessed at https://today.yougov.com/about/panel-methodology.

Criminal record: The criminal record conveyed on both the official background check and the Google search results screenshot consisted of an arrest and dismissal in 2014 for trespassing, and an arrest and conviction in 2016 for petty theft. The conviction carried a $150 fine and 6 months of probation. We selected misdemeanors due to their commonality in the criminal legal system (Kohler-Hausmann 2019; Natapoff 2018), but likelihood to produce some discriminatory effect in employment settings (Uggen et al 2014). In terms of misdemeanor charges and convictions, Natapoff (2018) estimates that approximately 80% of the criminal docket consists of misdemeanor cases, while CJARS estimates that per capita rates of misdemeanor defendants in the handful of states where data are available (which does not include Ohio, the residence of our test subjects) range from 1,297 per 100,000 (Oklahoma) to 5,545 per 100,000 (North Dakota) (CJARS n.d.). The Brennan Center estimates that 45 million Americans—approximately 14% of the population—have a misdemeanor conviction (Craigie et al 2020). In terms of workers with arrest records, a recent study by Bushway et al (2022) using the National Longitudinal Survey of Youth found that 44% of employed Black men, 32% of employed white men, and 36% of employed Hispanic men (ages 30 to 38) have an arrest record. Thus, we selected a criminal record profile (two arrests, one leading to a non-conviction and one leading to a misdemeanor conviction) likely to be prevalent among men similar to our test subjects.

Criminal background check: A 1-page official criminal background report included the applicant's name and address, photo, and list of criminal history events. For the control group, the official background check noted that a criminal history "does not exist" for the applicant.

Applicant race: Race was directly signaled via the criminal background check through a photo accredited to Facebook, which was sourced through the Creative Commons database.

“DeShawn Johnson” is Black and “Kyle Johnson” is White. In both photos, the applicant is facing the camera head on and smiling.

Google search results: A 1-page screenshot of Google search results presented a list of hits for a search of “*applicant name* + Cleveland.” For the treatment group, both the arrest/dismissal for trespassing and the conviction for petty theft appear in the top search results. For the control group, the Google search results include links to routine people search websites and a local high school football website.

Resumes: Applicant resumes were identical except for the first name of the applicant. “DeShawn Johnson” and “Kyle Johnson” both had work experience in hotels, a restaurant chain, and a warehouse, as well as a community college degree.

Willingness to hire refers to the respondent’s assessment of their likelihood of calling back or interviewing the job applicant. This is measured on a scale from 1 (very unlikely) to 7 (very likely).[7] We retain this measure in ordinal metric for the analysis. To facilitate comparison with prior criminal record hiring experiments, at relevant points, we also norm willingness to hire on a 0-100 scale so it resembles a callback percentage. This norming procedure yields the “percent of maximum possible score” (POMP) of Cohen et al. (1999), and we treat the ratio of POMPs for two contrasted groups as roughly equivalent to the ratio of two callback percentages. These POMP ratios are shown as Xs in Figures 1-3, and details on their construction are provided in the Supplement.

We also measure a number of respondent characteristics provided by YouGov, which are used as pretest variables to confirm balance of the random assignment. These include

[7] 1 = very likely, 2 = likely, 3 = somewhat likely, 4 = neither likely nor unlikely, 5 = somewhat unlikely, 6 = unlikely, 7 = very unlikely. For analysis, the categories are reverse coded so a higher value reflects greater willingness to hire.

demographics (age, sex, race), family circumstances (marital status, children), socioeconomic status (education, employment, income, industry), and a variety of political variables (partisanship, voting, political interest) as well as religiosity. Table 2 provides descriptive statistics for all measures. Additional information regarding the measurement of each variable is included in the Supplement.

*** Table 2 about here ***

For weighting purposes, YouGov tracked the demographics of both respondents who completed the survey as well as those who were screened out (because they were not involved in hiring). The YouGov analytics team then weighted all cases (completes + screenouts) to a general population frame (in this case representing employed US residents 21-65). Following this, YouGov subset and rebalanced the weights for clean complete cases. Although we report unweighted estimates, in sensitivity analyses that are not shown, we confirmed all results using the weights.

Analysis

We begin with an analysis of variance (ANOVA) of willingness to hire. Since the study design is a 2 x 2 x 2 factorial experiment, this is three-way ANOVA, which will yield F-statistics on the additive effect for each manipulation—criminal record (yes vs. no), applicant race (Black vs. White), and Google hit (yes vs. no)—as well as F-statistics on all two- and three-way interactions. We select a 5% (two-sided) criterion to judge statistical significance, but wish to also address practical significance that does not hinge on statistical significance. We thus supplement the ANOVA results with effect size estimates for the additive and interactive components (Cohen 1988).

We then perform linear regression to obtain estimates of the impact of each manipulation. These models preserve the 2 x 2 x 2 factorial design, but to economize output, we report additive marginal effects and only the interaction effects that are statistically significant from the three-way ANOVA. We supplement asymptotic p-values from the regression models with p-values obtained via randomization inference (Athey and Imbens 2017). These provide tests of a sharp null hypothesis of no effect of the manipulations for any unit. In the procedure, individuals are randomly reassigned to levels of the manipulation of interest in order to estimate "placebo" effects of the manipulations. After doing this repeatedly, the original estimate is compared to the distribution of placebo estimates to obtain a two-sided p-value directly, without the need for the central limit theorem. Because it is infeasible to enumerate all possible reassignments, the p-values are simulated from 5,000 draws (on software, see Heß, 2017).[8]

Results

In Figure 4, we graph the distribution of the seven-point willingness-to-hire outcome for each of the eight groups (2 x 2 x 2) formed by the fully-crossed manipulations. Several features stand out. First, individuals with a criminal record are substantially less likely to receive a favorable assessment than individuals without a criminal record. This is true irrespective of race and confirms our expectation from prior research that an official criminal record is a major stigma in hiring situations—even the hypothetical ones in our opt-in survey. Second, Black applicants are rated more favorably than White applicants. This minority preference seems to be

[8] For randomization inference, we preserve the 2 x 2 x 2 design by randomly reassigning each individual to one of the 8 groups formed by the fully-crossed manipulations.

true irrespective of the presence of a criminal record or a Google hit. This conflicts with our expectation from prior criminal record hiring experiments, although it aligns more closely with the few opt-in surveys that exist. Third, applicants with a Google hit are slightly less favorable in a hiring context than their peers with a clean Google search result. This form of stigma appears to be less pronounced than the stigma of an official criminal record, but still signals unofficial (though potentially unconfirmed) criminality.

*** Figure 4 about here ***

In Table 3, we report the findings from three-way analysis of variance (ANOVA). The rows provide F-statistics and effect sizes (eta) for the different components of ANOVA. The first three rows test the independent additive influence of each manipulation—criminal record, applicant race, and Google hit. The results indicate that each manipulation yields a significant difference in willingness to hire, as judged by the p-values. The impact of criminal record and applicant race are especially notable, when we consider that 0.3 is regarded as a medium effect size and 0.5 a large effect size (Cohen 1988). By comparison, the impact of a Google hit just exceeds the level of what would be regarded as a small effect size (0.1).

*** Table 3 about here ***

The next three rows in the table pertain to the two-way interactions. For example, these test (1) whether the effect of a criminal record on willingness to hire is equivalent for Black and White applicants, (2) whether the effect of a Google hit is equivalent for Black and White applicants, and (3) whether the effect of a Google hit is equivalent for applicants with and without an official criminal record (alternatively, whether the effect of a criminal record is equivalent for applicants with and without a Google hit). Of the three possible two-way interactions, only one is statistically significant: criminal record x applicant race. The effect size being no larger than 0.1 indicates a small but discernible effect for this interaction, suggesting that the impact of a criminal record differs meaningfully by applicant race. Note that the F-statistic and effect size are agnostic about the nature of the interaction, so further description must await the regression model results.

The remaining two-way interactions are not statistically significant by any conventional criteria, nor do the effect sizes approach practical significance. In other words, the impact of a Google hit on willingness to hire does not differ by applicant race, nor does it differ by the presence or absence of an official criminal record. This latter finding is especially intriguing, as it suggests that a criminal record and Google hit have independent effects on willingness to hire. When we consider the three-way interaction, the results point to nothing that approaches statistical or practical significance. Not shown in the table is eta-squared for the ANOVA model, which is interpreted the same as R-squared from linear regression. This indicates that the model explains 40 percent of total variation in willingness to hire.

The findings thus far point to criminal record, applicant race, and a Google hit—in descending order by size of the effect—as each producing significant differences in willingness to hire. Furthermore, among the possible two- and three-way interactions formed by the manipulations, only the interaction between criminal record and applicant race is worth noting, albeit small as judged by the effect size. We nevertheless explore all of the two-way interactions more systematically in the regression results below.

In Table 4, we present results from a linear regression model of willingness to hire including the fully-crossed manipulations with all two- and three-way interactions. Rather than show the full set of coefficients summarizing the main and interaction effects (see Appendix C), we provide just the marginal effects of each of the three manipulations, as well as the conditional marginal effects describing the two-way interactions. In addition to providing regression-based (asymptotic) p-values in the table, we provide p-values obtained from randomization inference with 5,000 draws. Using the POMP norming procedure described earlier (Cohen et al. 1999), we also convert the predictions implied by the regression model into approximate callback rates. This allows us to estimate percent differences in callback which we can compare to prior criminal record hiring experiments (see the Xs graphed in Figures 1-3).

*** Table 4 about here ***

The marginal effect for criminal record, as expected, indicates applicants with an official criminal record are rated two full points lower on willingness to hire. A two-point drop on the ordinal callback measure is substantial and represents a difference between "very likely" and "somewhat likely," for example, or between "somewhat likely" and "somewhat unlikely." The relative difference is –41.7% when we norm the marginal predictions to convert them to approximate callback rates, and then compute the percent difference between them. This estimate closely aligns with prior criminal record hiring experiments—including prior opt-in surveys (see Table 1)—and in fact is in the middle of those prior estimates (see the X_R marked in Figure 1). It also comports with prior research about the durability of labor market stigma from a criminal record.

The marginal effect for applicant race indicates Black applicants are rated one full point higher on willingness to hire than White applicants. As a relative difference, the normed marginal predictions indicate Black applicants are 30.0% more likely to receive a callback than White applicants. This provides evidence of a reversal of racial discrimination that is unexpected when viewed in the context of existing research on racial discrimination in hiring (Quillian et al. 2017), and is an apparent outlier even when compared to criminal record hiring experiments (see the X marked in Figure 3). But it is not unprecedented in criminal record hiring experiments that employ the opt-in survey design (see Table 1; DeWitt & Denver 2020; Sugie et al. 2020). We discuss this finding at a later point.

The marginal effect for a Google hit indicates applicants who have a record that appears in a search engine result are rated roughly one-third point lower on willingness to hire. While statistically significant, in relative terms (relative to a clean Google search result), the estimate represents a 7.7% lower callback rate when the predictions are normed to convert them to approximate callback rates (see the X_G marked in Figure 1). This puts our result for an unofficial criminal record at the small end of estimates of the criminal record penalty from prior studies. But we find it notable that a mere insinuation of a criminal record, which may or may not be confirmed in a criminal background check, nevertheless disadvantages a prospective job applicant.

To tease out the interaction between applicant race and criminal record, we estimate the marginal effect of a criminal record separately among Black and White applicants. In both cases, the marginal effect is negative and highly statistically significant, showing large criminal record stigma for applicants of either race. But it is clear the criminal record penalty is larger in magnitude for White applicants compared to Black applicants, both in absolute and relative terms (recall that Whites also have a lower callback rate than Blacks). When normed, Black applicants with a criminal record are 34.3% less likely to receive a callback than their Black peers without a criminal record, compared to White applicants with a criminal record who are 50.3% less likely to receive a callback than their White peers without a criminal record (see the X_R's marked in Figure 2). Our estimate for Black applicants is aligned with prior criminal record hiring experiments, while our estimate for White applicants is larger than what we would expect from prior studies (see Table 1). The difference in the race-specific marginal effects is statistically significant and is the source of the significant two-way interaction observed in the ANOVA model (see Table 3). The takeaway is that a criminal record is highly stigmatic no matter an applicant's race, but is apparently more stigmatic (significantly so) for a White applicant compared to a Black applicant.

The interaction between applicant race and a Google hit indicates that both Black and White job applicants suffer a similar willingness-to-hire penalty from appearing to have a criminal record in a search engine result. Both the marginal effects and normed differences are very similar (see the X_G's marked in Figure 2). This affirms the lack of an interaction between applicant race and Google hit in the ANOVA model (see Table 3). The implication is that any unofficial insinuation of a criminal history is stigmatic for both White and Black applicants.

To understand the interaction between criminal record and Google hit, we estimate the marginal effect of a Google hit separately for applicants with and without an official criminal record. The estimates are comparable, but slightly larger for applicants with a criminal record. In other words, among those with an official criminal record, an unofficial hit for a record in a Google search result yields even lower willingness to hire compared to a clean Google search result. When the marginal predictions are converted to approximate callback rates, the impact of a Google hit is more than twice as large in the presence of an official criminal record than in its absence (–12.3% vs. –4.9%). While this is a highly suggestive result, we caution from the ANOVA model that the interaction is not statistically significant.[9] In either case, an important takeaway is that even in the absence of an official criminal record, a Google hit is a liability in the hiring context.

By way of summary, these models point to basic conclusions that (1) an official criminal record is highly stigmatic in employment situations, (2) Black job applicants are substantially more favored than White job applicants, (3) an unofficial criminal record in the form of a Google search hit is modestly stigmatic, and (4) White job applicants experience more stigma from an official criminal record than Black job applicants. Furthermore, the impact of a criminal record and Google hit on willingness to hire are additive rather than interactive, although the findings are in a direction suggesting that an official criminal record modestly enhances the willingness-to-hire penalty from a Google hit.

5. DISCUSSION

In 1960, the total US incarceration rate hovered around 184 per 100,000 (Prison Policy Initiative 2023) and more than 2.1 million people were estimated to have a felony conviction (Shannon et al. 2017, in supplementary tables). At that time, Schwarz and Skolnick (1962) documented substantially lower levels of employer interest in hiring a person with a criminal conviction, even when they possessed work experience appropriate for the position—the callback rate was 36% among those with no criminal record versus 4% among those with a

[9] We can instead condition on a Google hit, and estimate the marginal effect of an official criminal record on willingness to hire. When we do so, and convert the marginal predictions to approximate callback percentages, the impact of an official criminal record (vs. no criminal record) is slightly larger in the presence of a Google hit than in its absence (–44.1% vs. –39.4%).

conviction. Even people with acquittals were less likely to receive a favorable response from employers (18%), suggesting any brush with the law was viewed as disqualifying in the labor market (see also Uggen et al. 2014).

The size and footprint of the criminal legal system have grown considerably since 1960. As of 2020, the total incarceration rate stands at 525 per 100,000 (Prison Policy Initiative 2023), and 45 million people are estimated to have a misdemeanor conviction (Craigie et al. 2020). Amidst a rise in the number of people who must contend with a criminal record in employment contexts, our review of prior studies hints at the possibility that labor market stigma has declined modestly as criminal records have become more pervasive (see Figure 1). While in-person audits have given way to online audits and opt-in surveys in recent years, criminal record disparity is highly consistent across different study designs (see Table 1 and the marginal distributions in Figure 1). In our opt-in survey experiment that uses YouGov's quota sampling with a non-probability panel, we affirm and strengthen this long-standing result: applicants with a criminal record communicated through an official background check are significantly less likely to receive a callback at a magnitude similar to prior studies (X_R = –41.7% when normed, as in Figure 1 and Table 4). Thus, we find that the official "mark of a criminal record" is alive and well in the labor market (Pager, 2003).

However, prior audits and employer surveys convey formal criminal records. In the age of the internet, information about criminal records is increasingly available at an employer's fingertips (Lageson 2020). This unofficial "trace of a criminal record," conveyed digitally via Google hit, might be equally stigmatizing as, and is certainly less expensive to obtain, than information about a formal criminal record. Our survey experiment indicates that a Google hit does significantly erode a job applicant's hiring prospects, although the disparity is not as large as that of an official criminal record ($X_G = -7.7\%$ when normed). But interestingly, a Google hit and criminal record do not interact, and instead exert additive and independent effects on employer willingness to hire. This alarming finding means even unconfirmed evidence of a criminal history can limit someone's ability to get a job.

Criminal record hiring experiments frequently include a manipulation for the race of job applicants, compelling Pager (2005, 2007) to observe that Black job applicants with a criminal record suffer "double jeopardy" in low-wage labor markets. Our review of prior studies actually points to similar effects of a criminal record for Black and White applicants (see Table 1). However, a temporal perspective points to mild reduction in criminal record disparity over time among Black applicants, coupled with mild growth in criminal record disparity over time among White applicants (see Figure 2). An implication is that, in 2000, criminal record disparity was larger for Black applicants, consistent with the double jeopardy explanation; but a criminal record hiring experiment designed today might be expected to yield no racial differences in the impact of a criminal record on callback, or possibly even a disparity that is larger for White applicants. Consistent with the latter, in our opt-in survey experiment, we find that the criminal record penalty is significantly larger for White applicants. Note this applies only to a formal criminal record, as the impact of a Google hit on callback does not differ significantly by race.

In several important respects, our experimental study affirms prior research findings of a large (official) criminal record penalty, extends prior research to compare new forms of criminal record signaling by way of an (unofficial) internet search hit, and documents an unexpectedly larger criminal record penalty experienced by White job applicants than that experienced by Black job applicants. We reflect on these findings by drawing from a number of theoretical explanations. These emphasize long-term (1) changes in administrative and statutory law, (2) changes stemming from social movements, (3) changes spurred by technology, and (4) changes in social science methodologies.

Our finding of a large impact of an official criminal record on hiring and a comparatively weaker impact of an unofficial criminal record—in the context of a slow, long-term decline in the magnitude of the criminal record penalty on hiring—aligns with shifts in public policy regarding hiring people with records, such as Ban the Box. A growing number of employers have likely received training against criminal record discrimination or are aware of Fair Chance Hiring provisions. This awareness may alter employer behavior by encouraging them to be more accepting of, or to even overlook, certain criminal record signals at the front end of hiring. Employers may also be more attuned to sorting official from unofficial sources in our information-rich society, putting more weight on state-sanctioned labels rather than other forms of stigma (Sugie et al. 2020).

Our finding of a larger criminal record penalty for White applicants, relative to the size of the criminal record penalty for Black applicants, may be partly explained by recent national conversations around race and the criminal legal system. Our study was fielded in 2021, in the aftermath of George Floyd's murder and the racial justice protests that followed. Some employers may be concerned about how their own hiring practices impact racial justice, leading them to self-report greater willingness to hire applicants of color or to proactively work against race-based discrimination, particularly in a low-stakes opt-in survey setting. Employers do not make decisions in a regulatory and cultural vacuum—decades of organizational sociology has shown how broader legal and cultural shifts directly impact institutional decision-making (e.g. Edelman 1992).

There is also a methodological artifact relevant for understanding minority preference in hiring that we observe. In the few opt-in surveys capable of speaking to racial discrimination, there is no evidence that survey respondents treat Black and White applicants differently, and in fact some indications that the likelihood of callback is slightly (but rarely significantly) higher for Black applicants. There are even a small number of online audits (and one in-person audit) that hint at a minority preference in hiring, so while minority preference is unusual in criminal record hiring experiments, it is not totally unprecedented. Yet it is still puzzling considering the seemingly unequivocal evidence from experimental estimates of racial discrimination in the broader audit literature. For instance, Quillian et al. (2017) show that the level of racial discrimination was unchanged between 1990 and 2015, with White applicants 36% more likely to receive a callback than Black applicants, on average. However, our quantitative summary does not inherently conflict with Quillian et al.'s analysis, which did not include online audits and opt-in surveys, and was not limited to the criminal record context. In our experiment, our inclusion of a background check (even a "clean" one) in all applicant portfolios may have primed respondents to be more reflective about race.

It may also be that our focus on criminal records, race, and willingness to hire simply operate in a different social environment today, particularly given the timing of our experiment in 2021. Studies published after Quillian et al.'s (2017) review suggest the landscape of racial discrimination in hiring might have shifted meaningfully. Germane to the current study is Kirk and Rovira's (2022) recent finding of a reversal of racial discrimination in the aftermath of George Floyd's murder in Minneapolis in May 2020. Prior to Floyd, Black applicants for service-related job openings were 6.6 percentage points less likely than White applicants to receive a callback. In the immediate aftermath of Floyd, but prior to Derek Chauvin's conviction, Black applicants were 4.6 percentage points more likely to receive a callback. This reversal, as judged by an interaction term, was marginally significant. In a recent criminal record audit, (Mobasseri 2019) also reports a race-by-record interaction whereby Black applicants with a criminal record are significantly more likely than White applicants with a criminal record to receive a callback, but only by employers whose establishments have below-average exposure to violent crime. In all other Black-White comparisons, callback behavior favors White job applicants over their Black peers.

Our study is not without limits and should be interpreted with appropriate caution. First, we utilize an opt-in survey panel of self-identified hiring managers, rather than fielding an experimental audit. To participate in the survey experience, respondents simply had to self-report having hired an employee in the last five years, which could also conflate hiring managers and HR personnel. Additionally, the fact that our survey respondents are not facing real-world hiring contexts, such as industry-specific demand for workers or broader labor market conditions, may shape our results. Our study only asked about one industry (hospitality), which limits generalizability, particularly if there are industry-specific racial dynamics to hiring. Relatedly, we did not screen survey respondents for specific hiring experience in the hospitality industry, so their responses may not accurately represent how hiring for a hotel position might operate in the real world.

Our design choices in how to communicate the digital "trace" of a criminal record via text-based Google results (rather than, say, a Google image search that prominently displayed mugshots) may have also impacted our results. While we created the study to test differences between potentially "unreliable" and "reliable" sources of criminal record information, we should note that our decision to show the Google search results at all may have influenced respondents to believe in the veracity of the internet search returns, thus concretizing the effect of the record when communicated as a digital "trace." We also elected to communicate applicant race via a profile photograph provided on the criminal background check that, according to the materials viewed by respondents, was sourced from Facebook. This may have made our respondents overly conscious of the fact that we were measuring employer responses to applicant race.

In fact, many of the shortcomings of our novel survey experiment mirror those of audits more generally: that experiments in this domain often rely on cues to communicate race and criminal record that are not directly reported or can be associated with other characteristics (such as name, age, and socioeconomic status), or that can be interpreted subjectively and in ways that vary over time, culture, and location. Indeed, our analysis of employer reactions to criminal records in Part 1 shows how this response has varied over time. Additionally, the relationship between a callback, interview, or job offer are often confounded in these studies and may not accurately reflect the applicant selection process. Additional ethical concerns for field audits include their necessary use of deception, the absence of a consent process to invite voluntary employer participation in a research study, and the potential drain on employer time from false impressions of the applicant pool. That said, audit studies and opt-in surveys provide a benchmark for each other to better understand the effect of criminal records on employment outcomes. Our Part 1 analysis also offers some assurances regarding concerns of social desirability bias, where the proportion of audited employers or survey respondents across studies respond to criminal records similarly (though baseline rates differ).

We should also point out that criminal record hiring experiments, including ours, have not yet come to terms with implications of endogenous sample selection. There is good evidence that people select themselves non-randomly into job search. This gives rise to a situation where the unobservables that endow one with a higher-than-average likelihood of engaging in job search (e.g., applying for a job or submitting a resume) also give one a higher- or lower-than-average likelihood of callback by an employer (or reported willingness to hire). By conditioning analysis on people who are engaged in job search, for one, studies can inadvertently mistake the impact of a criminal record on job search for the impact of a criminal record on the outcome of the job search (callback or willingness to hire). It is also plausible that the sample selection process differs in important ways by the kind of criminal record—an arrest record with a misdemeanor conviction compared to felony conviction with a sentence of incarceration—and even by the nature of the job search implied by the study design—distribution of a resume directly to an employer compared to use of an intermediary such as a personal recommendation or a job placement center. Even if random assignment of a criminal record conditional on job search alleviates bias concerns, there would still be a question of whether the estimates generalize to any well-defined population.

Our hope is that this study inspires additional research and a closer look at how study design alters our understanding of criminal records and race in hiring. Though online audits and opt-in surveys offer convenience and large samples, they may yield distorted estimates of hiring discrimination at the intersection of criminal records and race. On the other hand, our findings also encourage research that considers the relatively rapid pace of change in the social and policy landscape regarding employment, criminal records, and race. Perhaps a key takeaway of our project is that foundational experimental studies by Pager may have created ripple effects in how employers consider criminal records and race and how public policy should govern their discretion. The lengthy citation to Pager's research in the 2012 EEOC Guidelines represents a clear example of empirical research shaping policy, and some of the trends we identify may reflect a changed world.

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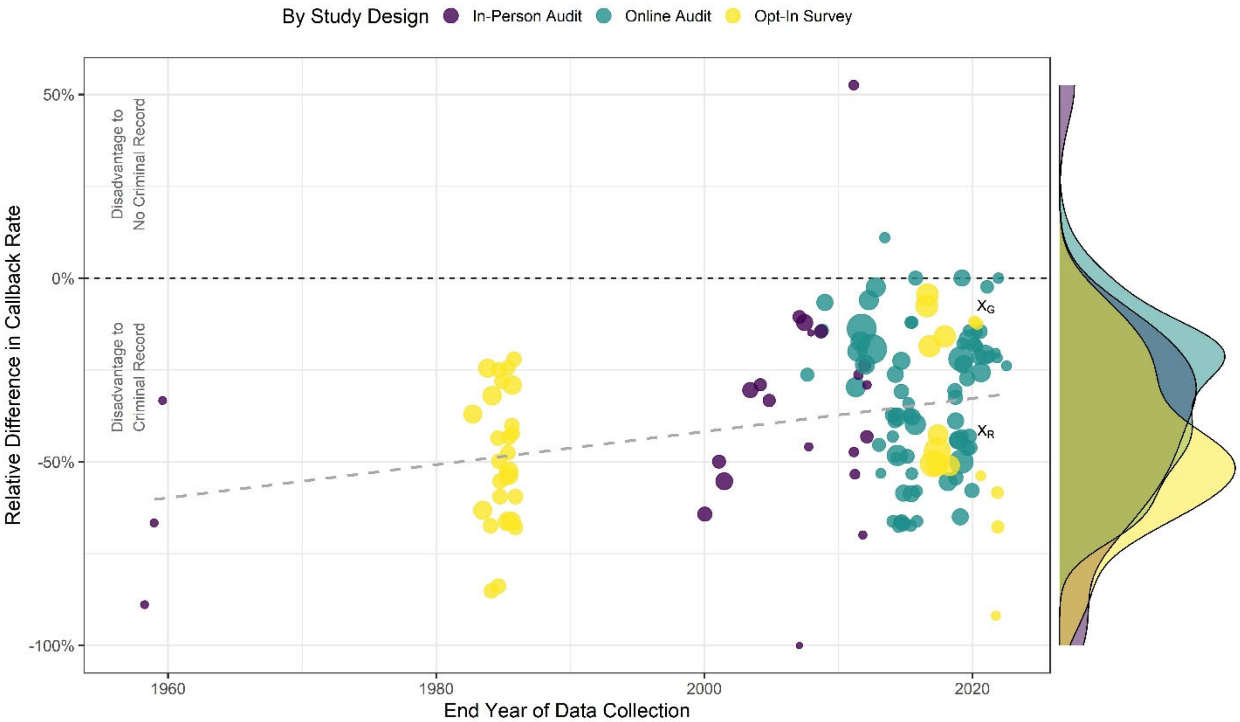
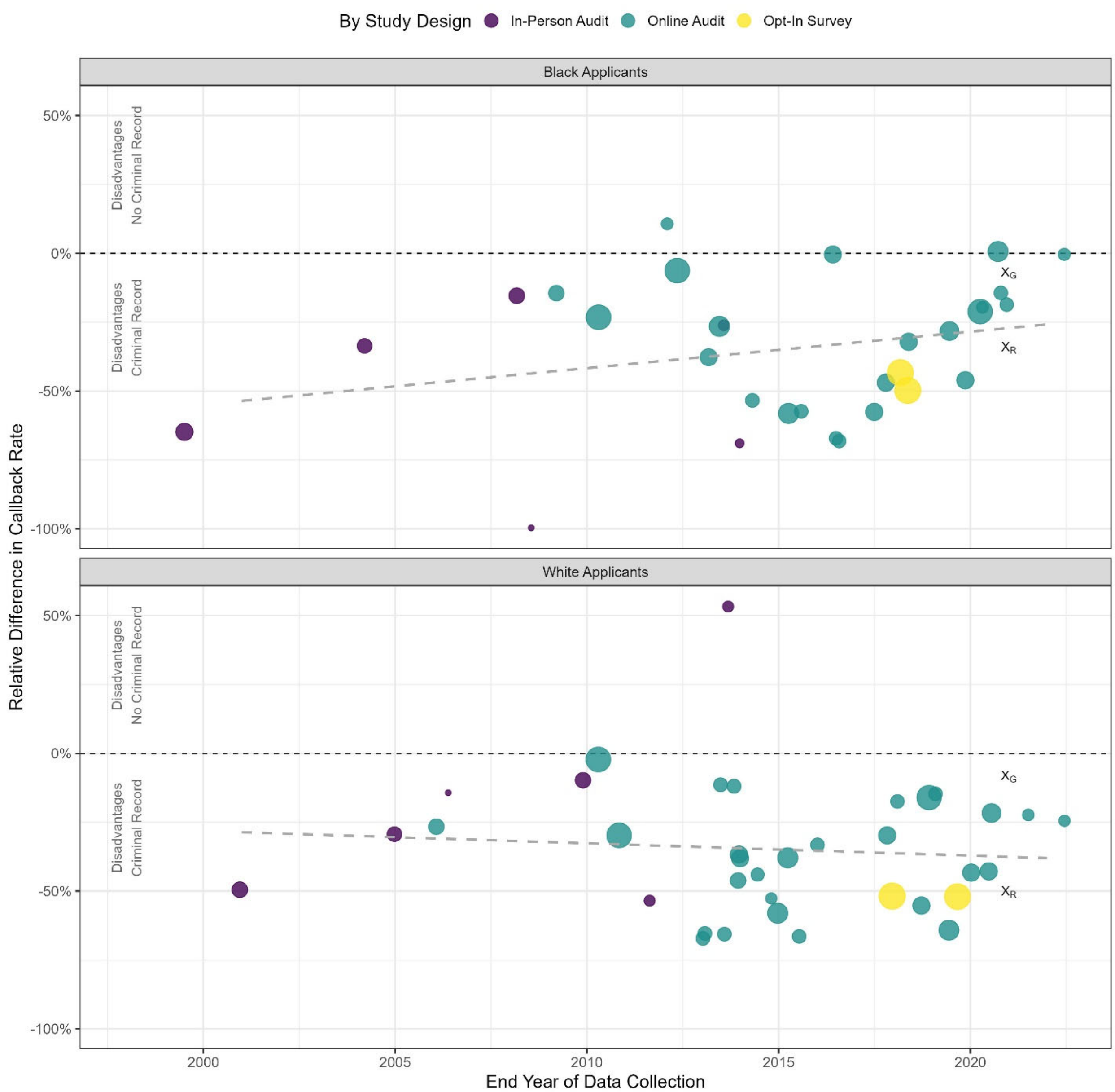
Figure 1
Criminal Record Discrimination in Prior Criminal Record Hiring Experiments, by Year and Study Design



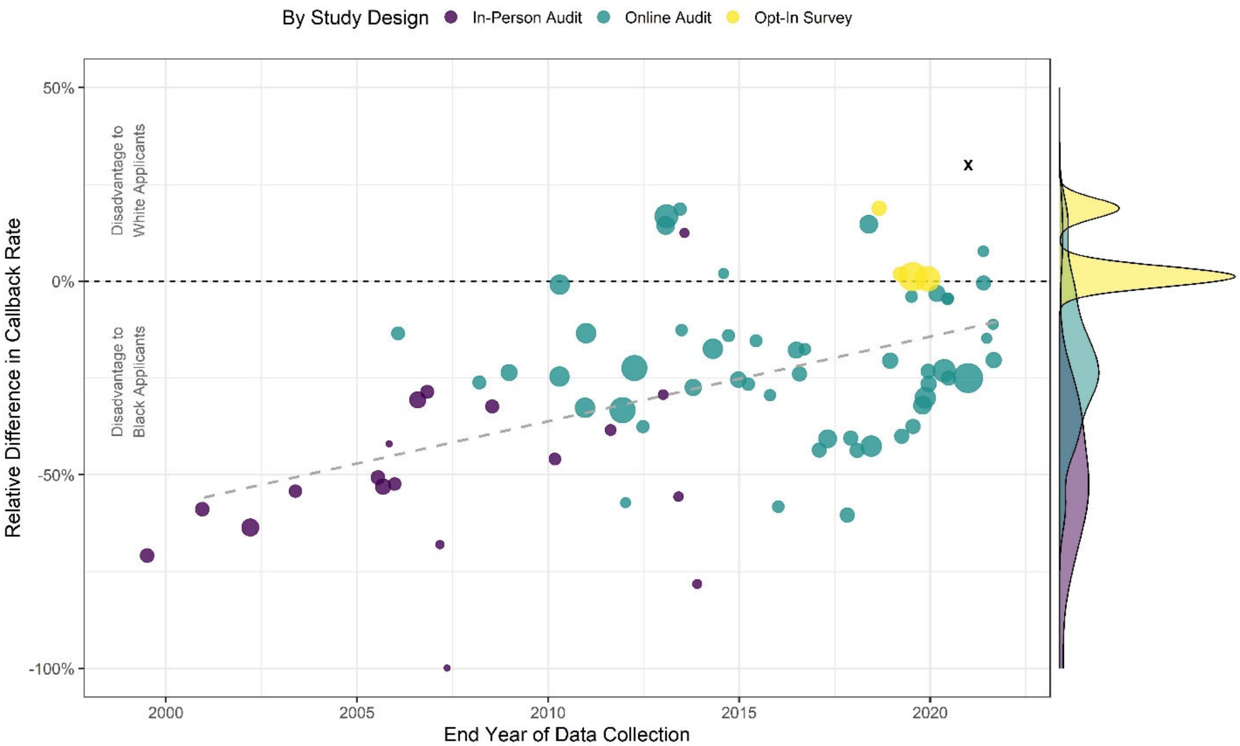
Notes: N = 144. The relative difference in callback rate is shown as a percentage lower or higher likelihood of a callback for someone with a criminal record versus no criminal record. To ensure otherwise equivalent comparisons, callback rates for the criminal record versus no criminal record conditions are limited to applicants of the same sex, race/ethnicity, work experience, and educational credentials. The two Xs mark the corresponding estimates from the current study for a criminal record (X_R = –41.7%) and a Google hit (X_G = –7.7%), to be described in detail at a later point. Data points are jittered to alleviate clustering by year, and weighted by the number of applications, although the regression line is unweighted. For the opt-in surveys, since willingness to hire is measured ordinally, the means are first normed by the range of the ordinal response variable to yield an estimate resembling a callback rate from an audit study: $100 \times (mean - y_{min})/(y_{max} - y_{min})$. Then the percent differences in callback by criminal record are computed.

Figure 2
Criminal Record Discrimination by Applicant Race in Prior Criminal Record Hiring Experiments, by Year and Study Design



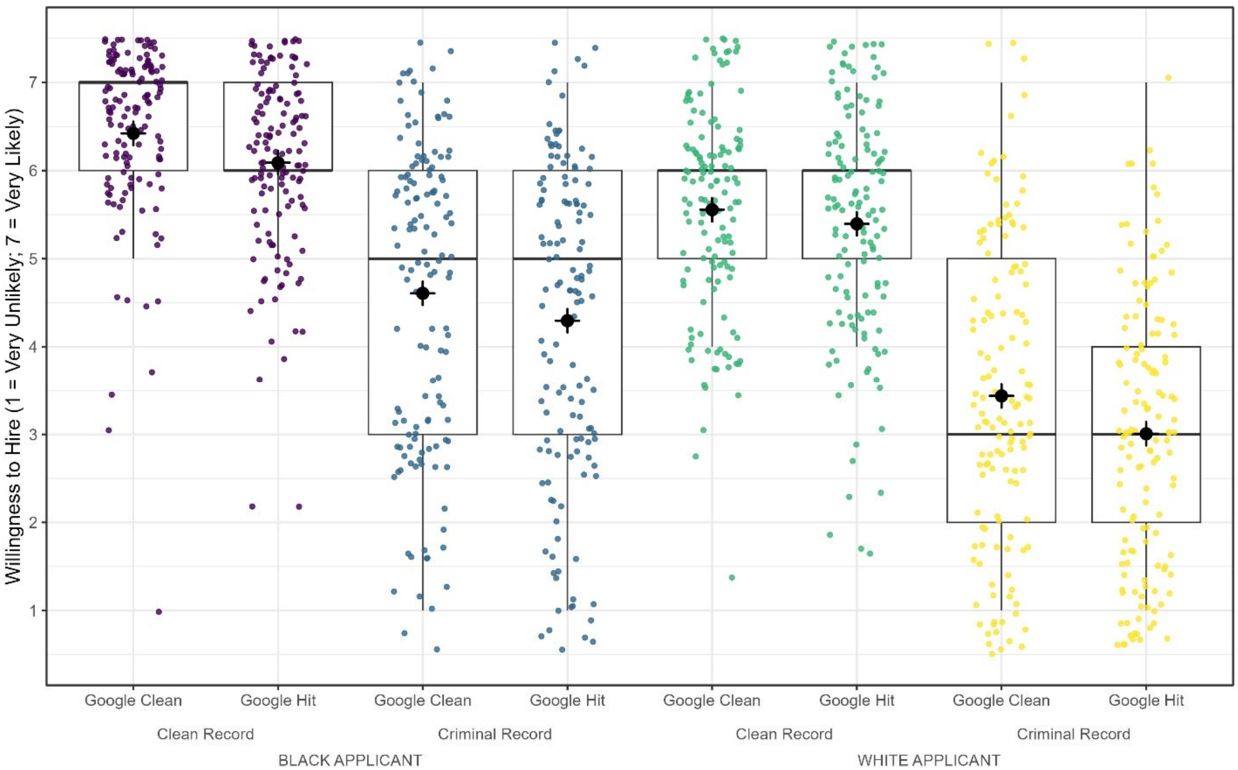
Notes: N (Black) = 31; N (White) = 36. These estimates are a subset of what are shown in Figure 1. The relative difference in callback rate is shown as a percentage lower or higher likelihood of a callback for someone with a criminal record versus no criminal record. To ensure otherwise equivalent comparisons, callback rates for the criminal record versus no criminal record conditions are limited to applicants of the same sex, race/ethnicity, work experience, and educational credentials. The Xs mark the corresponding race-specific estimates from the current study for a criminal record (Black X_R = –34.3%; White X_R = –50.3%) and a Google hit (Black X_G = –7.1%; White X_G = –8.4%), to be described in detail at a later point. Data points are jittered to alleviate clustering by year, and weighted by the number of applications, although the regression line is unweighted. For the opt-in surveys, since willingness to hire is measured ordinally, the means are first normed by the range of the ordinal response variable to yield an estimate resembling a callback rate from an audit study: $100 \times (mean - y_{min})/(y_{max} - y_{min})$. Then the percent differences in callback by criminal record are computed.

Figure 3
Racial Discrimination in Prior Criminal Record Hiring Experiments, by Year and Study Design



Notes: N = 77. The relative difference in callback rate is shown as a percentage lower or higher likelihood of a callback for a Black applicant versus a White applicant. To ensure otherwise equivalent comparisons, callback rates for Black versus White applicants are limited to applicants of the same criminal record condition, sex, work experience, and educational credentials. The X marks the corresponding estimates from the current study (X = +30.0%). Data points are jittered to alleviate clustering by year, and weighted by the number of applications, although the regression line is unweighted. For the opt-in surveys, since willingness to hire is measured ordinally, the means are first normed by the range of the ordinal response variable to yield an estimate resembling a callback rate from an audit study: $100 \times (mean - y_{min})/(y_{max} - y_{min})$. Then the percent differences in callback by applicant race are computed.

Figure 4
Distribution of Willingness to Hire, by Experimental Manipulations



Notes: N = 1,079. Data points are jittered to better visualize distributional features. Black points reference means. Numeric descriptives for the fully crossed manipulations are provided in Appendix A.

Table 1
Summary of Relative Differences in Callback Estimates from Prior Hiring Experiments

Criminal Record Discrimination	# Studies	# Estimates	Unweighted Difference	Weighted Difference
Pooled Estimates: Record vs. No Record	32	144	–38.0%	–35.2%
By Race/Ethnicity				
Black Applicants: Record vs. No Record	22	31	–35.0%	–32.1%
Latino Applicants: Record vs. No Record	6	6	–33.6%	–25.2%
White Applicants: Record vs. No Record	25	36	–34.9%	–36.4%
By Experimental Design				
In-Person Audits: Record vs. No Record	7	23	–38.7%	–34.9%
Online Audits: Record vs. No Record	18	78	–33.6%	–30.4%
Opt-In Surveys: Record vs. No Record	7	43	–45.6%	–42.0%
Black/White Racial Discrimination	**# Studies**	**# Estimates**	**Unweighted Difference**	**Weighted Difference**
Pooled Estimates: Black vs. White	23	77	–25.1%	–20.1%
By Experimental Design				
In-Person Audits: Black vs. White	7	19	–49.4%	–48.9%
Online Audits: Black vs. White	15	54	–18.8%	–19.6%
Opt-In Surveys: Black vs. White	1	4	+5.5%	+2.1%

Notes: In the top panel, callback percentages are selected that share the same race, sex, work experience, and educational credential, and differ only with respect to the presence or absence of a criminal record. In the bottom panel, callback percentages are selected that share the same criminal record condition, sex, work experience, and educational credential, and differ only with respect to applicant race. Weighted mean differences are weighted by the total number of applications comprising the groups being compared. For the opt-in surveys, since willingness to hire is measured ordinally, the means are first normed by the range of the ordinal response variable to yield an estimate resembling a callback rate from an audit study: $100 \times (mean - y_{min})/(y_{max} - y_{min})$. The supplementary material provides detail about the source of these estimates.

Table 2
Descriptive Statistics

Variable	Valid N	Mean (SD)	Min, Max
Manipulations			
Criminal Record = Yes	1,079	50.0%	0 , 1
Applicant Race = Black	1,079	50.0%	0 , 1
Google Hit = Yes	1,079	50.0%	0 , 1
Outcome Variable			
Willingness to Hire (1 = Very Unlikely; 7 = Very Likely)	1,079	4.85 (1.81)	1 , 7
Pretest Variables (Respondent Characteristics)			
Age	1,080	46.79 (9.88)	23 , 66
Sex = Male	1,080	52.4%	0 , 1
Race = Non-White	1,080	21.7%	0 , 1
Marriage = Cohabiting or Never Married	1,080	25.8%	0 , 1
Marriage = Married or Widowed	1,080	64.0%	0 , 1
Marriage = Separated or Divorced	1,080	10.2%	0 , 1
Child(ren) under 18 = Yes	1,080	39.3%	0 , 1
Education = Less than 4-Year Degree	1,080	36.6%	0 , 1
Education = 4-Year Degree	1,080	37.6%	0 , 1
Education = Post-Graduate	1,080	25.8%	0 , 1
Employment = Full-Time	1,080	91.4%	0 , 1
Family Income (1 = <$10,000; 16 = $500,000+)	996	9.01 (3.22)	1 , 16
Industry = Natural Resources, Construction, or Manufacturing	1,080	14.5%	0 , 1
Industry = Trade or Transportation	1,080	10.8%	0 , 1
Industry = Information, Financial, or Professional Services	1,080	20.3%	0 , 1
Industry = Education or Health Services	1,080	21.1%	0 , 1
Industry = Leisure, Hospitality, or Other Services	1,080	13.4%	0 , 1
Industry = Government or Other Industry	1,080	19.8%	0 , 1
Partisanship (1 = Strong Dem; 4 = Indep; 7 = Strong Repub)	1,076	3.33 (2.13)	1 , 7
Voted in Presidential Elections = Yes	1,080	81.9%	0 , 1
Interest in Political Issues (1 = Hardly; 4 = Most of the Time)	1,073	3.52 (0.72)	1 , 4
Religiosity (PC)	1,056	0.00 (1.00)	–1.22 , 1.64

PC = principal component, combining frequency of church attendance (1 = never; 6 = more than once a week), frequency of prayer (1 = never; 7 = several times a day), and importance of religion (1 = not at all; 4 = very important).
Notes: Means of binary variables are shown as percentages.

Table 3
Factorial Analysis of Variance and Effect Sizes

	DV = Willingness to Hire		
Manipulation	F	p	η
Additive Effects:			
Criminal Record	561.86	0.0000	0.59
Applicant Race	137.87	0.0000	0.34
Google Hit	13.00	0.0003	0.11
Two-Way Interaction Effects:			
Criminal Record × Applicant Race	6.90	0.0087	0.08
Criminal Record × Google Hit	0.52	0.4700	0.02
Applicant Race × Google Hit	0.03	0.8729	0.00
Three-Way Interaction Effect:			
Criminal Record × Applicant Race × Google Hit	0.73	0.3941	0.03
Full Model	103.06	0.0000	—

Notes: N = 1,079. Each row is a component of a three-way ANOVA. Effect sizes for each component are eta (η) from the correlation family, for which thresholds of practical significance are 0.1 (small), 0.3 (medium), and 0.5 (large).

Table 4
Marginal Effects from Linear Regression Models of Willingness to Hire

Manipulation	DV = Willingness to Hire ME	SE	p	RI	Normed Diff.
Criminal Record = Yes vs. No	–2.03	0.09	0.0000	0.0000	–41.7%
Applicant Race = Black vs. White	1.01	0.09	0.0000	0.0000	+30.0%
Google Hit = Yes vs. No	–0.31	0.09	0.0003	0.0000	–7.7%
Criminal Record by Applicant Race:					
Criminal Record = Yes vs. No | Applicant Race = Black	–1.80	0.12	0.0000	0.0000	–34.3%
Criminal Record = Yes vs. No | Applicant Race = White	–2.25	0.12	0.0000	0.0000	–50.3%
Google Hit by Applicant Race:					
Google Hit = Yes vs. No | Applicant Race = Black	–0.32	0.12	0.0082	0.0072	–7.1%
Google Hit = Yes vs. No | Applicant Race = White	–0.29	0.12	0.0145	0.0138	–8.5%
Google Hit by Criminal Record:					
Google Hit = Yes vs. No | Criminal Record = No	–0.25	0.09	0.0088	0.0087	–4.9%
Google Hit = Yes vs. No | Criminal Record = Yes	–0.37	0.14	0.0098	0.0096	–12.3%

ME = average marginal effect.
SE = standard error.
RI = p-value obtained via randomization inference with 5,000 draws.
Notes: N = 1,079. Models preserve the 2 × 2 × 2 design, but only the marginal effects of each additive manipulation as well as conditional marginal effects for the two-way interactions are shown. Robust standard errors are provided. The normed difference converts the (conditional) marginal predictions into percentage difference metric, resembling a callback difference as in audit studies. The full set of regression coefficients from which the estimates in this table derive is provided in Appendix B.

Appendix A
Descriptives of Willingness to Hire by Fully Crossed Manipulations

Criminal Record	Applicant Race	Google Hit	N	Willingness to Hire Mean (SD) [Median]	Normed Mean %
No	—	—	539	5.87 (1.16) [6]	81.1%
Yes	—	—	540	3.84 (1.78) [4]	47.3%
—	Black	—	540	5.35 (1.68) [6]	72.6%
—	White	—	539	4.35 (1.80) [5]	55.8%
—	—	No	540	5.01 (1.78) [5.5]	66.8%
—	—	Yes	539	4.70 (1.84) [5]	61.6%
No	Black	—	270	6.26 (0.97) [6]	87.6%
Yes	Black	—	270	4.45 (1.76) [5]	57.5%
No	White	—	269	5.48 (1.21) [6]	74.6%
Yes	White	—	270	3.22 (1.58) [3]	37.0%
—	Black	No	270	5.51 (1.65) [6]	75.2%
—	Black	Yes	270	5.19 (1.70) [6]	69.9%
—	White	No	270	4.50 (1.75) [5]	58.3%
—	White	Yes	269	4.20 (1.84) [4]	53.3%
No	—	No	270	5.99 (1.12) [6]	83.1%
No	—	Yes	269	5.74 (1.20) [6]	79.1%
Yes	—	No	270	4.02 (1.77) [4]	50.4%
Yes	—	Yes	270	3.65 (1.77) [3.5]	44.2%
No	Black	No	135	6.42 (0.93) [7]	90.4%
No	Black	Yes	135	6.09 (0.99) [6]	84.8%
Yes	Black	No	135	4.61 (1.72) [5]	60.1%
Yes	Black	Yes	135	4.30 (1.79) [5]	54.9%
No	White	No	135	5.56 (1.12) [6]	75.9%
No	White	Yes	134	5.40 (1.29) [6]	73.3%
Yes	White	No	135	3.44 (1.62) [3]	40.6%
Yes	White	Yes	135	3.01 (1.50) [3]	33.5%

Notes: N = 1,079. The ordinal willingness-to-hire measure is on a scale from 1 (very unlikely) to 7 (very likely). The normed mean converts the mean of an ordinal variable into a quantity resembling a callback rate from an audit study: $100 \times (mean - y_{min})/(y_{max} - y_{min})$, where $y_{min} = 1$ and $y_{max} = 7$.

Appendix B
Factorial Analysis of Variance of the Pretest Variables

Pretest Variable (Respondent Characteristic)	N	F	p
Age	1,080	1.13	0.3420
Sex = Male	1,080	0.37	0.9203
Race = Non-White	1,080	0.24	0.9739
Marriage = Cohabiting or Never Married	1,080	0.49	0.8432
Marriage = Married or Widowed	1,080	0.66	0.7073
Marriage = Separated or Divorced	1,080	1.34	0.2288
Child(ren) under 18 = Yes	1,080	1.40	0.1996
Education = Less than 4-Year Degree	1,080	0.74	0.6411
Education = 4-Year Degree	1,080	0.77	0.6112
Education = Post-Graduate	1,080	1.41	0.1973
Employment = Full-Time	1,080	1.45	0.1802
Family Income	996	0.26	0.9699
Industry = Natural Resources, Construction, or Manufacturing	1,080	0.41	0.8991
Industry = Trade or Transportation	1,080	1.03	0.4098
Industry = Information, Financial, or Professional Services	1,080	1.16	0.3204
Industry = Education or Health Services	1,080	0.08	0.9993
Industry = Leisure, Hospitality, or Other Services	1,080	1.03	0.4103
Industry = Government or Other Industry	1,080	0.55	0.7935
Partisan Identification	1,076	1.40	0.2018
Voted in Presidential Elections = Yes	1,080	0.64	0.7224
Interest in Political Issues	1,073	2.16	0.0356
Religiosity	1,056	2.70	0.0088

Notes: Each row is the outcome of a three-way factorial ANOVA. To conserve space, only the F-statistics and p-values for the full models are provided, but not for the separate components. Pretest balance is indicated by a non-significant F-statistic.

Appendix C
Linear Regression of Willingness to Hire by Fully-Crossed Manipulations

	DV = Willingness to Hire		
Variable	b	SE	p
Criminal Record = Yes vs. No	–2.12	0.17	0.0000
Applicant Race = Black vs. White	0.87	0.13	0.0000
Google Hit = Yes vs. No	–0.16	0.15	0.2776
Criminal Record × Applicant Race	0.30	0.24	0.2042
Criminal Record × Google Hit	–0.27	0.24	0.2631
Applicant Race × Google Hit	–0.17	0.19	0.3565
Criminal Record × Applicant Race × Google Hit	0.29	0.34	0.3941
Intercept	5.56	0.10	—
Model R-Square		0.40	
Model F		121.09	
Model p		0.0000	

Notes: N = 1,079. This model is the regression equivalent of the three-way factorial ANOVA, with main effects for criminal record, applicant race, and Google hit, as well as two- and three-way for these manipulations. Selected marginal effects from this model are reported in Table 4. Robust standard errors are provided, and it is for that reason the model F-test shown here differs from the model F-test shown in Table 3 (121.09 vs. 103.06). With non-robust standard errors, they are identical.

Supplement 1
Description of Callback Rate Coding from Prior Criminal Record Hiring Experiments

We sought to code raw callback rates from criminal record hiring experiments conducted in the United States, and to record this information in a way that was suitable for descriptive analysis. The callback rate represents the percentage of a group that received any favorable response from a prospective employer. We first recorded the overall callback percentage for the pooled sample when it was either reported directly or could be calculated from subgroup percentages and subgroup sizes. We then recorded callback percentages separately by criminal record subgroups and by race/ethnicity subgroups, although the latter percentages were not available from studies that did not include a test of racial discrimination. We then recorded callback percentages that intersected race/ethnicity and criminal record when that information was available. The bibliography of included and excluded studies is provided in Supplement 2. The estimates and basic information about each included study are provided in Supplement 3.

Studies were identified through Google Scholar searches for combinations of "criminal record," "labor market," "hiring," "audit," and "experiment." We then parsed each article's literature review to identify additional articles, dissertations, research briefs, and final grant reports that reported results from an online audit, in-person audit, or opt-in survey. We finally consulted with several other experts in the field who helped verify the comprehensiveness of our list of criminal record hiring experiments.

Because some studies have multiple criminal record conditions (and even multiple no-criminal-record conditions), we recorded callback rates for all available criminal record and no-criminal record subgroups that shared the same work and educational credentials. We did not wish to compare the callback rates of different criminal record conditions to each other, nor the callback rates of criminal record versus no-criminal-record conditions that possessed distinct work experiences or other credentials. Our goal was to ensure, to the degree possible, that the only difference between the two groups was the presence or absence of a criminal record. For example, Lindsay (2021) designed her study with two no-criminal-record groups—one with an HVAC credential and one without—and two criminal record groups—one with an HVAC credential earned in prison and one without. There are thus four callback rate differences available from this study:

(1) Record/no HVAC vs. no record/no HVAC
(2) Record/no HVAC vs. no record/HVAC
(3) Record/HVAC vs. no record/no HVAC
(4) Record/HVAC vs. no record/HVAC

For our analysis, we use only two of these callback rate differences: (1) and (4). We thus condition on the HVAC credential or its absence, and compare individuals with a criminal record to their counterparts without a criminal record, ensuring they have otherwise identical employment credentials.

Calculation of Callback Differences

The estimate that interest us is not in the callback rate itself, but the difference in callback rates between otherwise equivalent subgroups. To quantify hiring discrimination by a criminal record, for example, we compute the difference in callback rate of individuals with a criminal record as a percentage of the callback rate of their counterparts without a criminal record:

$$100 \times \left(\frac{\bar{Y}_{record}}{\bar{Y}_{no.record}} - 1 \right)$$

Here $\bar{Y}_{record}$ is the callback rate (a percentage) among individuals with a criminal record and $\bar{Y}_{no.record}$ is the callback rate among individuals without a criminal record. These are formed from groups that share the same sex, race/ethnicity, work experience, and educational credential, to ensure the only difference between them is the presence or absence of a criminal record. The resulting quantity is itself a percentage, but one reflecting the percentage difference in callback rates, meaning it may be positive or negative depending on whether a criminal record improves (positive) or worsens (negative) someone's chances in the labor market. We perform this calculation for all available two-way comparisons of individuals with a criminal record to their peers without a criminal record. A study with two criminal record conditions and two race subgroups will thus contribute three estimates: (1) all applicants with a criminal record vs. all applicants without a criminal record, (2) Black applicants with a criminal record vs. Black applicants without a criminal record, and (3) White applicants with a criminal record vs. White applicants without a criminal record.

Because some studies test more than one criminal record condition, there might be more than one estimate of the impact of a criminal record for a particular group. Schwarz and Skolnick (1962) serve as a straightforward example (see Supplement 3). They test three versions of a criminal record relative to no criminal record. The callback percentages reported in the study are 36% for individuals without a criminal record; 4% for individuals convicted and sentenced for assault, 12% for individuals tried and acquitted for assault, and 24% for individuals tried and acquitted for assault with an accompanying letter from the judge certifying a not-guilty finding. The information we record about this study can be seen in the Microsoft Excel screenshot, with the relevant callback rates shown in the last column:

num	study	end.year	design	group	record	race	rec.num	no.rec.num	n.apps	callback
1	Schwarz and Skolnick (1962)	1959	in person	pooled	comb	NA			100	19
1	Schwarz and Skolnick (1962)	1959	in person	no record	no	NA	0	0	25	36
1	Schwarz and Skolnick (1962)	1959	in person	record	yes	NA	1	0	25	4
1	Schwarz and Skolnick (1962)	1959	in person	record	yes	NA	2	0	25	12
1	Schwarz and Skolnick (1962)	1959	in person	record	yes	NA	3	0	25	24

From the information provided in the study, there are three estimates of hiring discrimination based on the formula above: –88.9% from conviction (4% vs. 36%), –66.7% from acquittal (12% vs. 36%), and –33.3% from acquittal with judicial certification (24% vs. 36%). These are all percentage differences in callback relative to the no-criminal-record group, and each is included in the summary table (Table 1 in the main text) as well as in the visualization of criminal record discrimination over time (Figure 1 in the main text). Note that for our purpose, we do not compare the criminal record subgroups to each other.

Pager (2003) provides an example of the information we obtain concerning the intersection of race and criminal record (see Supplement 3). Consider the following screenshot illustrating all available callback rates reported in her study:

num	study	end.year	design	group	record	race	rec.num	no.rec.num	n.apps	callback
2	Pager (2003)	2001	in person	pooled	comb	pooled			700	16.4
2	Pager (2003)	2001	in person	white	comb	white			300	25.5
2	Pager (2003)	2001	in person	black	comb	black			400	9.5
2	Pager (2003)	2001	in person	no record	no	pooled	0	0	350	22.6
2	Pager (2003)	2001	in person	record	yes	pooled	1	0	350	10.1
2	Pager (2003)	2001	in person	no record, white	no	white	0	0	150	34
2	Pager (2003)	2001	in person	record, white	yes	white	1	0	150	17
2	Pager (2003)	2001	in person	no record, black	no	black	0	0	200	14
2	Pager (2003)	2001	in person	record, black	yes	black	1	0	200	5

Note Pager (2003) only reports the four race-by-record percentages shown in the last four rows, so the additional percentages in the spreadsheet reflect our aggregated calculations using those percentages and the relevant number of applications. From this we can see there are three estimates of criminal record discrimination: (1) all applicants with a criminal record to all applicants without a criminal record (10.1% vs. 22.6% ⇒ –55.4%), (2) White applicants with a criminal record to White applicants without a criminal record (17% vs. 34% ⇒ –50.0%), and (2) Black applicants with a criminal record to Black applicants without a criminal record (5% vs. 14% ⇒ –64.3%). Each of these estimates is included in the summary table (Table 1 in the main text) and in the visualization of criminal record discrimination over time (Figure 1 in the main text). The race-specific estimates are also included in the visualization of criminal record discrimination by race over time (Figure 2 in the main text).

To quantify hiring discrimination by race, we use the same formula to compute the callback rate of Black applicants as a percentage of the callback rate of otherwise equivalent White applicants:

$$100 \times \left(\frac{\bar{Y}_{black}}{\bar{Y}_{white}} - 1 \right)$$

Here $\bar{Y}_{black}$ is the callback rate (a percentage) for Black applicants and $\bar{Y}_{white}$ is the callback rate for White applicants. These are formed from groups that share the same sex, criminal record, work experience, and educational credential, to ensure the only difference is applicant race. Although we record callback rate information for Latino applicants in our database, our primary interest is the Black/White callback difference, since it aligns with our study design. The resulting quantity may again be positive or negative depending on whether Black applicants are advantaged (positive) or disadvantaged (negative) in the labor market. We perform this calculation for the full sample (all Black applicants vs. all White applicants) as well as for subgroups possessing the same criminal record signal: Black applicants without a criminal record vs. White applicants without a criminal record, as well as Black applicants with a criminal record vs. White applicants with a criminal record.

Measurement of Callback from Opt-In Surveys

An obvious complication immediately arises when one wishes to compare results from in-person and online audits with results from opt-in surveys. In experimental audits, callback is measured directly by whether or not there is any favorable response from a prospective employer to a job applicant—this includes an offer of hire, an invitation to interview, or some other kind of positive follow-up or request for more information. It is thus a behavioral or “revealed” measure of willingness to hire. In opt-in surveys, because hiring managers are presented with a hypothetical employment portfolio, a behavioral measure of willingness to hire is neither practical nor possible. Rather, these designs present respondents with a Likert scale on which they rate their assessment of willingness to hire or interview the applicant with the credentials portrayed in the portfolio. It is thus a subjective measure of willingness to hire.

To place the means of the ordinal willingness-to-hire measures on a common scale with the mean callback percentages from experimental audits, we norm the former on a 0-100 scale so they resemble an approximate callback percentage. This is the "percent of maximum possible score" (POMP) of Cohen et al. (1999). It is calculated as follows:

$$\bar{Y}_{norm} = 100 \times \left(\frac{\bar{Y} - Y_{min}}{Y_{max} - Y_{min}} \right)$$

We can think of this loosely as a measure of callback likelihood, with the caveat that it is measured subjectively rather than behaviorally. The normed mean for different groups can then be used in the same manner as described in the previous section, to calculate callback differences conditional on criminal record, race/ethnicity, and the intersection of race/ethnicity with criminal record.

We can use results from Finn and Fontaine (1983) to provide an illustration of our approach with opt-in surveys (see Supplement 3). Consider the following screenshot illustrating how we obtain approximate callback rates from this study:

num	study	end.year	design	group	record	race	rec.num	no.rec.num	n.apps	mean	sd	callback
27	Finn and Fontaine (1983)	1983	survey	pooled	comb	pooled			1696	49.99	10	50.0
27	Finn and Fontaine (1983)	1983	survey	no record	no	pooled	0	0	424	58.6		72.6
27	Finn and Fontaine (1983)	1983	survey	record	yes	pooled	1	0	424	51.82		54.8
27	Finn and Fontaine (1983)	1983	survey	record	yes	pooled	2	0	424	48.38		45.7
27	Finn and Fontaine (1983)	1983	survey	record	yes	pooled	3	0	424	41.16		26.7

There are three criminal record conditions in this study: (1) arrested for armed robbery and released ($rec.num = 1$); (2) arrested for armed robbery, tried, and found not guilty ($rec.num = 2$); (3) arrested for armed robbery, tried, found guilty, and served 3 years in prison ($rec.num = 3$). As an aside, the estimates shown in the screenshot are aggregated over four different work experience conditions (also note the authors do not report standard deviations for subgroups). The reported means in this study are ranks (from 1 to 16) that the authors transform to a variable with an observed range of 31-69 (with an overall mean of 50 and standard deviation of 10). Using the lower value of the range, we then calculate the approximate callback rates. Consider the mean of 58.6 for the group with no criminal record:

$$\bar{Y}_{norm} = 100 \times \frac{58.6 - 31}{69 - 31} = 72.6$$

Treating the newly normed means as callback percentages, we can then proceed with computing differences in callback rates of individuals with a criminal record as a percentage of the callback rate of their counterparts without a criminal record, as described previously.

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Supplement 2
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Supplement 3
Details of Prior Criminal Record Hiring Experiments, by Year of Publication

	Study [Design]	Setting [Date]	Employers [Applications]	Record [Disclosure]	Callback [Notes]
1	Schwarz and Skolnick (1962) [in-person audit]	Catskill region, NY [summer 1959]	100 [N = 100]	(0) No criminal record vs. (1) conviction and sentence for assault vs. (2) trial and acquittal for assault vs. (3) trial and acquittal for assault with letter from judge certifying not-guilty finding [criminal record disclosed in an employment folder presented to the employer by a confederate on behalf of the applicant]	Pooled 19%; (0) 36%; (1) 4%; (2) 12%; (3) 24%
2	Finn and Fontaine (1983) [opt-in survey]	Undergraduate seniors enrolled in personnel management course [no date provided]	106 [N = 1,696]	(0) No criminal record vs. (1) arrested for armed robbery and released vs. (2) arrested for armed robbery, tried, and found not guilty vs. (3) arrested for armed robbery, tried, found guilty, and served 3 years in prison [criminal record disclosed in employment applications presented to participants]	Pooled 49.99 / 50.0%; (0) 58.60 / 72.6%; (1) 51.82 / 54.8%; (2) 48.38 / 45.7%; (3) 41.16 / 26.7% [respondents ranked 16 applications from 1 (least employable) to 16 (most employable), and ranks were converted to possess a mean of 50 and SD of 10, with observed range 31-69; %s aggregate over random assignment to work experience credentials]
3a	Finn and Fontaine (1985) [opt-in survey, male applicants]	Undergraduate seniors enrolled in personnel management course [no date provided]	225 [N = 2,250]	(0) No criminal record vs. (1) arrested, tried, and found not guilty vs. (2) arrested, found guilty, and given a 1-year suspended sentence vs. (3) arrested, found guilty, and given a 1-year prison sentence; (x.1) drug possession vs. (x.2) shoplifting vs. (x.3) armed robbery [criminal record disclosed in employment applications presented to participants]	Pooled 49.93 / 49.8%; (0) 65.84 / 89.6%; (1) 55.41 / 63.5%; (1.1) 57.92 / 69.8%; (1.2) 56.89 / 67.2%; (1.3) 51.42 / 53.6%; (2) 47.00 / 42.5%; (2.1) 50.67 / 51.7%; (2.2) 48.80 / 47.0%; (2.3) 41.53 / 28.8%; (3) 42.07 / 30.2%; (3.1) 46.39 / 41.0%; (3.2) 44.50 / 36.3%; (3.3) 35.33 / 13.3% [respondents ranked 20 applications from 1 (least employable) to 20 (most employable), and ranks were converted to possess a mean of 50 and SD of 10, with observed range 30-70]
3b	Finn and Fontaine (1985) [opt-in survey, female applicants]	Undergraduate seniors enrolled in personnel management course [no date provided]	225 [N = 2,250]	(0) No criminal record vs. (1) arrested, tried, and found not guilty vs. (2) arrested, found guilty, and given a 1-year suspended sentence vs. (3) arrested, found guilty, and given a 1-year prison sentence [criminal record disclosed in employment applications presented to participants]	Pooled 50.12 / 50.3%; (0) 66.97 / 92.4%; (1) 55.13 / 62.8%; (1.1) 57.92 / 69.8%; (1.2) 56.58 / 66.4%; (1.3) 50.90 / 52.3%; (2) 47.13 / 42.8%; (2.1) 50.84 / 52.1%; (2.2) 48.51 / 46.3%; (2.3) 42.04 / 30.1%; (3) 42.48 / 31.2%; (3.1) 46.53 / 41.3%; (3.2) 44.97 / 37.4%; (3.3) 35.94 / 14.9% [respondents ranked 20 applications from 1 (least employable) to 20 (most employable), and ranks were converted to possess a mean of 50 and SD of 10, with observed range 30-70]
4	Pager (2003) [in-person audit]	Milwaukee, WI [6/2001 – 12/2001]	350 [N = 700]	(0) No criminal record vs. (1) 18-month prison sentence [incarceration signaled on resume by 12 months out of labor force followed by 6 months employment in a prison industry; when asked, applicant admitted to felony cocaine possession w/ intent to distribute and 18 months prison time served; parole officer additionally listed as a reference in criminal record condition]	Pooled 16.4%; (0) 22.6%; (1) 10.1%; (W) 25.5%; (B) 9.5%; (W)(0) 34%; (W)(1) 17%; (B)(0) 14%; (B)(1) 5%
5	Galgano (2009)	Chicago, IL	300	(0) No criminal record vs. (1) prison sentence of unspecified length	Pooled 14.8%; (0) 16.5%; (1) 13.0%; (W) 16.5%; (B) 13.0%; (W)(0) 19%; (W)(1) 14%; (B)(0) 14%; (B)(1) 12%

	[online audit]	[8/2008 – 11/2008]	[N = 600]	[incarceration signaled on resume by 6 months employment in a prison kitchen; when asked, applicant admitted to felony conviction; parole officer additionally listed as a reference in criminal record condition]	[conflicting %s reported in abstract vs. text, so abstract estimates used]
6	Pager, Bonikowski, and Western (2009a) [in-person audit]	New York City [9 months in 2004]	340 [N = 1,020]	(0) No criminal record vs. (1) 18-month prison sentence [incarceration signaled on resume by 12 months out of labor force followed by 6 months employment in a prison industry; when asked, applicant admitted to felony cocaine possession w/ intent to distribute and 18 months prison time served; parole officer additionally listed as a reference in criminal record condition]	*Racial discrimination arm* Pooled 23.8%; (W) 31.0%; (B) 15.2%; (H) 25.1%; *Criminal record arm* (W)(1) 17.2%; (B)(0) 13.0%; (H)(0) 15.4%
7	Pager, Western, and Sugie (2009b) [in-person audit]	New York City [9 months in 2004]	250 [N = 500]	(0) No criminal record vs. (1) 18-month prison sentence [incarceration signaled on resume by 12 months out of labor force followed by 6 months employment in a prison industry; when asked, applicant admitted to felony cocaine possession w/ intent to distribute and 18 months prison time served; parole officer additionally listed as a reference in criminal record condition]	Pooled 22.0%; (0) 28.0%; (1) 16.0%; (W) 26.5%; (B) 17.5%; (W)(0) 31%; (W)(1) 22%; (B)(0) 25%; (B)(1) 10%
8	Wells (2013) [in-person audit]	Milwaukee, WI [5/2007 – 9/2007]	30 [N = 60]	(0) No criminal record vs. (1) 18-month prison sentence [incarceration signaled on resume by 12 months out of labor force followed by 6 months employment in a prison industry; when asked, applicant admitted to felony cocaine possession w/ intent to distribute and 18 months prison time served; parole officer additionally listed as a reference in criminal record condition]	Pooled 28.5%; (0) 37.0%; (1) 20.0%; (W) 43.5%; (B) 13.5%; (W)(0) 47%; (W)(1) 40%; (B)(0) 27%; (B)(1) 0%
9a	Ortiz (2014) [in-person audit]	Phoenix, AZ [summer 2012]	60 [N = 252]	(0) No criminal record vs. (1) 3-year prison sentence [incarceration signaled on resume by 2-1/2 year out of labor force followed by 6 months employment in a prison job; when asked, applicant admitted to felony cocaine possession w/ intent to distribute and 3 years prison time served]	Pooled 16.7%; (0) 18.9%; (1) 14.4%; (W) 14.8%; (B) 9.1%; (L) 22.9%; (W)(0) 11.8%; (W)(1) 18.0%; (B)(0) 13.3%; (B)(1) 4.0%; (L)(0) 30.4%; (L)(1) 16.0% [%s aggregate over random assignment to educational credentials; %s for race-by-record subgroups obtained from Decker et al. (2014)]
9b	Ortiz (2014) [online audit]	Phoenix, AZ [summer 2011 and summer 2012]	515 [N = 3,090]	(0) No criminal record vs. (1) 3-year prison sentence [incarceration signaled on resume by 2-1/2 year out of labor force followed by 6 months employment in a prison job; when asked on application, applicant admitted to felony cocaine possession w/ intent to distribute and 3 years prison time served]	Pooled 7.9%; (0) 8.8%; (1) 7.1%; (W) 9.4%; (B) 6.3%; (L) 8.0%; (W)(0) 11.1%; (W)(1) 7.8%; (B)(0) 7.0%; (B)(1) 5.7%; (H)(0) 8.3%; (H)(1) 7.8% [%s aggregate over random assignment to educational credentials; %s for race-by-record subgroups obtained from Decker et al. (2014)]
10	Uggen, Vuolo, Lageson, Ruhland, and Whitham (2014) [in-person audit]	Twin Cities, MN [8/2007 - 6/2008]	150 [N = 600]	(0) No criminal record vs. (1) arrest for disorderly conduct w/ no resulting charge or conviction [when asked on application, applicant answered "no" to prior felony convictions but admitted to misdemeanor arrest only; if not asked on application, applicant self-disclosed to hiring manager according to a script]	Pooled 31.0%; (0) 33.0%; (1) 29.0%; (W) 36.8%; (B) 25.5%; (W)(0) 38.8%; (W)(1) 34.7%; (B)(0) 27.5%; (B)(1) 23.5%
11a	Decker, Ortiz, Spohn, and Hedberg (2015) [in-person audit]	Phoenix, AZ [10-week period in summer 2012]	57 [N = 266]	(0) No criminal record vs. (1) 3-year prison sentence [incarceration signaled on resume by 2-1/2 year out of labor force followed by 6 months employment in a prison job; when asked, applicant admitted to felony cocaine possession w/ intent to distribute and 3 years prison time served]	Pooled 14.3%; (0) 18.3%; (1) 10.4%; (W) 21.6%; (B) 11.5%; (L) 7.3%; (W)(0) 29.4%; (W)(1) 13.7%; (B)(0) 13.4%; (B)(1) 9.8%; (L)(0) 8.6%; (L)(1) 6.0% [%s aggregate over random assignment to educational credentials]

11b	Decker, Ortiz, Spohn, and Hedberg (2015) [online audit]	Phoenix, AZ [summer 2011 and summer 2012]	518 [N = 3,108]	(0) No criminal record vs. (1) 3-year prison sentence [incarceration signaled on resume by 2-1/2 year out of labor force followed by 6 months employment in a prison job; when asked on application, applicant admitted to felony cocaine possession w/ intent to distribute and 3 years prison time served]	Pooled 7.4%; (0) 8.0%; (1) 6.9%; (W) 7.8%; (B) 6.0%; (L) 8.5%; (W)(0) 8.0%; (W)(1) 7.7%; (B)(0) 6.7%; (B)(1) 5.3%; (H)(0) 9.2%; (H)(1) 7.7% [%s aggregate over random assignment to educational credentials]
12	Cundiff (2016) [online audit]	Raleigh-Durham metro area, NC [2/2014 - 3/2014]	50 [N = 300]	(0) No criminal record vs. (1) drug felony [applicant self-disclosed felony during application process]	(W) 16.0%; (W)(0) 20.7%; (W)(1) 11.3% [%s aggregate over random assignment to educational credentials]
13	Leasure and Andersen (2016) [online audit]	Columbus, OH [5/2015 - 8/2015]	316 [N = 316]	(0) No criminal record vs. (1) 1-year-old felony drug conviction vs. (2) 1-year-old felony drug conviction w/ certificate of qualification for employment [applicant self-disclosed conviction in a statement accompanying resume]	(W) 21.7%; (W)(0) 29.0%; (W)(1) 9.8%; (W)(2) 25.5%
14	Leasure and Andersen (2017) [online audit]	Columbus, OH [5/2015 - 8/2015]	303 [N = 303]	(0) No criminal record vs. (1) 1-year-old felony drug conviction vs. (2) 10-year-old felony drug conviction [applicant self-disclosed conviction in a statement accompanying resume]	(W) 19.5%; (W)(0) 29.0 (W)(1) 9.8%; (W)(2) 19.2%
15a	Agan and Starr (2018) [online audit, NJ]	New Jersey [1/2015 - 2/2015, before Ban the Box]	1,037 w/ box out of 2,864 [N = 1,037]	(0) No criminal record vs. (1) felony conviction for drug crime vs. (2) felony conviction for property crime [applicant self-disclosed conviction on applications submitted to establishments with "the box" before state adoption of Ban the Box]	Pooled 13.8%; (0) 16.4%; (1) 12.7%; (2) 10.2%; (W) 15.1%; (B) 12.4%; (W)(0) 18.8% (W)(1) 11.8% (W)(2) 11.8% (B)(0) 13.9% (B)(1) 13.9% (B)(2) 8.7%
15b	Agan and Starr (2018) [online audit, NYC]	New York City [6/2015 - 8/2015, before Ban the Box]	1,618 w/ box out of 4,381 [N = 1,618]	(0) No criminal record vs. (1) felony conviction for drug crime vs. (2) felony conviction for property crime [applicant self-disclosed conviction on applications submitted to establishments with “the box” before state adoption of Ban the Box]	Pooled 9.2%; (0) 11.8%; (1) 6.1%; (2) 7.1%; (W) 8.5%; (B) 9.9%; (W)(0) 11.1% (W)(1) 6.9% (W)(2) 4.6% (B)(0) 12.6% (B)(1) 5.2% (B)(2) 9.3%
16	Leasure (2019) [online audit]	Columbus, OH [5/2015 - 8/2015]	582 [N = 582]	(0) no criminal record vs. (1) 1-year-old misdemeanor drug conviction vs. (2) 1-year-old felony drug conviction [applicant self-disclosed conviction in a statement accompanying resume]	Pooled 17.2%; (0) 26.8%; (1) 13.8%; (2) 9.0%; (W) 18.8%; (B) 15.6%; (W)(0) 29.0%; (W)(1) 16.5%; (W)(2) 9.8%; (B)(0) 24.8%; (B)(1) 11.6%; (B)(2) 8.1%
17	Mobasseri (2019) [online audit]	Oakland, CA [8/2014 - 12/2014]	184 [N = 368]	(0) No criminal record vs. (1) 18-month prison sentence [incarceration signaled on resume by 18 months work experience in prison; applicant self-disclosed conviction in a statement accompanying resume]	Pooled 24.7%; (0) 30.7%; (1) 18.7%; (W) 28.0%; (B) 17.3%; (L) 29.9%; (W)(0) 38.2%; (W)(1) 17.9%; (B)(0) 16.4%; (B)(1) 18.2%; (L)(0) 39.1%; (L)(1) 20.0%
18	Cerda-Jara, Elster, and Harding (2020) [online audit]	California 6-city metro [no date provided]	600 [N = 1,798]	(0) No criminal record vs. (1) incarceration of unspecified length [incarceration signaled on resume by a 2-year gap in work experience, description of volunteer work connected to prisoner reentry as a “fellow formerly incarcerated” person, and disclosure in cover letter]	Pooled 5.4%; (0) 7.6; (1) 4.3%; (W) 5.8%; (B) 3.0%; (L) 6.4%; (W)(0) 10.2%; (W)(1) 3.7%; (B)(0) 3.8%; (B)(1) 3.9%; (L)(0) 8.5%; (L)(1) 5.4% [%s aggregate over random assignment to timing of receipt of educational credential among incarceration group]

19	DeWitt and Denver (2020) [opt-in survey]	National MTurk sample [4/2018 - 5/2018]	5,822 [N = 5,822]	(0) No criminal record vs. (1) 18-month prison sentence for cocaine possession with intent to distribute vs. (2) 18-month prison sentence for aggravated assault [felony conviction and prison sentence referenced directly on application; prison sentence additionally shown as an 18-month employment gap]	Pooled 4.06 / 76.4%; (0) 4.55 / 88.7%; (1) 2.88 / 46.9%; (2) 2.76 / 43.9%; (W) 4.04 / 76.1%; (B) 4.07 / 76.8%; (W)(0) 4.54 / 88.5%; (W)(1) 2.73 / 43.2%; (W)(2) 2.73 / 43.3%; (B)(0) 4.56 / 88.9%; (B)(1) 3.04 / 51.0%; (B)(2) 2.78 / 44.4 [respondents rated the likelihood of calling applicant for an interview on a scale from 1 (very unlikely) to 5 (very likely)]
20	Kukucka, Applegarth, and Mello (2020) [opt-in survey]	US graduate programs and professional organizations in human resources or related fields [no date provided]	82 [N = 82]	(0) No criminal record vs. (1) felony conviction with 27 months prison time vs. (2) felony conviction with 27 months prison time but conviction overturned and individual exonerated [criminal record disclosed on application in response to a question about felony convictions, with a written explanation of the timing of conviction and release from prison; offense was not specified]	Pooled 4.41 / 68.3%; (0) 4.70 / 74.0%; (1) 4.24 / 64.8%; (2) 4.26 / 65.2% [respondents rated the likelihood of interviewing applicant on a scale from 1 (very unlikely) to 6 (very likely)]
21	Leasure and Andersen (2020) [online audit]	Columbus, OH [5/2015 - 8/2015]	612 [N = 612]	(0) No criminal record vs. (1) 1-year-old felony drug conviction vs. (2) 1-year-old felony drug conviction w/ certificate of qualification for employment [applicant self-disclosed conviction in a statement accompanying resume]	Pooled 18.6%; (0) 27.3%; (1) 8.5%; (2) 18.3%; (W) 21.5%; (B) 15.4%; (W)(0) 29.0%; (W)(1) 9.4%; (W)(2) 25.5%; (B)(0) 25.0%; (B)(1) 8.3%; (B)(2) 10.5%
22	Sugie, Zatz, and Augustine (2020) [opt-in survey]	National Research Now sample [spring 2017]	2,841 [N = 2,841]	(0.1) No criminal record or signal of prior drug use vs. (0.2) no criminal record but Facebook signal of prior drug use (cocaine addiction and rehabilitation) vs. (1) arrest but no conviction for drug possession (felony cocaine possession) along with Facebook signal of prior drug use vs. (2) conviction for drug possession along with Facebook signal of prior drug use [criminal record confirmed by report from court record; work history and conviction dates constructed to allow for a brief incarceration spell, but no explicit statement that applicant had been incarcerated]	Pooled 3.37 / 39.4%; (0.1) 3.69 / 44.8%; (0.2) 3.37 / 39.5%; (1) 3.26 / 37.7%; (2) 3.19 / 36.5% [respondents rated the likelihood of calling back or interviewing applicant on a scale from 1 (extremely unlikely) to 7 (extremely likely)]
23	Leasure and Kaminski (2021a) [online audit, paired testers]	Cleveland, OH [1/2019 - 6/2019]	400 [N = 800]	(0) No criminal record vs. (1) 3-year-old felony drug and theft convictions along with 6-year-old misdemeanor drug conviction w/ certificate of qualification for employment [applicant self-disclosed criminal history via cover letter; drug misdemeanor did not specify sanction, drug felony specified probation, and theft felony specified incarceration]	Pooled 18.8%; (0) 22.3%; (1) 15.3%; (W) 21.3%; (B) 16.3%; (W)(0) 25.1%; (W)(1) 17.4%; (B)(0) 19.4%; (B)(1) 13.1% [race-by-record estimates adjust for control variables]
24	Leasure and Kaminski (2021a) [online audit, unpaired testers]	Cleveland, OH [1/2019 - 6/2019]	1,200 [N = 1,200]	(0) No criminal record vs. (1) 3-year-old felony drug and theft convictions along with 6-year-old misdemeanor drug conviction vs. (2) 3-year-old felony drug and theft convictions along with 6-year-old misdemeanor drug conviction w/ certificate of qualification for employment [applicant self-disclosed criminal history via cover letter; drug misdemeanor did not specify sanction, drug felony specified probation, and theft felony specified incarceration]	Pooled 14.7%; (0) 22.0%; (1) 12.3%; (2) 9.8%; (W) 18.5%; (B) 10.8%; (W)(0) 27.4%; (W)(1) 15.6%; (W)(2) 12.5%; (B)(0) 16.6%; (B)(1) 8.9%; (B)(2) 7.0% [race-by-record estimates adjust for control variables]

25	Leasure and Kaminski (2021b) [online audit]	Cleveland, OH [1/2019 - 6/2019]	400 [N = 800]	(0) No criminal record vs. (1) 3-year-old felony drug and theft convictions along with 6-year-old misdemeanor drug conviction [applicant self-disclosed criminal history via cover letter; drug misdemeanor did not specify sanction, drug felony specified probation, and theft felony specified incarceration]	Pooled 17.1%; (0) 22.0%; (1) 12.3%; (W) 18.5%; (B) 10.8%; (W)(0) 27.1%; (W)(1) 15.4%; (B)(0) 16.9%; (B)(1) 9.1% [race-by-record estimates adjust for control variables]
26	Leasure and Zhang (2021) [online audit]	Cleveland, OH [10/2019 - 4/2020]	600 [N = 600]	(0) No criminal record vs. (1) 4-year-old felony drug conviction vs. (2) 4-year-old felony drug conviction w/ certificate of qualification for employment [applicant self-disclosed conviction in a statement accompanying resume]	Pooled 24.5%; (0) 27.5%; (1) 22.5%; (2) 23.5%; (W) 25.0%; (B) 24.0%; (W)(0) 28.0%; (W)(1) 23.0%; (W)(2) 24.0%; (B)(0) 27.0%; (B)(1) 22.0%; (B)(2) 23.0% [race-by-record estimates adjust for control variables]
27	Lindsay (2021) [online audit]	5 states: CA, TX, GA, OH, NY [4/2020 - 8/2020]	1,502 [N = 3,004]	(0.1) No criminal record w/ HVAC credential vs. (0.2) no criminal record w/o HVAC credential vs. (1) 3-year prison sentence w/ HVAC prison credential vs. (2) 3-year prison sentence w/o HVAC prison credential [when asked on application, applicant admitted to felony conviction]	Pooled 22.2%; (0.1) 27.3%; (0.2) 24.2%; (1) 21.3%; (2) 18.0%; (W) 25.3%; (B) 19.2%; (W)(0.1) 30% (W)(0.2) 28% (W)(1) 25% (W)(2) 22% (B)(0.1) 24% (B)(0.2) 22% (B)(1) 19% (B)(2) 16% [race-by-record estimates adjust for control variables]
28	Ripper (2022) [online audit]	Prince William County, VA [9/2021 - 2/2022]	398 [N = 398]	(0) no criminal record vs. (1) prison sentence of unspecified length w/ reentry work certificate vs. (2) prison sentence of unspecified length w/o reentry work certificate [incarceration signaled on resume by 18 months employment in a prison job; reentry work certificate conveyed on resume as voluntary completion of 8-week career readiness and job skills program]	Pooled 39%; (0) 42% (1) 33%; (2) 41%; (W) 39%; (B) 39%; (W)(0) 46%; (W)(1) 35%; (W)(2) 36%; (B)(0) 39%; (B)(1) 31%; (B)(2) 39%
29	Santos, Jaynes, and Thomas (2023) [opt-in survey]	National Qualtrics panel [9/2021 - 10/2021]	591 [N = 2,364]	(0) No criminal record (person A) vs. (1) conviction for drug possession with intent to distribute 1 year ago (person B); A/B testing design where hiring managers were forced to choose between the two candidates in order to determine at what point (e.g., at what starting wage) they were indifferent to a criminal record [criminal record disclosed directly as a prior conviction]	(0.1) 0.71 / 70.6%; (0.2) 0.76 / 75.6%; (0.3) 0.93 / 92.5%; (0.4) 0.68 / 68.4%; (1) 0.29 / 29.4%; (2) 0.24 / 24.4%; (3) 0.08 / 7.5%; (4) 0.32 / 31.6% [in four different arms of the study with distinct experimental manipulations, %s are limited to applicants with the same educational attainment, reference letter, requested wage, and work experience; given the forced choice design, %s choosing person A and person B are complements, conditional on the same credential]

Note: Only studies conducted within the United States are included and summarized here. Studies including more than one arm (e.g., both an in-person audit and online audit) are summarized in separate rows, as are studies reporting separate results for male and female applicants (e.g., Finn and Fontaine, 1985) or separate results by jurisdiction (e.g., Agan and Starr, 2018). Criminal record conditions include (0) no criminal record and (1) criminal record. When there are additional criminal record conditions (e.g., Schwarz and Skolnick, 1962), they are assigned higher integers than 1. When there are additional no-criminal-record conditions (e.g., Lindsay, 2021), they are assigned 0s with decimal points to clarify the same-credential comparison from the criminal record conditions. Race/ethnicity subgroups include (W) White applicants, (B) Black applicants, and (L) Latino applicants. For opt-in surveys, both the mean of the willingness-to-hire outcome is reported, as well as the mean normed on a 0-100 scale so it resembles an approximate callback percentage.