CRIMINOLOGY

THE EDGE OF STIGMA: AN EXPERIMENTAL AUDIT OF THE EFFECTS OF LOW-LEVEL CRIMINAL RECORDS ON EMPLOYMENT*

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Ample experimental evidence shows that the stigma of a prison record reduces employment opportunities (Pager, 2007). Yet background checks today uncover a much broader range of impropriety, including arrests for minor crimes never resulting in formal charges. This article probes the lesser boundaries of stigma, asking whether and how employers consider low-level arrests in hiring decisions. Matched pairs of young African American and White men were sent to apply for 300 entry-level jobs, with one member of each pair reporting a disorderly conduct arrest that did not lead to conviction. We find a modest but nontrivial effect, with employer callback rates about 4 percentage points lower for the experimental group than for the matched control group. Interviews with the audited employers suggest three mechanisms to account for the lesser stigma of misdemeanor arrests relative to felony convictions: 1) greater employer discretion and authority in the former case; 2) calibration of the severity, nature, and timing of the offense; and 3) a deeply held presumption of innocence, which contrasts the uncertainty of arrest with the greater certainty represented by convictions. In addition, personal contact and workplace diversity play important roles in the hiring process.

The presumption of innocence has been a cornerstone of Anglo-American criminal law since the eighteenth century, reflected in the dictum "innocent until proven guilty." Yet criminal records can haunt the accused, as well as the convicted. As comprehensive background checks have become standard practice in hiring, rising numbers of low-level arrestees are correspondingly subject to their stigmatizing effects. In Erving Goffman's

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(1963) terms, they have gone from potentially "discreditable" to formally "discredited" as applicants.

Arrest records have long been available to anyone with the time and inclination to seek them, but a tectonic shift has occurred in their accessibility (Bushway, Stoll, and Weiman, 2007; Raphael, 2010). What once required a trip to the courthouse and the better part of an afternoon is now accomplished with a few keystrokes or a nominal fee to a private firm. As a result, everyday citizens, employers, and landlords now routinely consult criminal databases. These background checks can be consequential, as felony criminal records clearly reduce employability (Apel and Sweeten, 2010; Bushway and Apel, 2011; Pager, 2007; Western, 2007). Because background checks typically unearth information on arrests as well as convictions, however, *even people never charged with a crime* may still bear the mark of a criminal record.

Yet we know little about the far-flung effects of arrest records and "misdemeanor justice" (Kohler-Hausmann, 2013). Are arrests for relatively minor transgressions overlooked, or do they effectively disqualify applicants? On the one hand, any negative signal could hinder an applicant's prospects. On the other hand, employers are likely becoming more discerning in evaluating criminal records, such that they may discount an otherwise-qualified applicant's brush with the law. Emile Durkheim (1895: 100) famously imagined a society of saints, where "crime" was unknown but trivial or venial faults aroused scandal. Although the United States is no society of saints, the question before us today is whether the routine public disclosure of low-level arrest data arouses scandal sufficient to bar the door to employment.

This article first reviews research establishing 1) that a rising proportion of firms are routinely checking applicant backgrounds and 2) that employers discriminate on the basis of felony-level records. We then ask the following scientific question with pressing policy implications: Does *low-level arrest* information affect hiring, and if yes, then how does it affect hiring decisions? We use an experimental audit method to estimate the impact of a single arrest on employment, and then we interview a subset of audited employers to learn precisely how they incorporate low-level records in their decisions.

PROLIFERATION OF BACKGROUND CHECKS

It is normative for firms to conduct background checks, with 60 percent of California employers indicating that they always check the criminal backgrounds of applicants and another 12 percent reporting they sometimes check (Raphael, 2010). A Society for Human Resource Management survey (2010) puts the figure even higher, with 73 percent reporting checks for all positions and 19 percent reporting checks for selected positions. Apart from new technologies that have made the checks cheap and easy, this high prevalence can be explained by employer fears of liability (Finlay, 2009), confusion over legal responsibility (Bushway, Stoll, and Weiman, 2007; U.S. Equal Employment Opportunities Commission [U.S. EEOC], 2012), a desire to avoid high monitoring costs (Finlay, 2009), and simple distrust of those with criminal backgrounds (Bushway et al., 2007).

Employers can access criminal history information using public court records, Internet searches, and private data harvesting companies, each of which has become more accessible in the past decade. According to one survey, 28 states allow direct Internet access to criminal records (Mukamal and Samuels, 2003). Although the use of public and private databases is soaring, there is little consistency in how such databases are compiled and

the content of the records therein. Some public sources only show criminal convictions, but private firms often report arrests for misdemeanors that result in dismissal (Bushway et al., 2007). Historically, low-level offenses have been assembled only at the state and local levels, although the federal government has considered expanding national databases to include crimes such as "vagrancy, urinating in public, public intoxication, [and] public disturbances" (Palazzolo, 2006). The time an offense should remain on record also is debated, as the likelihood of a new offense among former offenders eventually approximates that of the general population (Blumstein and Nakamura, 2009; Kurlychek, Brame, and Bushway, 2006).

States also vary in the extent to which employers are legally permitted to consider arrest records in hiring decisions. Mukamal and Samuels (2003) reported that 38 states permit both public and private firms, as well as occupational licensing agencies, to access and consider arrest records that did not result in a conviction. More recently, the U.S. EEOC (2012) issued an enforcement guidance stating that employers who use arrest records as the sole grounds for exclusion may be in violation of Title VII of the 1964 Civil Rights Act.[1] Nevertheless, as private repositories continue to disseminate arrest information, the extent of employer adherence to such guidelines remains unknown.

DISCRIMINATION, AUDITS, AND RACE

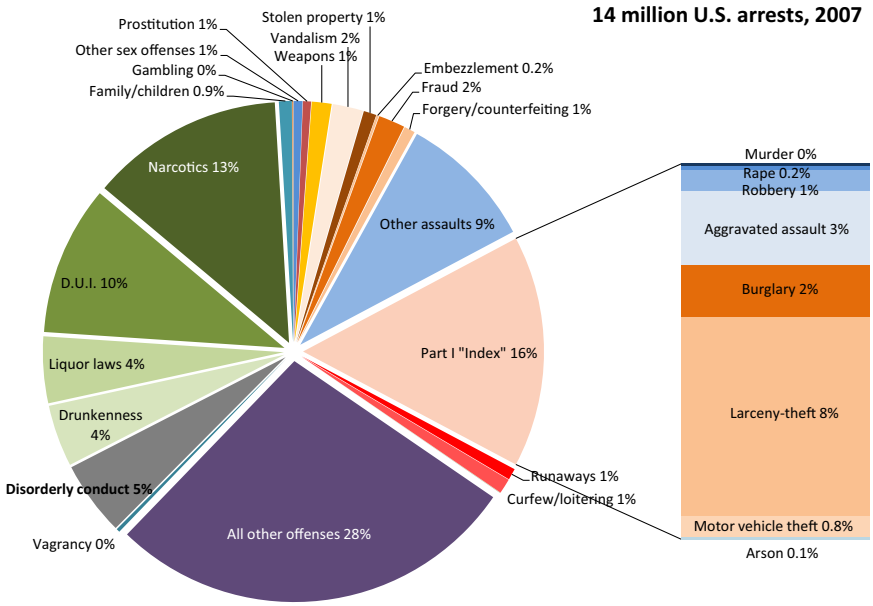
The powerful audit studies by Devah Pager (2003, 2007) have firmly established that employers discriminate on the basis of felony-level prison records, reducing the likelihood of a favorable employer "callback" by 50 percent for Whites and more than 60 percent for African Americans. Felony records are thus linked to employment prospects, which, in turn, are likely to affect subsequent crime (Sampson and Laub, 1990; Uggen, 2000) and broader patterns of inequality (Wakefield and Uggen, 2010). An estimated 12.8 percent of the U.S. adult male population has been convicted of a felony at some point in their lives (Uggen, Manza, and Thompson, 2006), with far higher rates among African Americans (Western, 2006).

Yet millions more enter the leaky funnel of criminal justice system processing via arrests (Kohler-Hausmann, 2013). Using self-reported arrest data from a national survey, Brame et al. (2012) estimated that 30 percent of U.S. youth are arrested by 23 years of age—a sizeable increase over the 22 percent estimate derived from earlier cohorts. And this risk is unevenly distributed by race and sex, with approximately 49 percent of African American males, 44 percent of Hispanic males, and 38 percent of White males arrested by 23 years of age (Brame et al., 2014).

Although 1.6 million individuals were held in U.S. prisons in 2010, the number of arrests was more than eight times higher in that year—approximately 13.1 million (U.S. Department of Justice, 2008). Only approximately 16 percent were arrested for serious Part I crimes, as figure 1 shows for 2007. Of those remaining, the most common arrests involved drugs and alcohol (31 percent), other assaults (9 percent), and

1. Under the 2012 guidance, employers may still make job decisions based on the conduct underlying an arrest if the conduct makes the individual unfit for the position in question. In Minnesota, the site of the current investigation, employers may view arrest information in public and private databases, but state law prohibits blanket discrimination on the basis of arrests only (see M.S. §364 [1974]; M.S. §609B [2005]; M.S. §245C.15 [2005]).

Figure 1. Offense Distribution of Arrest in the United States



Source: Federal Bureau of Investigation, 2008.

disorderly conduct (5 percent), the latter of which plays an important part in the experiment to follow. Before the advent of electronic databases and widespread background checks, arrest histories were far less visible. With the proverbial cat now out of the bag, even minor arrest records constitute highly visible, easily accessible, and virtually indelible marks of social dishonor. Following Goffman (1963), arrestees can no longer "pass" as normal, and this stigma colors their interactions with employers and others. Indeed, merely being arrested or stopped by the police seems to affect peer interactions, identity, and behavior (Wiley, Slocum, and Esbensen, 2013).

Surprisingly little research has directly addressed the employment effects of low-level records, although arrest is associated with long-term joblessness among young men (Grogger, 1992). Previous investigations found that employers attend to distinctions between arrest and conviction, as well as offense types (Boshier and Johnson, 1974; Buikhuisen and Dijksterhuis, 1971; Schwartz and Skolnick, 1962). Such results are consistent with signaling models (Bushway and Apel, 2011; Spence, 1973), in which employers interpret arrests as indicating low worker quality. Similarly, economic theories of statistical discrimination (Arrow, 1973; Phelps, 1972) suggest that employers will exclude arrestees based on prevailing stereotypes about the average productivity of persons with criminal records. Sociological accounts of discrimination also have pointed to stereotyping, although these have tended to emphasize organizational and structural factors rather than perceived productivity differences (Pager and Shepherd, 2008; Reskin, 2012).

Audit studies have revealed strong discrimination on the basis of race as well as criminal history, such that African American applicants with "clean" backgrounds fare no better

than Whites just released from prison (Pager, Western, and Bonikowski, 2009). Given race differences in the prevalence of criminal records, employers in more diverse firms are likely more experienced in reading criminal records and recognizing the low-level arrests. Consistent with this idea, one employer survey found a significant correlation between hiring African American men and the willingness to consider applicants with criminal histories (Raphael, 2010: table 5).

Although firms discriminate on the basis of prison records, jobseekers can mitigate this discrimination through personal contact. Once hired, workers can influence employer beliefs about themselves and those with similar characteristics (Altonji and Pierret, 2001; Pager and Karafin, 2009). As Pager (2007: 103) noted, applicants who meet face-to-face with hiring authorities replace broad generalizations based on group membership with more nuanced information about their individual qualities. Jobseekers who make direct contact are thus much more likely to be called back by employers, who may wish to provide a "second chance" to an otherwise promising applicant (Pager, Western, and Sugie, 2009: 206). In addition, such personal contact may weaken race effects on employment (Pager, Western, and Bonikowski, 2009: 784), although African Americans may have significantly less access to hiring authorities than Whites (Pager, Western, and Sugie, 2009).

As for employers, one survey found minority-owned businesses to be four times as likely to express willingness to hire ex-offenders (Pager, 2007: 129). Experience in reading and interpreting criminal records may help explain this effect—African Americans, in particular, are more likely than Whites to have vicarious exposure to incarceration, which reduces punitive attitudes (Johnson, 2007; Rose and Clear, 2004) and lessens the associated stigma (Hirschfield and Piquero, 2010). They may therefore look more closely at the applicant's individual circumstances and, perhaps, provide a clear-eyed (if not sympathetic) reading of the record. If so, then employers of color and those hiring from a more diverse applicant pool may be especially likely to discount arrests without convictions and petty offenses. Alternatively, previous literature suggested that employers of color may be as likely as White employers to discriminate on the basis of criminal records (Kirschenman and Neckerman, 1991; Wilson, 1996).

Although this line of research has developed rapidly, we know little about a question affecting millions of jobseekers: How do *minor* crimes—the "venial faults," now codified as low-level records—affect employment prospects? Do firms disqualify applicants based on the negative signal sent by a criminal record, regardless of its severity? Or, is arrest now so common that employers brush it aside? Alternatively, might honestly divulging a brush with the law actually improve one's prospects? Such would be the case if employers made the statistical discrimination assumption that applicants (particularly African American males) who do not disclose misdemeanor arrests are actually harboring more serious criminal records.

We test these ideas using an experimental audit methodology and face-to-face employer interviews. After describing our design, we present results showing the effect of a misdemeanor arrest record, race, and personal contact on employer callbacks. We then analyze interviews with a subset of these employers to learn precisely how they use background checks in hiring and the mechanisms linking criminal records to employment outcomes. Finally, we engage scientific and policy debates about variation in stigma and public access to criminal records.

STUDY DESIGN

For our experimental audit, young male "testers" applied for entry-level jobs using fictitious identities (see appendix A in the online supporting information for the study protocol).[2] Testers were grouped into pairs by race and selected for their shared physical and personal characteristics. Each week, one tester per pair was assigned to the treatment condition: a sole arrest for disorderly conduct, with no resulting charge or conviction. We chose this offense because it consistently ranks among the *least* serious crimes that would be familiar to nonexperts (Figlio, 1975; Rossi et al., 1974), prohibiting "offensive, obscene, abusive, boisterous, or noisy conduct" (M.S. §609.72). Disorderly conduct arrests rank sixth most prevalent in the state in which the experiment was conducted, trailing only driving under the influence, larceny-theft, assault, and liquor and drug violations. Unlike these offenses and more serious misdemeanors (e.g., domestic assault), disorderly conduct implies neither bodily injury, nor substance use, nor theft. A single disorderly conduct arrest should thus represent a minimally stigmatizing record.

From August 2007 to June 2008, each pair submitted close to 300 applications at 150 job sites, with each tester assigned to the treatment condition for half the audits. This process allowed us to determine the likelihood of an employer callback for each condition-race pairing (White control, White treatment, African American control, and African American treatment).[3] Four male college students in their early 20s were selected as testers and assigned a detailed biography and résumé reporting high-school education, steady employment in service industry and labor positions, and no special training or certifications (see the online supporting information). Because each pair applied to a different set of jobs, only two individualized biographical backgrounds were needed, eliminating differences within and between tester pairs. Our fabricated biographies were similar in every regard, except the rotating treatment condition and race cues (e.g., participation in an African American student group).

Each Sunday, we reviewed the job postings in the classified ads from five print sources (*Minneapolis Star Tribune, St. Paul Pioneer Press, Employment News, Employment Guide,* and *JobDig*) and one online source (*Craigslist*). For the online query, only jobs posted in the preceding five days were reviewed. All entry-level advertisements were selected, so long as they required no special skills or licenses, instructed applicants to apply in person, and were located in the seven-county Twin Cities metropolitan area. Advertisements were cross-referenced with a database of completed audits to ensure that no location was audited more than once and simple randomization was used to allocate the jobs among the tester pairs. On average, testers conducted seven audits per week.

2. Additional supporting information can be found in the listing for this article in the Wiley Online Library at http://onlinelibrary.wiley.com/doi/10.1111/crim.2014.52.issue-4/issuetoc.

3. We did not send White and African American testers to the same employer because the power to detect a difference would have diminished precipitously if the pairs were further divided into the four groups necessary to conduct such an experiment. Because our primary research interest concerns the potentially modest treatment effects of low-level records, we designed the study to maximize the power to detect these effects (see footnote 7). The absence of interracial prospective employer covariance in this design, however, means that our results can only provide indirect evidence on the question of racial discrimination (cf. Pager, Western, and Bonikowski, 2009).

CONVEYING THE TREATMENT

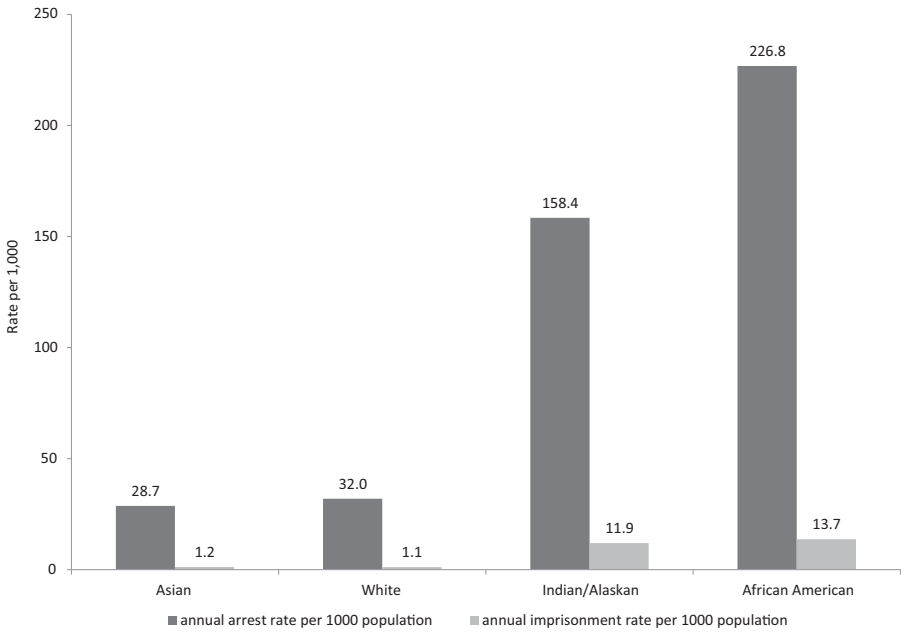
Before fielding the study, we held focus groups with men who had experience conveying their own criminal records. Consistent with advice from local job coaches, they advised our team to disclose criminal history information in full on application forms. If any question about such disclosure were to arise, then they advised testers to explain their record as "something that has come up before." With most firms now checking records, such disclosures have become routine. Eighty percent of the application forms in our sample contained specific criminal history questions, although their wording varied considerably. Testers were instructed to respond directly to each question and to add any information needed to convey the status of misdemeanor arrest. For instance, testers would respond "no" if asked about felonies, but they would then write "misdemeanor arrest only, 2005." This method was pretested with local hiring authorities to ensure that the record was conveyed in a realistic manner that would not arouse suspicion.

After arriving at each establishment and submitting their applications and résumés, the testers were instructed to maximize personal contact by asking to speak to the manager. We documented all such contact, allowing for analysis of its effects. Personal contact (and the résumé cues noted earlier) also helped ensure that race was conveyed to the hiring authority. When no criminal history question appeared on the application, the tester communicated the arrest in person to the hiring authority, following standard scripts developed in consultation with our focus group participants. They first asked questions about the job's pay and hours to express genuine interest in the position, before saying, "I was arrested but never convicted of a misdemeanor offense, it was minor and stupid on my part, and I wanted to be upfront about it in case it came up in a background check." If probed for more information, the testers would respond, "I was arrested for a disorderly conduct misdemeanor in August 2005, but I was never convicted. I was downtown late one night. Some friends got into a fight and I got pulled into it. Nobody was actually hurt. I just acted irresponsibly, but I was young and that's all in the past." This script was used whenever testers were asked about their criminal records, which sometimes occurred as they submitted applications. Because they had rehearsed this script repeatedly in training and mock interviews, testers responded naturally to such questions. Before leaving the job site, they asked for an extra application to give to a friend or a business card. These were used to arrange follow-up interviews with employers and to document that testers had applied at the appropriate location. After each audit, testers immediately detailed their experience in a four-page tester response form and debriefed with research staff.

CALLBACKS, INDEPENDENT VARIABLES, AND THE MINNESOTA SETTING

As in previous employment audits (Pager, 2007), our primary dependent variable is the "callback." A callback is a tangible positive response from an employer—an on-site job offer, an on-site offer for an interview, a voicemail job offer or interview request, or a call from an employer for something beyond a reference or request for basic information. Callbacks were tracked for 4 weeks after the final audit for each pair. We recorded other independent and control variables to help interpret treatment effects and for covariate adjustment on factors that might influence these effects. These variables include the order in which testers applied (first or second), the advertising source, when the test occurred, whether it occurred in Minneapolis-St. Paul or a suburb, and the monthly unemployment

Figure 2. Minnesota Annual Arrest and Imprisonment Rate per 1,000 by Race, 2007



Source: Minnesota Department of Public Safety, 2008.

rate. The response form also gathered critical information on personal contact with the hiring authority and workplace diversity (whether either tester observed persons of color working in the establishment, coded dichotomously). Table A.2 in the online supporting information shows descriptive statistics for these covariates. Although characteristics of the job site (i.e., advertising source, location, industry, and diversity) are subject to the randomization process (attested to by their lack of covariation with the treatment effect noted below), other covariates are less amenable to randomization resulting from other factors, such as the timing of the ad (i.e., monthly unemployment and test order) or the time of day the tester applied (i.e., contact with the hiring authority). Even though it is typical to include such measures in an experimental context, we note that such covariates are observational rather than experimentally manipulated.

By national standards, the Minneapolis-St. Paul area is characterized by relatively low unemployment and low incarceration rates. Nevertheless, its racial disparities in both spheres are among the highest in the nation. Figure 2 shows Minnesota's annual arrest and incarceration rates by race. The disparities are great by each measure, but the rate of arrest is strikingly high for some groups. For African Americans, the annual arrest rate is a staggering 227 per thousand—seven times the White rate of 32 per thousand.[4] These disparities are driven in part by differences in neighborhood context (Kirk, 2008); in poor and working-class African American communities, social control on this scale profoundly

4. Note that the same individuals can be arrested multiple times in a given year.

affects daily life (Goffman, 2009). Without question, arrest records are commonly encountered by hiring authorities in Minnesota—particularly those whose applicant pool extends beyond Whites and Asian Americans.

EMPLOYER INTERVIEWS

After the audits, we conducted face-to-face interviews with a subset of the hiring authorities. We called 100 employers to request interviews, explaining our interest in understanding the hiring process and how employers respond to criminal records. At no point was the fact that they had been audited disclosed. We conducted 48 in-person interviews with hiring authorities, who broadly represented the range of audited establishments.[5] The 30- to 45-minute recorded interviews included open-ended questions about hiring procedures and culture, as well as fixed-choice questions regarding their level of concern with factors such as the time since offense, the type and level of offense, and case disposition. Interviewees were predominantly White (94 percent) and male (63 percent). Most had been in management for more than 3 years (83 percent) and had completed some college-level coursework (65 percent).

We began by asking employers to describe their organization and their role within it. We then discussed their hiring process and use of background checks, asking employers to consider various criminal records: low-level misdemeanors versus felonies, recent versus older offenses, convictions versus dismissals, and particular offense types. We asked them to share examples of when they confronted such records and how they reacted. Finally, they were asked about company policies and personal concerns regarding applicants with criminal records.

Interviews were recorded and transcribed, with identifying information removed before analysis. All qualitative analysis was conducted blindly, so coders were unaware of establishment names and covariates such as workplace diversity, callback outcomes, and tester race. We used a multistep procedure to ensure intercoder agreement. The third author and two research assistants independently coded every interview, using a grounded theory approach (Glaser and Strauss, 1967). Then, the team met to review each transcript, identifying common themes and coding like categories of data together. Each coder then placed conceptual labels on the events, experiences, and feelings reported in the interviews, resulting in a set of axial codes. The major (and axial) codes were as follows: reasons for checking (subthemes: information seeking, assessing trust, cost, and liability), reading records (understanding information, clarifying questions, and assessing severity), hiring (discretion, company policies, and hiring experience), mitigating factors (type of offense, time since offense, in-person contact, and giving second chances), and excluding factors (workplace harm and personal liability). The coder-specific axial codes were consistent, so they were grouped thematically into a master list of major codes and subthemes. The team then revisited each transcript, labeling passages to denote the themes. The third author then re-read all 48 transcripts to select excerpts, ensuring that they accurately represented each theme and reflected the sample as a whole (see Lageson, Vuolo, and Uggen, 2014, for further discussion of the interviews and their relation to callback rates).

5. The 48 percent response rate is comparable with other interview-based studies of organizations, including Pager and Quillian (2005), who reported 58 percent in a similarly designed study.

STATISTICAL MODELS

The audit data structure is as follows: Two White testers were sent to 147 randomly assigned employers, and two African American testers were sent to 153 randomly assigned employers. Each racial group can therefore be thought of as a separate experiment, conforming to a completely randomized block design for each race, with employer as the block (Cox, 1958). Each employer thus represents a cluster with two repeated measures, varying only on whether the applicant reports a misdemeanor arrest. Because responses within clusters are correlated, methods that do not take this correlation into account are problematic (Agresti, 2002: 491).

For bivariate analysis of the dichotomous callback outcome, we use McNemar's (1947) test of difference for matched pairs. Following Agresti (2002: 410–11), π_{ab} denotes the population probability of outcome a (callback) for the first tester and outcome b (no callback) for the other tester at the same employer. n_{ab} represents the count of the number of pairs in each cell, and p_{ab} denotes the sample proportion. The test assesses the hypothesis of marginal homogeneity or equality between cells in which testers had different outcomes: $H_0 : \pi_{1+} = \pi_{+1}$. McNemar's test depends only on cases classified in different categories (i.e., the discordant cells) for the two matched observations, but all cases contribute to inferences about how much π_{1+} and π_{+1} differ. It is thus equivalent to a fixed-effects logit model with only the treatment effect as a predictor. The test statistic simplifies to:

$$\chi_1^2 = \frac{n_{21} - n_{12}}{\sqrt{n_{21} + n_{12}}}$$

For the multivariate analysis, we use the random effects logit model, also known as generalized linear mixed models (GLMMs), because of our interest in effects that vary between employers (e.g., workplace diversity and race) and the heterogeneity between employers.[6] For a dichotomous outcome in a block design (Agresti, 2002: 493–5), this model is represented by the following equations:

$$\text{logit}\left[P\left(Y_{i1} = 1|u_i\right)\right] = \alpha + u_i \qquad \text{logit}\left[P\left(Y_{i2} = 1|u_i\right)\right] = \alpha + \beta + u_i$$

where $u_i = \alpha_i - \alpha$ for some constant α. Here, u_i is a random effect for employer cluster i, with $\{u_i\}$ independent from a normal distribution with mean 0 and variance σ^2 with σ unknown. Because the univariate random effect adjusts the intercept but does not modify the fixed effect, the model is often called a random intercept model. Instead of the usual fixed intercept α, it has a random intercept $\alpha + u_i$. The fixed effects, or betas, are typically the main focus of a GLMM, with the random effects describing positive correlation between observations within a cluster. The random effects parameters also indicate the degree of heterogeneity of a population.

Note that the fixed effects represent cluster-specific rather than population-average effects. That is, β is the effect of an arrest record on the probability that a given employer

6. The Hausman test confirms the equivalency of the random-effects and fixed-effects approaches. The online supporting information discusses the modeling choice of both fixed effects and random effects in greater detail, as well as the choice of GLMM over the alternative generalized estimating equation models.

will call one applicant relative to the other applicant. The first equation is thus the probability that a particular employer will call the tester with no record; the second is the probability the same employer will call the tester reporting an arrest. A predictor that does not vary within an employer, such as the race of the two testers submitting applications, can be interpreted as the effect for those with a similar random effect for the different groups (e.g., racial categories) (Agresti, 2002: 498). GLMM models were estimated with the xtmelogit procedure in Stata 12.0 (StataCorp, College Station, TX).

These significance tests prompt a discussion of statistical power. Although several values determine power, an intuitive way to pose the question is in terms of the magnitude difference: At what magnitude difference in the population does a particular sample size provide a reasonable chance (typically 80 percent) that a statistically significant effect ($p < .05$) will emerge in a given sample? For paired designs with dichotomous outcomes, however, power depends on more than this magnitude difference. First, the proportion of pairs in the concordant cells (i.e., neither or both testers received callbacks) compared with the discordant cells (i.e., the testers received different outcomes) contributes to the power. Second, the power is lower as the discordant proportions simultaneously approach .5. Because these quantities are difficult to determine a priori, power calculations are challenging in the design phase of such experiments. Nevertheless, our design provides power to detect a 5 to 10 percentage point difference between treatment and control groups based on the one-tailed test appropriate to our directional hypothesis, with the exact amount depending on the two conditions described (see Vuolo, Uggen, and Lageson, 2013, for examples and *R* functions to compute power and sample size). Because we designed our experiment with the directional hypothesis that employers will more often call back only the tester with the clean record, we report one-tailed tests for the main effect of our treatment (a misdemeanor arrest) within the text.

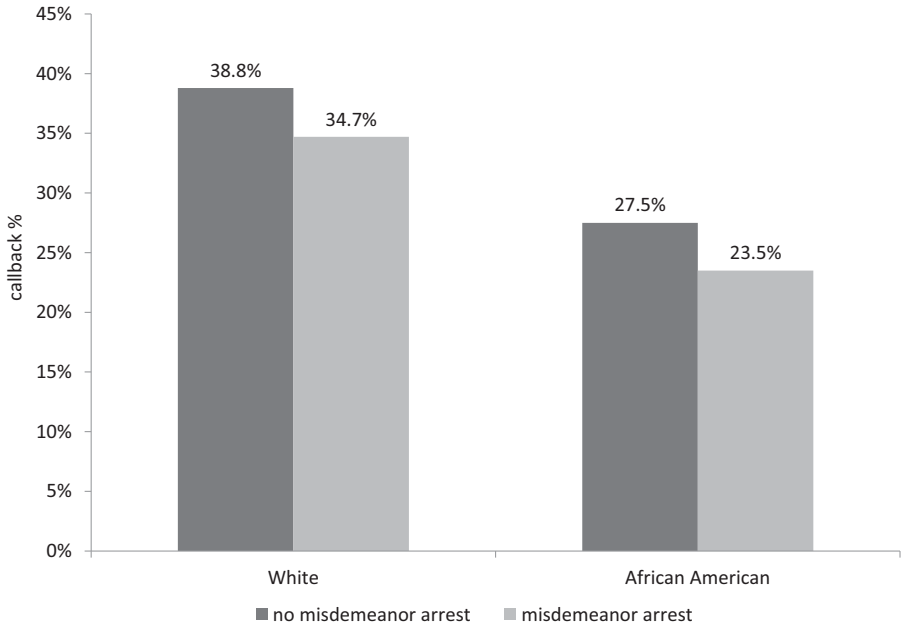
RESULTS

BIVARIATE AUDIT RESULTS

Figure 3 shows that Whites reporting no arrest received the most employer callbacks at 38.8 percent. For Whites in the arrest treatment condition, the callback rate was 34.7 percent. African Americans reporting no arrest had a 27.5 percent callback rate, relative to 23.5 percent for African Americans reporting an arrest. For both racial groups, there was thus a 4 percentage point difference between those reporting no record and those reporting a misdemeanor arrest. African Americans, however, received significantly fewer callbacks than Whites.

McNemar's test assesses whether the 4 percentage point difference for each race is statistically significant. Table 1 shows the distribution of callbacks by race and for the pooled sample as a whole. The table treats each *employer* as a case, distributed by whether they called back both, neither, or only one tester. Beginning with the table for Whites, both testers received a callback from 24.5 percent of employers and neither received a callback from 51.0 percent of employers. Referring to the off-diagonal, the control tester received a callback 14.3 percent of the time when the tester reporting an arrest did not. In 10.2 percent of cases, however, the treatment tester received a callback when the control did not. According to a one-tailed test following our design, McNemar's test is thus not statistically significant for Whites ($p = .20$, one-tailed, 95 percent confidence interval [CI] for

Figure 3. Callbacks Received from Employers by Race and Criminal Record



the .04 difference: [–.026, .108]), although the odds ratio of "only control called back" to "only arrestee called back" is 1.4. For African Americans, neither tester received a callback from 64.7 percent of employers, whereas both received a callback from 15.7 percent of employers. In 11.8 percent of cases, the control was called back and the arrest tester was not, relative to 7.8 percent in which the treatment tester was called back when the control was not. Again, McNemar's test is not significant ($p = .18$, one-tailed, 95 percent CI [–.019, .099]), with an odds ratio for the discordant cells of 1.5.

Although our design prioritizes between-record comparisons rather than between-race comparisons, the magnitude of the race difference is noteworthy. African Americans were called back almost one third less often than Whites, an 11 percentage point difference in both treatment and control groups. A comparison of the race differentials in the control (no-record) conditions of other audits is instructive. The Minnesota differential is significantly smaller than that reported in Pager's similarly designed Milwaukee audit (2003), and the more definitive New York City audit that sent testers of different races to the same employer (Pager, Western, and Bonikowski, 2009). In the latter study, the no-record callback rate was 31 percent for Whites, 25 percent for Latinos, and 15 percent for African Americans. We elaborate on these differences in our employer interviews.

Pooling the African American and White cross-classifications in the bottom panel of table 1, 13 percent of employers called the control tester but not the arrest tester. Conversely, the treatment tester received a callback from 9 percent of jobs in which the control did not. Here, McNemar's test is marginally significant according to a one-tailed test ($p < .10$, one-tailed, 90 percent CI [.005, .075], 95 percent CI [–.004, .084]), with an odds

Table 1. Proportion of Callbacks by Criminal Record for Each Paired Audit and McNemar's Test

		Misdemeanor Arrest		
White (n = 147)		**Callback**	**No Callback**	**Total**
No Misdemeanor Arrest	Callback	.245	.143	.388
	No Callback	.102	.510	.612
	Total	.347	.653	1.000
McNemar's Test: $p_{1+} - p_{+1} = .041$; SE = .041; OR = 1.400 $\chi^2 = .694$, d.f. = 1, $p = .405$ (two-tailed)				
		Misdemeanor Arrest		
African American (n = 153)		**Callback**	**No Callback**	**Total**
No Misdemeanor Arrest	Callback	.157	.118	.275
	No Callback	.078	.647	.725
	Total	.235	.765	1.000
McNemar's Test: $p_{1+} - p_{+1} = .036$; SE = .040; OR = 1.500 $\chi^2 = .833$, d.f. = 1, $p = .362$ (two-tailed)				
		Misdemeanor Arrest		
Total (n = 300)		**Callback**	**No Callback**	**Total**
No Misdemeanor Arrest	Callback	.200	.130	.330
	No Callback	.090	.580	.670
	Total	.290	.710	1.000
McNemar's Test: $p_{1+} - p_{+1} = .040$; SE = .027; OR = 1.444 $\chi^2 = 1.833$, d.f. = 1, $p = .176$ (two-tailed)				

ABBREVIATIONS: d.f. = degrees of freedom; OR = odds ratio; SE = standard error; p_{1+} = proportion in row 1; p_{+1} = proportion in column 1.

ratio of 1.44. The test thus provides some grounds for rejecting the null hypothesis of marginal homogeneity. Nevertheless, we observe markedly smaller differences than audit studies testing the effect of prison records. Although a minor arrest record is certainly no help to job applicants, it rarely disqualifies them from consideration.

LOGIT GLMM FOR MATCHED PAIRS

Table 2 shows estimates from logistic mixed-effects models, again presenting race-specific and pooled results. For each, models are shown with and without covariates. Although the bivariate models parallel the tests in table 1, those in table 2 also provide estimates for the treatment effect and the variability attributed to employers. The covariates additionally adjust for possible imperfect randomization on confounding factors that might mediate or moderate the treatment effect. One employer for each pair was dropped because of missing data on a covariate.

Models 1 through 6 again treat the results for the two race groups as separate randomized block design experiments. Models 1 and 4 only include a fixed effect for

Table 2. Logistic Mixed-Effects Regression for Receiving a Callback from an Employer

N (employers/observations)	White (146/292)			African American (153/306)			Pooled (299/598)			
Effects	**Model 1**	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**	**Model 9**	**Model 10**
Fixed Effects										
Misdemeanor arrest only	−.34	−.53	−.57	−.40	−.50	−1.43*	−.36	−.36	−.49†	−.95**
	(.34)	(.36)	(.50)	(.37)	(.39)	(.58)	(.25)	(.25)	(.26)	(.37)
White								1.02*	1.04*	1.05*
								(.42)	(.42)	(.42)
Contact with hiring		1.76***	1.75***		2.26***	2.40***			2.03***	2.05***
authority		(.50)	(.50)		(.57)	(.61)			(.38)	(.38)
Monthly unemployment		−1.64†	−1.64†		1.44	1.47			−.33	−.31
		(.85)	(.85)		(.99)	(1.07)			(.64)	(.65)
Workplace diversity (presence		.91	.87		.05	−.93			.48	.03
of minority employees)		(.57)	(.65)		(.60)	(.77)			(.41)	(.49)
Misdemeanor arrest only			.08			2.08*				.97†
*workplace diversity			(.69)			(.87)				(.52)
Second tester at employer		.48	.48		−.24	−.28			.13	.11
(vs. first)		(.35)	(.35)		(.38)	(.40)			(.26)	(.26)
Online ad (vs. paper)		.87	.87		−.21	−.21			.43	.44
		(.72)	(.72)		(.71)	(.77)			(.50)	(.51)
Audit number		−.01	−.01		−.01	−.01			−.01	−.01
		(.01)	(.01)		(.01)	(.01)			(.01)	(.01)
In Twin Cities (vs. suburbs)		−.54	−.54		.98	1.05			.15	.17
		(.58)	(.58)		(.63)	(.68)			(.43)	(.43)
Industry: office work		.32	.32		.16	.07			.21	.21
(vs. restaurant)		(1.16)	(1.16)		(1.56)	(1.69)			(.94)	(.96)
Industry: retail (vs. restaurant)		−.07	−.07		.30	.36			.28	.29
		(.83)	(.83)		(.80)	(.86)			(.57)	(.58)
Industry: warehouse/labor		.48	.48		−.34	−.35			.37	.38
(vs. restaurant)		(.73)	(.73)		(.95)	(1.02)			(.59)	(.60)
Industry: hotel (vs. restaurant)		.39	.39		1.12	1.14			.85	.84
		(.90)	(.90)		(.96)	(1.03)			(.67)	(.68)

(*Continued*)

Table 2. Continued

N (employers/observations)	White (146/292)			African American (153/306)			Pooled (299/598)			
Effects	**Model 1**	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**	**Model 9**	**Model 10**
Industry: other (vs. restaurant)		1.57 (1.19)	1.57 (1.19)		−1.02 (1.27)	−1.07 (1.37)			.50 (.84)	.52 (.86)
(Intercept)	−.85* (.34)	4.80 (3.58)	4.81† (3.58)	−1.91*** (.43)	−7.92† (4.19)	−7.78† (4.50)	−1.37*** (.27)	−1.88*** (.36)	−1.31 (2.71)	−1.08 (2.75)
Random Effects										
Employer	2.50 (.48)	2.20 (.49)	2.20 (.49)	2.68 (.52)	2.36 (.54)	2.60 (.59)	2.63 (.37)	2.59 (.36)	2.37 (.36)	2.42 (.37)
Log-Likelihood	−175.63	−160.68	−160.67	−156.32	−141.76	−138.51	−335.07	−331.98	−310.26	−308.51

†$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$. (two-tailed).

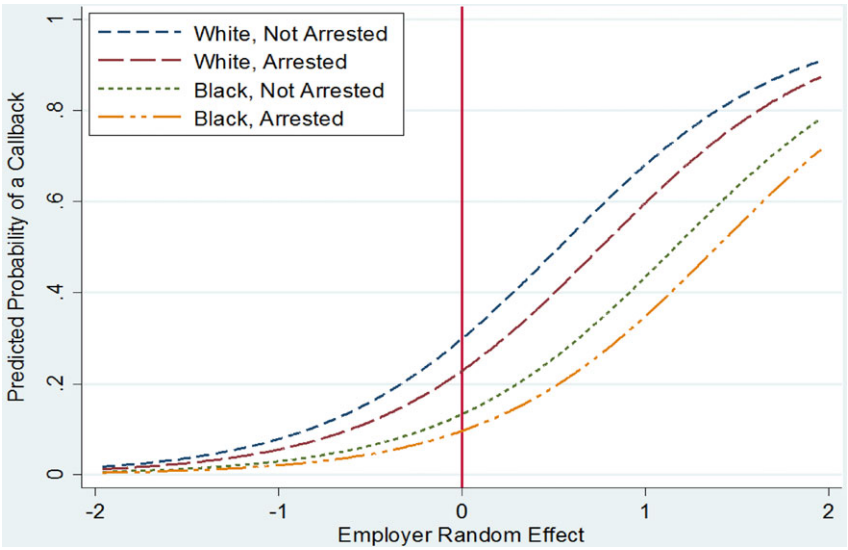
the misdemeanor arrest treatment and a random effect for employer, which provides analogous results to McNemar's test. Arrest is not statistically significant in either case, although it is negative in direction as expected. The random effect significantly improves the fit of the models, as indicated by likelihood ratio tests against a standard logistic regression model. The intercepts show a greater probability of callback for Whites than for African Americans. For a White tester without a criminal record, the probability of a callback is approximately .30 ($e^{-.848}/(1 + e^{-.848})$, 95 percent CI [.16, .44]). For an African American tester in the control state, the probability of a callback is .13 (95 percent CI [.03, .22]). The significant intercept in both models implies that this probability is significantly different from zero. Although nonsignificant, the misdemeanor arrest is estimated to reduce the probability of a callback for Whites to .23 (95 percent CI [.11, .36]) and for African Americans to .09 (95 percent CI [.11, .36]).

As expected, the random effects indicate that the population of employers is heterogeneous in the probability of calling back any applicant. In keeping with the cross-classifications in table 1, there is a strong employer-specific response pattern, such that either both or neither testers are likely to get callbacks (patterns [0,0] and [1,1]). For Whites, 111 of 147 employers, or approximately 76 percent, make the same (concordant) callback response. For African Americans, 123 of 153 employers, or approximately 80 percent, make the same response. Although this reveals a high within-employer association, it also implies variability in between-employer odds ratios. The impact of the level of heterogeneity is demonstrated in figure 4, which shows the probability of callback for employers up to 2 standard deviations from the mean random effect by race and arrest (from models 1 and 4). The figure plots the gap in the models among an employer with mean random effect (the zero line), as well as an increasing gap in the probability of callback for both race and the arrest treatment among employers with larger intercepts. The growth across the entire distribution demonstrates that all applicants benefit from employers who are more likely to hire, but this growth is more pronounced for Whites and those without an arrest record.

Models 2 and 5 add covariates to the race-specific models, which significantly improves model fit. The reduction in the random effects from models 1 and 4 implies that the covariates help explain some of the heterogeneity in the population. According to a one-tailed test following our experimental design and hypothesis, the arrest effect becomes marginally significant, decreasing the odds of callback by approximately 41.4 percent for Whites and 36.9 percent for African Americans ($p < .10$, one-tailed). An examination of models with only a single covariate (available upon request) reveals that this change is almost entirely a result of contact with the hiring authority. That is, the coefficient for arrest remains virtually unchanged when each covariate is tested singly, except for employer contact. Thus, the other measures' lack of covariation with the arrest effect attests to successful randomization. Contact with the hiring authority, which unlike the other covariates could not be randomly assigned, does covary with the arrest effect and results in a suppression effect. Contact dramatically increases the odds of a callback by approximately 5.8 times for Whites and 9.6 times for African Americans ($p < .001$). From models 1, 2, 4, and 5, we conclude that there is a marginally significant, modest effect of a misdemeanor arrest for both races but only net of the effect of making contact with a hiring authority. We explore this suppression mechanism in more detail in our interview data.

In addition to the main effects of the covariates, we considered interactions with the experimental arrest condition. Unemployment increased over the course of the study, for

Figure 4. Probability of a Callback Across the Distribution of the Employer Random Effect by Race and Arrest Treatment



example, and we tested whether the unemployment rate interacted with the arrest treatment effect (it did not). After testing all such interactions singly, only one emerges as significant—the callback differential for African Americans in the arrest and control conditions is significantly different in diverse workplaces. This effect is added to model 6 and depicted in figure 5 (although nonsignificant, we also show the effect for Whites in model 3 and the pooled model 10 for symmetry). Where non-White workers were observed, African American testers in the arrest group actually received more callbacks (33.3 percent) than did control testers (27.0 percent). The *lowest* callback rate (16.9 percent) involved African American testers with arrest records applying at all-White establishments. Given the dramatic racial disparities in arrest, employers who hire only Whites are likely less experienced in distinguishing, and discounting, the minor criminal records presented by our testers. In contrast, those employing persons of color may have learned through experience that a low-level record has little bearing on worker productivity.

As noted, personal contact is an especially powerful predictor for African American applicants. With other covariates held constant at their respective means and considering employers with similar random effects, table 3 shows consistently strong contact effects. The highest callback probability is for the no-arrest condition in all-White workplaces (.50 with contact and .08 without), followed closely by the arrest condition in more diverse workplaces (.43 with contact and .06 without). When employees of color are present, the nonrecord condition has a .28 callback probability with contact and .03 without contact. Again, those faring the worst are African American applicants with arrests applying to all-White establishments. When such applicants make contact with hiring authorities, they have a .19 probability of callback, relative to a .02 probability without contact. Thus, contact has a powerful and robust effect across all combinations of the interaction.

Figure 5. Callbacks for African American Applicants by Workplace Diversity (Presence of Minority Employees) and Criminal Record

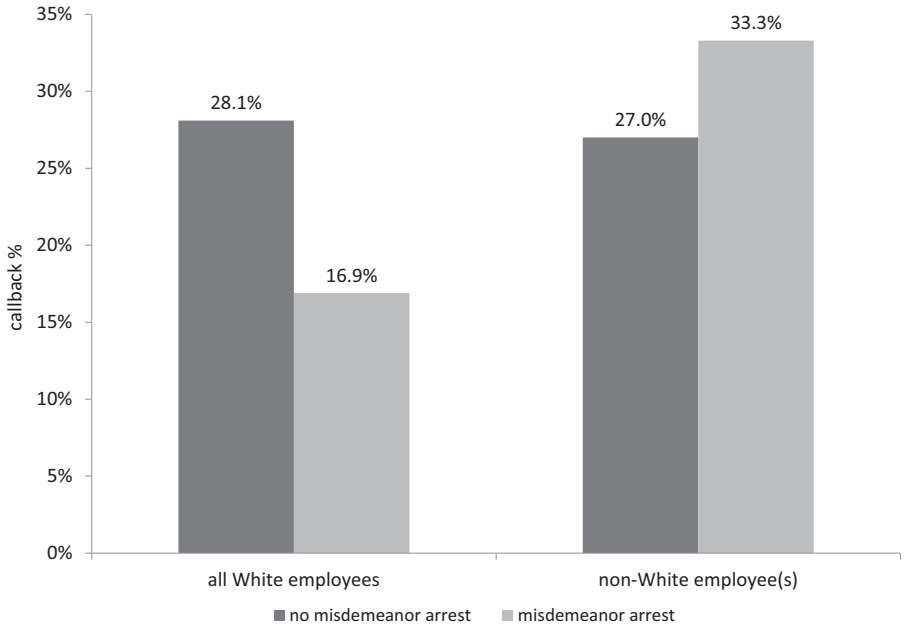


Table 3. Predicted Marginal Probability of Callback for African American Applicants for Employers of Similar Random Effects and with Covariates Constant at the Mean ($N = 146$ Employers, 292 Observations)

Variable	Contact with Hiring Authority	No Contact with Hiring Authority
No misdemeanor arrest, all-White workplace	.497	.082
	(.158)	(.044)
Misdemeanor arrest, minority employees	.427	.063
	(.152)	(.043)
No misdemeanor arrest, minority employees	.281	.034
	(.132)	(.026)
Misdemeanor arrest, all-White workplace	.191	.021
	(.108)	(.016)

NOTE: Numbers in parentheses are prediction standard errors.

Models 7 through 10 of table 2 show results when the data are pooled. Although White and African American tester pairs were not sent to the same employers, these results help calibrate the observed race differences in callbacks. As with McNemar's test, the arrest treatment is marginally significant ($p < .10$, one-tailed). The odds a given employer will call the applicant with the misdemeanor arrest are 30.5 percent lower than for the

applicant with no record. The race effect in model 8 indicates differences in the estimated probability of callback for employers with similar random effects, with Whites about 2.8 times more likely to receive callbacks ($p < .05$). From this model, the probabilities of the arrest and nonarrest conditions can be computed, producing results paralleling the race-specific models.[7] Model 9, which adds covariates, shows results analogous to the race-specific models: Because of the effect of contact with the hiring authority, the arrest treatment magnitude increases. Here, however, the effect is significant according to a one-tailed test ($p < .05$, one-tailed), with an arrest decreasing the odds of a callback by approximately 38.8 percent.

We specified several additional models to confirm the robustness of the findings, each described in the online supporting information. First, when there was no criminal record question on the application, testers were instructed to convey the record verbally to a hiring authority but were occasionally forced to tell another employee. Both situations could alter the callback probability. In the latter case, we cannot be certain the arrest record was actually conveyed to a hiring authority. In the former, verbal communication to a hiring authority may increase callbacks, either by displaying honesty or by altering previously held conceptions about testers' criminal backgrounds. We therefore estimated models that *omitted* all such tests. Second, given the centrality of the interaction of workplace diversity and the arrest condition to our findings, we examined several alternative codings of diversity.[8] Finally, we conducted an in-depth analysis of geographic context, collecting data on the 84 neighborhoods within which the audited jobsites were located. We found little evidence that applicants with records fared better (or worse) in more diverse, advantaged, or politically liberal areas. Although the estimates change slightly as cases are dropped, none of the specifications alters the substantive findings presented.

EMPLOYER INTERVIEWS

To help identify and elaborate the mechanisms behind the audit results, we interviewed a subset of the audited employers. The interviews contextualize the key audit findings—a reduction of approximately 4 percentage points in callback rates, reflective of the low level of the misdemeanor arrest, and a strong effect of employer contact. Although hiring authorities described a range of processes, three mechanisms for discounting low-level records emerged consistently in our interviews. First, many had the authority and discretion to make personal evaluations rather than judging candidates solely "on paper." Second, hiring authorities attended to offense severity and discounted nonfelony records, particularly when they risked losing a "good person." Finally, many held fast to the presumption of innocence, drawing clear distinctions between arrests and the greater degree of certainty represented by convictions.

7. A difference-in-difference test, based on the interaction between race and arrest, is close to zero and nonsignificant ($b = .05, p = .93$). In block designs where those of different races are not sent to the same employer, there is no covariance between the arrest treatment and race; thus, this result is not surprising (note that the arrest estimate and its standard error remain unchanged when race is added in model 8 because of the absence of covariation).
8. The online supporting information describes the consistency of testers' counts of non-White employees, as well as the distribution of contact with hiring authorities by tester.

Discretion and Experience

Consistent with theories of statistical discrimination, hiring authorities often relied on stereotypes about applicants with criminal histories when they lacked other information. Consistent with the sizeable effect of personal contact, however, our interviews affirm that these stereotypes can be overcome when authorities have the discretion to consider applicants as individuals and the experience to make informed judgments. Discretion and experience allow employers to look beyond low-level offenses, especially when applicants have compensating advantages. As Janet, a warehouse hiring manager in a diverse workplace, explained, "There is a lot of other factors that you look at as far as their experience, what is your pool of other candidates … so many other things you factor into that."[9]

When they encountered an offense on an application, some employers said they looked for positive traits to balance it against or used it to gauge the candidate's honesty and humility in confessing a past mistake. Justin, a sous chef and kitchen manager, was impressed by such candor: "If anything, people writing it down and telling me what they—admitting it to me before being questioned. A little more on the honest side almost helps them out a little." Zachary, a manager at another restaurant said (underlining indicates authors' emphasis):

> It's on a case-by-case basis. If the person has a good personality and I can read the person well and I feel that they are an honest person and they had an honest mistake in their past, it's a mistake that they had in their past and I'm willing to move on from that. That doesn't affect my decision making process.

For Zachary, how the candidate *relayed* the record was a more telling indicator of character and honesty than the simple fact of the record itself. Most such managers would learn of the record one way or another: *two thirds* of the 48 hiring authorities told us they conducted formal background checks for entry-level positions.

Discretion and experience also help explain the outsized effect of personal contact in our statistical models. Mark, a manager at a distribution center who called back both our (White) testers, personally evaluated applicants rather than relying on background checks:

> The thing about filling out an application and doing background checks is it's all paper. I actually talk to the person. If they get to the point in our company where we're doing a background check, I've talked to them. So I at least, I've looked them in the eye, he's answered some questions, I watch the way he reacts, I watch his mannerisms. Do I understand people totally? No. But at least I've seen this individual once. All they've seen is a piece of paper.

Similarly, Julie, who runs a dry cleaning chain, told us, "I can't say that there is a magical formula. I think that you get a sense for somebody when you sit down and talk." As these quotes suggest, managers draw on their experience and judgment in evaluating candidates

9. Table A.1 in the online supporting information provides an alphabetized list of interview respondents by firm type, title, education, race, and experience.

with little information at their disposal—relying on personal interactions and impressions rather than on a "piece of paper" or "magical formula" to reveal an applicant's honesty, reliability, or productivity. Furthermore, these quotes suggest why we observe an arrest effect when contact with the hiring authority is statistically controlled. Clearly, "looking someone in the eye" helps overcome some of the stigma of a low-level arrest, so those who have an opportunity to make a personal impression are less disadvantaged by the record.

Some hiring authorities found it difficult to attract and retain good workers in low-paying entry-level positions, so they looked closely at promising applicants with records. Don, a manufacturing plant manager in a diverse workplace, told us he needed discretion in such cases and "would hate to not get a good person because of a record." The disorderly conduct arrest tested as our treatment condition likely fit this mold, as it allowed room for discretion that may not have been extended to applicants with more serious records (Chiricos et al., 2007).

Severity and Stigma

Most employers explicitly distinguished between felonies and misdemeanors, consistent with legal and popular notions of severity and the modest effect we observed relative to Pager's (2007) studies of felony-level prison records. When asked directly, 60 percent of the employers said they treated felonies and misdemeanors differently, in some cases because their hands were tied by law (Stoll and Bushway, 2008) or by their superiors. When asked to rate the seriousness of the two categories on a scale from 1 to 10, they rated misdemeanors at 4.2 (with a standard deviation [SD] of 2.5) and felonies at 8.2 (SD = 2.0). Megan, who manages a chain hotel with racially diverse employees, said, "[T]he distinction between the two is big, especially if it's job-related. Felonies are obviously bigger crimes, so you look at those more carefully. But if they just have a misdemeanor and it's not really job-related, I think we kind of overlook that." Nancy, a restaurant owner, echoed this sentiment when she said, "You can get a misdemeanor for just about anything." Felonies, in contrast, were uniformly viewed as serious, if not disqualifying:

> We don't hire felons, by law they are more severe crimes. I can't imagine hiring a felon. For example, I had a great applicant—just wonderful, but they had a low-level felon[y] and I couldn't hire him. (Angela, country club manager)
>
> [W]hen we get the background check back we look and see; they do tell you if it's a misdemeanor or a felony. If it's a felony it's pretty serious you know, you want to pay attention to that and if it's serious enough we don't hire them. Even on our application it says if you've been accused of a felony, they put down there and you kind of look at that and go "I don't think so, I'm not interested." (Joyce, factory human resources manager)
>
> Felony there is going to be no time limit, I'm going to want to know, and there again comes in the background check. Misdemeanor is not so important, within a year, yeah kind of important, after that then not so important. I just know there are so many millions of people that have these little misdemeanors. (Matt, restaurant manager)

Comments regarding the time since offense show that employers attached far heavier and more *enduring* stigma to felonies than to misdemeanors, with the former often disqualifying applicants and the latter more typically leaving some latitude for discretion.

Certainty and the Presumption of Innocence

Finally, the presumption of innocence attending to arrest records helps explain the relatively modest arrest-only treatment effect we observed in the audit study—even before the 2012 EEOC guidance was issued. Most of our interview respondents relied on private search databases that included arrest information. Nevertheless, 63 percent of hiring authorities told us they differentiate between arrest and conviction, ranking the average severity of a dismissed offense at 3.5 (SD = 2.6) and a convicted offense at 7.5 (SD = 2.4). Dwight, a manager for a delivery company, explained, "If it was dismissed I would definitely look upon that more favorably than if they were convicted. Anybody can be accused of a crime." Roger, a car dealership general manager, similarly discounted dismissed crimes as "irrelevant," saying, "[A]nyone can be accused of something, but that doesn't make them guilty." Finally, Chris, who managed a pizza place, carefully elaborated on the presumption of innocence and his policy of hewing closely to the decisions of the justice system:

> [W]e like to rely on our judicial system … if something was brought up and they were found innocent we take the court's decision and of course we like to get a little background from the individual as well, you know understanding the situation. Maybe it was false allegations, wrong place wrong time … falsely accused and then found innocent. We tend to forget about the situation to some degree; let the innocent be innocent and the guilty be guilty.

As they screen applicants, employers thus seem to make judgments based on three interrelated aspects of criminal history: level of offense, case outcome, and time since offense. Such judgments, however, are driven in part by personal contact with applicants and their own experience and discretion. Of course, this process also reflects the organizational context and legal restrictions governing particular occupations and industries. For example, hiring authorities in state-licensed nursing homes reported less latitude in hiring persons with felony-level records than did those in family-owned restaurants. Despite such differences, managers often develop similar strategies to navigate criminal records.

CRIME AND DISCRIMINATION AT THE MARGINS

By carefully estimating the effects of the *least* serious criminal histories, this article's contribution is to establish a clear lower bound for the stigma of a criminal record. Prior experimental audits showed that African American men with prison records are all but disqualified from consideration for employment (Pager, 2007). Our study of low-level arrest-only records finds more muted effects. Apart from severity (of felony vs. misdemeanor) and certainty (of arrest vs. conviction), are the studies otherwise comparable? Pager (2003) graciously shared training materials with our team, and we closely replicated the key design features of her study. Moreover, the economic conditions in her Milwaukee study are not dissimilar to those faced by our testers. The Milwaukee area unemployment rate varied from 4.5 to 5.2 percent during Pager's 2001 audit (2007: 186), relative to an

average of 4.3 percent in our 2007–2008 Minnesota study. Differences in methodology and prevailing economic conditions are thus unlikely to explain the great discrepancy in results. Nevertheless, we should not minimize the impact of low-level records. Although our 3-year-old disorderly conduct arrest did not disqualify applicants outright, it still reduced employer callbacks by 4 percentage points among both racial groups.

In combination with previous audit studies, this article thus helps calibrate the stigma being attached to various criminal history profiles, which in our case cannot be confounded with prison-related gaps in employment history. Although we are cautious not to generalize beyond our own experiment, such calibration should inform policy efforts to strike an appropriate balance between the rights of employers and the rights of the accused or convicted. Although the justice system has long made stigma public, new technologies have radically reshaped the landscape of criminal background checks. With the growing visibility of criminal records, persons who were once "discreditable" if their crimes were discovered are today formally "discredited" at early stages of the hiring process (Goffman, 1963).

Although they may be discredited, applicants with low-level arrest records are not necessarily "discounted." Both our audits and our interviews show that hiring authorities often look beyond the mere fact of a criminal record, at least when they have the discretion to do so. As in previous investigations, we find that personal contact with applicants exerts a strong positive effect on callback rates. Such contact provides information that helps overcome statistical discrimination based on assumptions about group characteristics. In the absence of contact, employers may assume that arrestees are less productive than nonarrestees. If applicants can get a foot in the door, however, then they gain an opportunity to overcome negative stereotypes and reveal positively valued traits. This conclusion must be tempered, however, by persistent racial disparities in employment. To the extent that contact mediates stigma, race differences in job referral networks tend to disadvantage African American applicants (McDonald, Lin, and Ao, 2009; Wang, Mears, and Bales, 2010), who may lack the "weak ties" (Granovetter, 1973) or "bridging" social capital (Putnam, 2000) crucial to job search success.

These findings must be considered in light of gross racial disparities in arrest, as well as the spatial and economic marginality of African American men with criminal records (Gowan, 2011). The story is less that low-level records carry a greater stigma for African Americans and more that a much greater percentage of African Americans actually bear this stigma. Apart from the main audit results, perhaps our most intriguing finding concerns workplace diversity. The presence of at least one person of color in the establishment significantly reduces the effect of a low-level record for African Americans, which leads us to speculate that greater workplace diversity may signal greater employer familiarity with criminal records. Given racial disparities in punishment, employers with diverse workforces will almost inevitably have more experience evaluating criminal records than those with all-White workforces. To the extent that they learn from such experiences and update their group-level assumptions (Altnonji and Pierret, 2001; Pager and Karafin, 2009), employers may discount low-level records if they observe no productivity differences between arrestees and nonarrestees.

The term "felon-friendly" employer (Opsal, 2012) signals both that some organizations discount stigma (Goffman, 1963: 52) and that applicants still face *unfriendly* employers. Given the rising number of people with criminal records, the spotty quality of the various databases, and the ease of obtaining this information, the time is ripe for a renewed

national conversation about access to criminal histories. In 2012, the U.S. EEOC issued a detailed guidance document, designed to clarify standards and provide "best practices" on how employers may check criminal backgrounds without violating prohibitions against employment discrimination under Title VII of the 1964 Civil Rights Act (U.S. EEOC, 2012). Such a conversation is made more urgent by the dramatic overrepresentation of African Americans at every stage of criminal justice processing. Beyond its effect on the life chances of individuals with criminal records, access to criminal history information also contributes to group-based racial inequalities (Wakefield and Uggen, 2010; Western, 2006).

Finally, these results speak to policy efforts to regulate stigma and to balance the legitimate rights of employers and private citizens (Blumstein and Nakamura, 2009; Bushway and Sweeten, 2007). Although the permanent public availability of all criminal records is often taken for granted, such practices are the result of social choices. Our experimental audit suggests renewed attention to the appropriate threshold for making information public (arrest vs. conviction), the severity level that should pertain (misdemeanor vs. felony), and the duration of time records should be broadly available (limited-term vs. lifetime). The laws governing discrimination on the basis of criminal records are changing rapidly, with the EEOC guidance published in 2012 and 10 states passing "ban-the-box" legislation, which limits employer access to criminal records (National Employment Law Project, 2013). As of 2014, Minnesota employers are no longer permitted to inquire into criminal histories until applicants have been selected for an interview or offered a job. Such laws give jobseekers the chance to make contact with prospective employers—contact that this study suggests is crucial to the hiring process.

Whereas Pager's (2007) research addressed the core of criminal justice intervention—prison time served for a felony conviction—our study of low-level arrest considers the edge of stigma. At that edge, the mark of a criminal record is indeed fainter, although still consequential, for the individuals and groups most subject to both arrest and low-wage work.

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SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

Table A.1. Interview Sample Characteristics
Table A.2. Descriptive Statistics for Covariates