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ARTICLE

Criminal records and college admissions: A modified experimental audit*

Robert Stewart | **Christopher Uggen**

Department of Sociology, University of Minnesota

Correspondence
Robert Stewart, Department of Sociology, University of Minnesota, 909 Social Sciences, 267 19th Avenue S, Minneapolis, MN 55455.
Email: stewa640@umn.edu

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Abstract

In this article, we consider the effect of criminal records on college admissions. Nearly 72 percent of colleges require criminal history information during their application processes, which indicates that an applicant's criminal history could be a significant impediment to achieving the benefits associated with higher education. We conducted a modified experimental audit to learn whether and to what extent criminal records affect admissions decisions. Matched same-race pairs of tester applications were sent to a national sample of nonelite 4-year colleges, with both testers applying as either Black or White. Within each pair, one application signaled a prior low-level felony conviction only when required by the application. Consistent with the findings of research on employment, we find the rejection rate for applicants with felony convictions was nearly 2.5 times the rate of our control testers. Relative to the large racial differences observed in previous studies of hiring decisions, we find smaller racial differences in admissions decisions. Nevertheless, Black applicants with criminal records were particularly penalized when disclosing a felony record at colleges with high campus crime rates. We address implications for reentry, racial progress, and the college "Ban the Box" movement. We suggest colleges consider narrowing the scope of such inquiries or removing the question altogether – particularly when it conflicts with the goals of these institutions, including reducing the underrepresentation of students of color."

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KEYWORDS
collateral consequences, college, criminal records, education, experimental

Criminal records are a pervasive, acutely restrictive feature of American social life (Shannon et al., 2017; Travis, Western, & Redburn, 2014). They can limit access to employment (Pager, 2003; Pager, Bonikowski, & Western, 2009; Uggen, Vuolo, Lageson, Ruhland, & Whitham, 2014), public assistance (Gustafson, 2011), housing (Thacher, 2008), dating (Evans, 2019; Massoglia, Remster, & King, 2011), family planning options (Children's Bureau, 2015), and civic participation (Manza & Uggen, 2006). These restrictions can perpetuate cycles of crime, inequality, and lost opportunity, especially for low-income people and people of color.

Higher education has long been considered an instrument of social mobility and social cohesion in American society, as well as a potentially compelling mechanism for facilitating desistance (Blomberg & Pesta, 2017; Ford & Schroeder, 2010; Runell, 2017). College attendance and completion are associated with lower rates of unemployment and with higher relative earnings (Arum & Hout, 1998; Mayhew, Bowman, Rockenbach, Seifert, & Wolniak, 2016; U.S. Bureau of Labor Statistics, 2017). Receiving a higher education can also unlock access to valued opportunities, develop human capital, and foster civic membership (Becker, 1994; Boli, Ramirez, & Meyer, 1985; Brown, 2001). Yet, most colleges require applicants to disclose detailed criminal history information as part of the application process, and some evidence indicates that applicants are being rejected on the basis of their records (Custer, 2018; Pierce, Runyan, & Bangdiwala, 2014; Rosenthal, NaPier, Warth, & Weissman, 2015; Weissman, Rosenthal, Warth, Wolf, & Messina-Yauchzy, 2010). Thus, the benefits of higher education may not accrue for students with criminal records.

The increasing scrutiny of criminal records in college admissions is especially consequential for groups most subject to the criminal justice system, particularly young Black males (Brame, Bushway, Paternoster, & Turner, 2014). Considering the historic underrepresentation of Black Americans in higher education (Davis & Otto, 2016) and their overrepresentation in justice-involved populations (Kaeble & Glaze, 2016), criminal history disclosure requirements could lead to additional barriers to racial progress, student learning, and democracy. Officials cite campus safety as the principal reason for requiring criminal history information, especially in the aftermath of widely publicized on-campus incidents (Dickerson, 2008; Pierce et al., 2014). Significantly, however, these questions may be ill-suited for predicting future criminality (Runyan, Pierce, Shankar, & Bangdiwala, 2013) and reducing campus crime rates (Olszewska, 2007).

The use of criminal records in the college admissions process has rarely been considered in criminology, so little is known about the prevalence of criminal history disclosure questions and the degree to which a criminal record might affect college acceptance. In this article, we provide the first estimates of the effect of a criminal record on college admissibility of young men, as well as of the race differences in these effects. We begin by reviewing research on the relationship between higher education and punishment, discrimination on the basis of criminal records, and the use of criminal records in the higher education context. We then discuss the likely impact of policy remedies such as the "Ban-the-Box" (BTB) movement before asking an empirical question with policy implications: whether and how felony records affect college rejection rates. We use a modified experimental audit method to generate a specific estimate of the impact of a single felony conviction for robbery or burglary when applying to nonelite 4-year colleges, and we briefly highlight other forms of differential treatment that applicants with records encountered in our study. We find that even though a felony record is not a categorical

disqualifier, it can be a barrier to college access. We conclude by placing these results in the context of research on employment discrimination and the nascent BTB movement for colleges that parallels recent "fair chance" legislation governing job applications.

1 | PUNISHMENT AND EDUCATION

In a considerable amount of research literature, scholars have examined the effects of employment on desistance and recidivism (see, e.g., Berg & Huebner, 2011; Denver, Siwach, & Bushway, 2017; Laub & Sampson, 2001; Pager, Western, & Sugie, 2009; Skardhamar & Savolainen, 2014; Uggen, 2000), but there have been few rigorous studies in which the focus was on the relationship between higher education and desistance/recidivism, particularly for those *not* in prison. In existing observational and quasi-experimental studies of prison education, researchers have generally found that higher education (whether in or out of prison) is associated with positive outcomes, including lower recidivism and higher employment and earnings (Duwe & Clark, 2014; Lockwood, Nally, Ho, & Knutson, 2012). With respect to recidivism, in studies in which propensity score matching is employed to adjust for selectivity, scholars have generally found that prison-based college programs reduce recidivism measured by rearrest, reconviction, reincarceration, and revocation (Duwe & Clark, 2014; Kim & Clark, 2013). In a meta-analysis of more than 50 studies, the researchers estimated that participants in prison education programs were 43 percent less likely to recidivate than were nonparticipants (Davis, Bozick, Steele, Saunders, & Miles, 2013). Although these outcomes can be highlighted to speak to *prison* education, they are of limited usefulness because people who are incarcerated are at much greater likelihood of reoffending than is a general population sample of college applicants. Still, these research findings should encourage further study of education and postcriminal justice involvement and its link to employability and desistance from crime. Given the public and policy interest in higher education, desistance, and employability, an examination of barriers to college is merited.

Obtaining a higher education may facilitate desistance by serving as a bridge to cultivate social bonds with prosocial peers and role models, develop self-efficacy, and acquire human capital (Becker, 1994; Brown, 2001; Ford & Schroeder, 2010; van Dinther, Dochy, & Segers, 2011). Furthermore, receiving an education likely reduces crime by increasing the likelihood of better earnings and the quality of employment (Arum & Hout, 1998; Carnevale, Rose, & Cheah, 2013; Mayhew et al., 2016; U.S. Bureau of Labor Statistics, 2017), which could help counterbalance the negative effects of criminal justice involvement on employment, earnings, and mobility (Western, 2002).

Nevertheless, just as in the labor market, people with criminal records may be denied access to the benefits of higher education precisely *because* of their criminal records. A criminal record can be interpreted as a "negative credential" that can be used to restrict opportunities in much the same way that a college degree can be interpreted as a "positive credential" to expand opportunities (Gaddis, 2015; Pager, 2003). Where having a college degree might indicate honesty, a strong work ethic, and responsibility to an employer, for example, having a criminal record might indicate dishonesty, dangerousness, and irresponsibility (Kurlychek, Brame, & Bushway, 2006; Lageson, Vuolo, & Uggen, 2015), constraining job opportunities. Thus, if having a criminal record poses a similar challenge to higher education access as it does in the labor market, then the presence of prior criminal justice involvement could serve as a limitation to educational attainment and its beneficial effects on desistance and the transition to adulthood. As Lovenheim and Owens (2014) found by exploiting a federal policy change that restricted financial aid for a certain time period for people convicted of drug felonies, even though there was little evidence the policy change was an effective deterrent, there were notable consequences for affected students who experienced a delay in college enrollment and attainment, likely stunting

potential growth in their lifetime earnings. Consequently, barriers to college access that do not serve a sound public safety interest likely cause considerably more harm than they prevent.

2 | CRIMINAL RECORD DISCRIMINATION

The collateral consequences of a criminal record, particularly a felony-level record, have received great attention in institutional domains such as the labor market (Pager, 2007a), the political system (Manza & Uggen, 2006), and parenting (Wakefield & Wildeman, 2013). Although typically studied in isolation, these consequences are experienced as a system of disadvantage (Wheelock, 2005) in which legal and social restrictions are broadly and indiscriminately "piled on" (Uggen & Stewart, 2014). In the labor market, for example, researchers typically assess the effects of criminal records on hiring decisions, but such records are also used to constrain the opportunity to apply for many positions, whether through formal prohibitions on occupational licensing or employment in certain industries or through informal access to educational opportunities and credentials (May, 1995).

2.1 | Labor market context

Experimental research scholars have convincingly established that applicants with criminal records face significant discrimination during the application stage of the hiring process, in part, as a result of these criminal history questions. Schwartz and Skolnick (1962) were among the first to show how a conviction reduces "positive responses" [i.e., callbacks] from employers and leads to lasting "status degradation." Devah Pager and colleagues continued this work with rigorous, in-person audit studies to test the effect of a major felony drug conviction (Pager, 2003, 2007a; Pager, Bonikowski, et al., 2009), finding a significant reduction in callbacks for applicants with felony records compared with their nearly identical control applicants. As Pager summarized the results (2007a, p. 147), a criminal record represented "one serious strike" against White applicants but "almost total disqualification" for Black applicants. Using a similar in-person audit design, Uggen and colleagues observed a similar pattern in testing the effect of low-level arrest records, which reduced the callback rate by approximately 15 percent for Black applicants and 10 percent for White applicants (Uggen et al., 2014).

More recently, Agan and Starr (2017) conducted a large-scale correspondence audit in New York and New Jersey. They again confirmed the negative impact of a criminal record (in this case a low-level drug or property felony) on the first stage of the employment process, reducing callback rates by nearly 40 percent. Contrary to the standard racial hierarchy (in which the White applicant serves as the benchmark) found by Pager et al. and Uggen et al., however, Agan and Starr found almost no racial differences in callback rates, which may be a result of differences in how race is conveyed in correspondence studies (Decker, Ortiz, Spohn, & Hedberg, 2015).

2.2 | Higher education context

Paralleling the hiring process, most colleges ask applicants for criminal history information during the application process. A survey of 2-year and 4-year admissions departments by the Center for Community Alternatives found that 81 percent of private and 55 percent of public colleges have some type of criminal history question (CHQ) on their applications (Weissman et al., 2010). In preparation for this research, we reviewed freshman undergraduate applications for the fall 2015 entering class for every U.S. 4-year, nonprofit, nonspecialized college listed in Barron's *Profiles of American Colleges 2016* (Barron's PAC) that was accessible (a total of 1,350 colleges; table 1) and found comparable

TABLE 1 Share of colleges with Criminal History Questions (CHQs) on their applications, by institutional characteristics (N = 1,350)

Variable	*N*	CHQs
Total	1,350	71.6%
Competitiveness		
Most Competitive	89	89.9%
Highly Competitive	98	82.7%
Very Competitive	304	81.3%
Competitive	625	68.3%
Less Competitive	175	64.0%
Non-Competitive	59	33.9%
Institution Type		
Private	501	58.1%
Public	849	79.6%
Crime[a]		
Lower Crime	628	65.9%
Higher Crime	722	76.6%
Consider Race in Admissions		
No/Unknown	991	67.6%
Yes	359	82.7%
Setting		
City	627	70.8%
Suburb	326	77.6%
Town/Rural	397	68.0%
Size		
Large	263	59.7%
Medium	388	75.5%
Small	529	76.4%
Very Small	170	66.5%

[a]Higher crime (10 or more) and lower crime (less than 10) refer to reported crimes per 1,000 students over previous 3 years.

results. Barron's PAC is a popular college guidebook in which an admissions competitiveness ranking system is employed that is widely used in education research (see, e.g., Alon & Tienda, 2005; Carini, Hayek, Kuh, Kennedy, & Ouimet, 2003; Monks, 2000). Nearly 80 percent of private colleges and 58 percent of public colleges required applicants to answer CHQs on their applications that season. Furthermore, colleges that have more competitive admissions are more likely to feature CHQs on their applications, ranging from almost 90 percent for colleges classified as "Most Competitive" to slightly less than 34 percent for "Non-Competitive." Colleges with higher crime rates, colleges that consider race/ethnicity in their admissions process, and suburban colleges are also more likely to include CHQs on their applications. In a multivariate analysis that includes each of these factors (available from authors), more competitive institutions, private colleges, and those with higher Black undergraduate enrollments were significantly more likely to ask about criminal records. Like Vuolo, Lageson, and Uggen (2017), we found considerable variation in how colleges asked about criminal records on their applications, including interests in different levels of offense (any conviction, felonies,

juvenile offenses, certain offense types) and phases of the justice system (charge, arrest, conviction, supervision status).

The point at which criminal history information is considered in the admissions process varies, including before, during, or after considering academic admissibility (Rosenthal et al., 2015; Weissman et al., 2010). Most institutions require students to submit additional information if they disclose a criminal record, which could include a letter of explanation, multiple letters of recommendation, a criminal background check from a third-party vendor (at the applicant's expense), and an official criminal history report from their state's criminal record custodian, which often contains information that is publicly accessible (e.g., convictions) and information that is not (e.g., arrests without convictions and juvenile adjudications; Weissman et al., 2010). Collecting and submitting these additional materials can be time-consuming and burdensome, and many applicants choose to drop out of the process at this point (Rosenthal et al., 2015).

To be sure, colleges and universities ask their applicants about criminal history information for a variety of reasons, but publicly and in surveys, college administrators have suggested these questions are primarily grounded in concerns about campus crime and safety, or at least in the *perception* of campus crime and safety (Jaschik, 2016; Lantigua-Williams, 2016). At least half of the respondents of a 2011 survey of administrators at colleges that require criminal history disclosure about criminal records said that reducing violence, protecting against liability, and reducing illegal drug use were important reasons why they require criminal history information (Pierce et al., 2014). No evidence has been found, however, that criminal history questions on college applications are effective tools for reducing campus crime (Olszewska, 2007; Pierce et al., 2014; Runyan et al., 2013). In a 2007 study in which researchers controlled for various institutional characteristics, for example, no statistically significant difference was found in rates of campus crime at colleges that require criminal history disclosure compared with colleges that do not (Olszewska, 2007). In more recent work on college crime reports before and after state-implemented student background check requirements, researchers similarly found little effect on campus crime (Hughes, Elliott, & Myers, 2014).

Furthermore, the usefulness of criminal history application questions for soliciting accurate information has not been demonstrated empirically. For nearly all institutions, these questions rely at least initially on self-reported information from the applicant (Pierce et al., 2014; Weissman et al., 2010). As of this writing, college usually only verified affirmative criminal history responses; otherwise, the application continues to move through the process unfettered (Rosenthal et al., 2015). Otherwise, the application continues to move through the process unfettered. Thus, these questions are susceptible to inaccurate responses and may not be predictive of future criminal behavior or college misconduct.[1] Runyan et al. (2013), for example, surveyed graduating seniors at a large public university and conducted criminal background checks on a subsample. When they compared those data with students' responses to the criminal history question on their undergraduate applications, the researchers found that most students did not accurately disclose their criminal records. They did find that students with criminal records were somewhat more likely to engage in misconduct while in college (primarily related to marijuana or alcohol use), but application CHQs overall were poor predictors of that college misconduct. Runyan et al., however, did not include students who were rejected on the basis of their precollege criminal records nor students who left college before graduation, so the question of the predictive capability of criminal history screening questions in college admissions remains unclear.

[1] A University of North Carolina (UNC, 2004) task force found that of the 532 campus crimes reported between 2001 and 2004 in which a student was named as a suspect, only 21 had a criminal history. Only 8 of those 21 students, however, had disclosed their criminal record on their application. When UNC later expanded its use of background checks, there was no reduction in campus crime (Hughes et al. 2014).

Apart from direct concerns about campus crime, college policies may also be motivated by considerations of reputational and legal risk. To the best of our knowledge, there has not yet been a successful "negligent admissions" case in which a college was sued for admitting a student with a criminal record who later went on to victimize another student on campus (Dickerson, 2008; Ramaswany, 2014). This area of tort law is particularly complicated, however, involving, among other issues, a college's duty to protect students, faculty, and staff, and whether a person with a criminal record is a foreseeable risk (Dickerson, 2008). This legal uncertainty coupled with high campus crime rates—or the perception of high crime rates—in an era of penal risk, aversion and mitigation may provoke colleges to take a hardline approach to students with records and lead to significant discrimination in admissions, just as employers discriminate in the labor market, where negligent hiring suits are a more realistic possibility (Pager, 2007a; Todd, 2004). Thus, evaluating the role of campus crime and potential risk in higher education admissions with respect to applicants with criminal records would be instructive.

3 | BAN THE BOX GOES TO COLLEGE

The labor market disadvantages of criminal records and their racial implications have motivated at least 30 states and 150 cities and counties since 1998 to enact BTB policies (Avery, 2018). The basic BTB policy prohibits some or all employers in a given jurisdiction from inquiring about an applicant's criminal history on the initial application, theoretically preventing employers from categorically rejecting applicants with records with little-to-no consideration. But these policies do often allow for the employer to make those inquiries in later stages of the hiring process. The logic underlying such policies assumes that applicants with records will fare better if they are given some initial consideration and a "fair chance" to explain their criminal record during the interview phase (Avery, 2018; Uggen et al., 2014).

BTB advocates argue these policies could help to close the racial employment gap. According to estimates by Shannon et al. (2017), ~23 percent of Black adults overall and 33 percent of Black adult males have felony records, compared with 8 percent of adults and 13 percent of adult males in the overall population, respectively. Thus, Black adults are more acutely affected by criminal record questions, and removing these questions could reduce the effect of this disadvantage. Researchers on the policy's impact, however, have raised troubling questions about potential statistical discrimination when "the box" is banned. The findings from their work indicate that BTB policies might exacerbate some forms of racial discrimination (Agan & Starr, 2018; Doleac & Hansen, 2016; Vuolo et al., 2017). Agan and Starr (2018), for example, tested the interaction between felony record and race in correspondence audits conducted before and after BTB in New York City and New Jersey. After implementation of BTB, the Black–White gap in callbacks at companies that removed the box from their applications grew six-fold compared with the pre-BTB gap (43 percent and 7 percent, respectively). The implication is that the lack of direct criminal history information may have harmed Black male applicants *without* records as they had fewer opportunities to overcome employers' erroneous assumptions regarding their criminality (Agan & Starr, 2018; Phelps, 1972; Vuolo et al., 2017). Any BTB policy initiative should therefore seriously consider potential unintended consequences for those with and without criminal records, at least with respect to the employment context.

In recent years, the BTB movement has expanded to the higher education context. Based on concerns regarding barriers to reintegration and the racially disproportionate effects of the justice system, the Obama administration released a report in which colleges were urged to reevaluate and reconsider their admissions policies regarding criminal history questions (U.S. Department of Education, 2016). Later that same year, the State University of New York (SUNY) Board of Trustees voted to remove the

criminal history box from SUNY's application (Jaschik, 2016). Louisiana became the first state to pass and enact a college BTB bill in 2017 for its public colleges, followed by Maryland wherein the Maryland General Assembly overrode a veto in early 2018 to enact its own college BTB bill (Dresser, 2018; Newhouse, 2017). And in August 2018, *The Common Application* (2018) announced that it would no longer include a criminal history question on its main application component, although member colleges can continue to require criminal history information on their supplemental applications.

BTB reforms in the labor context have generally involved shifting the point at which an employer is permitted to inquire about criminal history from the application stage to a later stage in the process, such as the interview. But the context of higher education, which typically involves a single decision point—the admissions decision—rather than several stages as in the hiring process, might make delaying asking more complicated. Instead, most college BTB reforms have either done away with the questions entirely or narrowed the contexts wherein a college would request criminal history information, such as on-campus housing applications or when applying to certain programs in which a criminal record could become a barrier. In light of these reforms and the unintended consequences of BTB policies in the employment sector (e.g., Agan & Starr, 2018), it is increasingly urgent to learn how banning the box from college applications would affect applicants with and without criminal records.

4 | AUDIT METHOD AND ESTIMATING DISCRIMINATION

Audit studies have become an increasingly important methodology in the social sciences, particularly in discrimination studies (Gaddis, 2018b; Pager, 2007b). Randomized experiments have far greater capacity to detect causal relationships than have alternative approaches such as covariate adjustment analysis of survey data (Neumark, 2018). Because audits are typically conducted in real-world environments, they also tend to have greater external validity compared with lab-based experiments. The basic audit design typically includes a field experiment in which an attribute is randomly assigned to one tester within a pair of similar or near-identical testers to test the effect of that attribute on the outcome of interest (Gaddis, 2018a).

Researchers have used the audit method to detect discrimination in diverse contexts, including housing (Ghoshal & Gaddis, 2015; Turner, 1999; Wissoker, Zimmermann, & Galster, 1998; Yinger, 1998), medical decision-making (Schulman et al., 1999), and commercial transactions (Ayres & Siegelman, 1995; Ridley, Bayton, & Outtz, 1989). Perhaps the most notable application of audit methodologies has been in the area of labor market discrimination with respect to gender (Neumark, Bank, & Van Nort, 1996), sexual orientation (Tilcsik, 2011), race (Bertrand & Mullainathan, 2004; Pager, Western, et al., 2009), college credentials (Gaddis, 2015), and interactions among these and other factors (Gaddis, 2015; Pager, Bonikowski, et al., 2009). Recently, the audit method has been particularly effective in estimating criminal record discrimination in the labor market, particularly along dimensions of race, gender, and severity (Agan & Starr, 2018; Ahmed & Lång, 2017; Baert & Verhofstadt, 2015; Galgano, 2009; Pager, 2003; Pager, Bonikowski, et al., 2009; Uggen et al., 2014), and in housing (Evans, 2016; Evans & Porter, 2015; Furst & Evans, 2017).

5 | DESIGN AND APPROACH

Based on this literature, we designed a study to answer the following research questions: 1) whether and to what extent 4-year colleges discriminate on the basis of criminal records; 2) whether such discrimination varies by race and institutional context, including the local crime rate; and 3) how a BTB

strategy to remove criminal records questions from college application forms might affect admissions and racial inequality.[2]

To our knowledge, this study is the first audit of criminal records in higher education. The admissions context is distinctive in four fundamental ways that guide our research design. First, third-party testing services (e.g., ACT and SAT) and high schools are important intermediaries in the college admissions process (Klasik, 2012). To apply to nearly any U.S. college, applicants must request that official documentation (e.g., transcripts) be sent directly from their high schools. Many colleges, especially at the baccalaureate level, also require applicants to request that college entrance examination scores (e.g., ACT and SAT) be sent directly from their testing agencies. Thus, audits in the higher education context can either (a) attempt to fabricate this information or (b) recruit real people and use their actual educational background information. We chose the latter option, such that the testers in this project present real rather than fabricated educational histories.

Second, the applicant pool in higher education fills hundreds or thousands of seats rather than the small number of positions available in most employment contexts. We therefore measure final acceptance as the primary outcome rather than initial expressions of interest or callbacks. Third, admissions decisions are likely to be made by a group or team rather than by a single authority in the hiring context (Stevens, 2009). This structural difference might lead to greater risk aversion in accepting stigmatized applicants, although both environments are currently characterized by great legal uncertainty (Lageson et al., 2015). Finally, in contrast to the vast majority of research on race-based admissions and affirmative action, we examine admissions practices across a diverse range of nonelite 4-year institutions. As Hirschman and Berrey suggested, in much prior research, scholars tend "to infer the logic of the entire field of higher education from analyses of the admissions practices of elite institutions" (2017, p. 449), but we instead focus on the 80 percent of 4-year colleges that serve a wider array of students, geographies, and interests.

5.1 | "Modified" audit design

The college admissions context is an environment where complete fabrication, as is standard with the traditional audit design, is simply not realistic. Thus, we endeavored toward a "modified" audit design—modified because we use real records that are matched as opposed to totally fabricated records—in which two main goals are balanced. First, we prioritized modal representativeness by designing a project to emulate the real world as closely as possible. The second goal flows from the first in balancing goals of representativeness with realistic and practical significance to produce meaningful results. We sought to determine whether criminal record discrimination occurs in the admissions process and whether that discrimination is mediated by race.

The "modified" design presents opportunities for experimental research in contexts not otherwise available to the traditional audit design. Although labor market and housing audits have typically been focused on the initial phase of the application process, proceeding further in the process with fabricated materials is likely to raise suspicion and/or may not be possible when official documents are required. By incorporating real records that would pass the application review process with a much lower likelihood of detection, researchers have the opportunity to gain access to later stages of the process that otherwise might not be available or realistic with fabricated materials. Going further in the application process presents opportunities for collecting primary data for additional analyses,

[2]We initially planned to include female testers and testers in other racial and ethnic categories, but we revised this plan in view of budget limitations, difficulties identifying and securing suitable matched pairs, and the possibility of detection when submitting matched pairs of applicants from groups that are underrepresented on many of our sample campuses.

such as direct communications and correspondence generated as part of the process. We outline next how we proceeded through the design process for this field experiment of college admissions.

5.1.1 | Tester recruitment and matching

The first and most crucial step in our design process was to determine the makeup of our testers. As mentioned, it would not have been feasible to fabricate educational credentials. Most colleges require high school transcripts be sent directly from the high school. And third-party standardized testing companies (e.g., ACT or SAT scores), whose legitimacy is heavily embedded in the validity of their test scores, would not be willing to create fake score reports. We would also argue that it would have been unethical to assign felony records to these participants as some colleges seek official documentation of those criminal records. Therefore, we recruited real people to serve as the basis of our tester profiles and submitted tester applications using their actual high school transcripts, actual test scores, and actual criminal records.

We began recruiting in early 2015. For both profile conditions, we sought testers who had finished high school, taken the ACT, and were in their late teens or early twenties. Additionally, potential "Record testers" had to have a single felony conviction and "No Record testers" had to have no criminal history. The testers agreed that they would not attend the colleges in the sample, and we specifically sought testers who were not considering applying to any college within our sampling frame. We used several recruitment methods, including Craigslist ads, local and student newspaper ads, e-mail announcements, word-of-mouth, and partnerships with local nonprofits. Those interested were directed to an online sign-up form that asked basic background questions (name, age, contact information, graduation year, high school GPA, ACT score) and criminal history information. After approximately 11 months, we had a list of ~25 possible treatment testers and more than 450 possible control testers who met the basic qualifications.

From those two lists, we began the matching process. Knowing that it would be impossible to match testers perfectly on observable and unobservable characteristics that could bias the results (Heckman, 1998), we created matches that were purposely asymmetrical but would be analyzed as if they were equivalent. In each matched pair, the Record tester is slightly better qualified academically relative to the No Record tester (signaled by a slightly higher GPA and/or ACT score), whereas other features, such as high school rigor, high school profile, and applicant age were comparable. The assumption, then, is if one were to set aside the felony and compare the applications head to head, the Record tester application would be slightly more likely to be admitted than the No Record counterpart. Our matched pairs were reviewed by six admissions counselors at different colleges to confirm that, but for the felony, our record applicants would be the more competitive of the pair. We also calculated admissions index scores using formulas from seven different college systems to ensure that, within each pair, the Record testers were always the better of the two students on paper. The use of this asymmetrical approach will thus provide a conservative estimate of discrimination by design.

We successfully recruited two well-matched pairs of male testers,[3] one pair of B- students and one pair of C students, who were within a year of each other in age (early 20s). The Record testers' academic qualifications were slightly higher than their counterparts in terms of GPA (approximately +0.2 grade points), ACT (+1–2 composite points), and class rank (approximately +10–15 percentage points). Both Record testers were serving 5 years of unsupervised probation for felony convictions when they were 18 (one for a single count of aiding and abetting simple robbery and the other for a single count of burglary).

[3]We also recruited a well-matched pair of female testers, but one member of the pair withdrew from the study before we could enter the field.

After the matching process, we assembled application profiles for each tester. Each profile included tester-specific information (e.g., high school transcript, ACT score information, and other information that we would not be able to assign) and unique contact information (e.g., unique e-mail addresses, street addresses, phone numbers with voicemail boxes, and social media accounts). We then created sets of generic profile attributes that could be randomly assigned within each pair, including extracurricular activities, volunteering information, and employment information.

Essays were also required at several colleges. We approached writing the essays as if they were vignettes in a simple factorial experiment. We identified four widely used essay prompt themes and wrote pairs of essay responses for each theme, engaging similar topics in both essays. Although we wanted to be cautious about revealing information that might compromise our testers, these essay responses were generally focused on learning from bad choices. We then randomly assigned the essays within each essay pair to the Record tester or the No Record tester and swapped in relevant information where appropriate.

5.1.2 | Signaling race

Researchers using correspondence audits to test race must inevitably determine how best to effectively convey race for the given context. Where race cannot be signaled directly on application forms, researchers attempt to signal it indirectly through use of race-typed names, physical presentation or photos of applicants, membership in race-typed clubs (e.g., the Black Student Association), and other markers thought to be associated with race (Pager, 2007b). The U.S. Department of Education requires colleges that receive federal financial aid to ask a set of voluntary race and ethnicity questions on the application. In this context, we directly signaled race by selecting either "Black or African American" or "White," although we could not directly observe how it was used in their deliberations. Importantly for our purposes, the names of our testers were not particularly indicative of a specific race group and were unlikely to convey significant race signals on their own (see appendix A where we followed the guidance of Gaddis [2017]), so we randomly assigned race to each tester pair–college combination regardless of the actual race of participants. That is, the same tester pairs both checked "White" at one subsample of colleges and both checked "Black" at another subsample of colleges.

5.1.3 | Signaling criminal record

Unlike in previous audits in which the effect of criminal records in the labor market context was tested, we only conveyed the criminal record when asked on the college application. It would be highly unusual for an applicant to offer such potentially stigmatizing information unprompted in the higher education context. In most cases, colleges with criminal record questions would require additional materials relating to the criminal record. The specific additional materials requested varied widely from college to college, ranging from short explanations to several multipage documents. The typical request for additional information was a written explanation of the incident (which the research team prepared), official copies of police or court records, and an official criminal history report (that included public and private data) from the state criminal record custodian. Other documents that we submitted included letters of recommendation (which we fabricated), a letter from a probation officer or other criminal justice official, and a background check from a third-party vendor (typically at the applicant's expense).

5.2 | Sample

Defining our sample required several design choices. To guide our decision-making, we prioritized external validity. Thus, we chose to focus on 4-year colleges rather than on community colleges or

other 2-year institutions, which were much less likely to request criminal history information compared with 4-year colleges (Weissman et al., 2010) and offer fewer benefits in terms of lifetime earnings and employability compared with 4-year colleges. We similarly excluded highly specialized colleges (such as colleges of art or music and military academies) because of their unique nature.

Our universe of possible colleges ($N = 1{,}350$) consisted of U.S. 4-year colleges, stratified by the selectivity criteria noted in *Barron's Profiles of American Colleges* (Barron's College Division Staff, 2015), a popular college guidebook. *Barron's* competitiveness index, which is derived from a formula that includes various measures of SAT/ACT medians and scoring, high school class rankings, and acceptance rates of previous incoming classes, is widely used in academic and policy research for comparing higher education institutions. Barron's competitiveness index includes six levels ranging from "Non-Competitive" to "Most Competitive."

The wide variation in college selectivity compelled us to choose whether to draw our sample from the entire universe of 4-year colleges or from a select range of colleges. Because it would have been atypical for our applicant testers to apply to elite or upper-tier institutions (Bowen & Bok, 2016; Roderick, Coca, & Nagaoka, 2011), particularly those colleges where our testers would not have met the minimum academic requirements or would have been significantly below the ACT 25th percentile for the previous year, we excluded colleges ranked in the top two tiers of admissions competitiveness ("Most Competitive" and "Highly Competitive"). Removing the most competitive colleges left a possible sampling frame where our testers had a reasonable chance of being accepted. This choice follows the logic employed by many audits of the labor market (Pager, 2003; Pager, Bonikowski, et al., 2009; Uggen et al., 2014) where researchers had testers apply for entry-level positions rather than for executive or management positions.

Our final sampling frame included 1,163 institutions that served 82.1 percent of all 4-year college students. To this list of colleges, we added 2015 institutional data from the NCES Integrated Postsecondary Education Data System and drew two initial representative random samples of 200 colleges for each race condition. We assumed, however, that unanticipated complications (such as document requirements we could not satisfy or late transcript requests) would likely prevent us from completing some portion of the applications, thus, reducing the total sample. The initial samples were representative of the sampling frame on four key variables: 1) whether there was a criminal history question (dichotomous), 2) selectivity (more competitive, competitive, less competitive), 3) public or private (dichotomous), and 4) percentage of undergraduate enrollment who identified as White (as a proxy for undergraduate campus diversity). Each subsample was randomly assigned to one of two pairs of applications ([1] Black/no-record and Black/record or [2] White/no-record and White record). This design allows for us to calibrate the effect of a criminal record for Black and White applicants.

5.3 | Estimation

As the results of recent methodological work indicate, the traditional paired design in audit studies is not necessarily optimal in all settings (Vuolo, Uggen, & Lageson, 2016, 2018). Our design and statistical models closely parallel those of recent employment audits, particularly the research of Devah Pager (2003), Mike Vuolo (Uggen et al., 2014; Vuolo et al., 2016), and S. Michael Gaddis (Gaddis, 2015). Each record and race contrast can be considered a separate experiment (Cox, 1958). We have a within or matched design on the criminal record signal and a between design on the race signal (Gaddis, 2018a; Lahey & Beasley, 2018). Fear of arousing the suspicion of admissions officials or introducing potential bias led us to opt for a mixed within/between design across characteristics rather than for a full (four applications per institution) within design (Gaddis, 2015; Phillips, in press; Weichselbaumer, 2015, 2016).

Our primary dependent variable is the admission decision: accepted or not accepted. To be included in our analyses, both paired tester applications must be categorized as either "accepted" or "not accepted," thus, forming a complete pair. If the admissions decision for a tester application is either accepted or accepted with conditions, it is categorized as "accepted." If the application is rejected, waitlisted, the decision is deferred, or the college did not provide a decision by the beginning of the fall semester, it is categorized as "not accepted." An application that had any other outcome is deemed incomplete, and its pair is not included in our analyses.

From December 2015 to May 2016, we submitted 800 applications at 400 4-year colleges, with each tester pair assigned as White or Black for half the audits, which allowed us to determine the likelihood of college acceptance for each condition-race pairing (White record/White no-record; Black record/Black no-record).[4] Each pair applied to a different set of colleges as Black and as White. We left the field in September 2016 with 280 complete pairs (150 Black, 130 White). Reasons for a tester application to be categorized incomplete include documentation required that we could not provide (16.6 percent), researcher error (2.6 percent), a formal interview requirement (1.5 percent), and website issues (0.7 percent). Because we left the field with less than our entire sample of 400, the estimates we present next could be interpreted as a local average treatment effect.

Table 2 shows how our audit sample ($n = 280$) compares with our sampling frame ("Very Competitive to Non-competitive"; $n = 1{,}171$) and the larger scope of 4-year colleges ($N = 1{,}350$). The audit sample is similar to our sampling frame with a few notable exceptions. Colleges in the audit sample were less likely to consider race in admissions, were more likely to be in rural settings, and had slightly whiter undergraduate populations.

5.4 | Statistical models

We include a slate of institutional characteristics and audit characteristics to adjust for between-college heterogeneity. For each college, we include whether criminal history disclosure was an application requirement (0 = no, 1 = yes), institution type (public or private), collapsed admissions competitiveness categories (*Barron's* "Noncompetitive" and "Less Competitive" = less competitive, *Barron's* "Competitive" = moderately competitive, *Barron's* "Very Competitive" = more competitive), location (rural/town, suburban, urban), institution size (very small, small, medium, large), and whether the college considers race and ethnicity in admissions (according to responses to The College Board's *Annual Survey of Colleges*). We also include continuous measures to reflect the makeup of the undergraduate student population, including percent receiving Pell grants (to indicate student socioeconomic status), percent Black and percent White (to indicate student body diversity), and 6-year graduation rate. For audit characteristics, we include which tester pair the applicant belongs to (that is, the B- pair of the C pair), whether the applicant applied first within the pair to that college, and the audit order in which the colleges received applications.

We are also interested in whether there is a relationship between the perception of campus safety and applying to college with a felony record. We thus include a dichotomous measure of campus crime rate as a publicly available indicator of the perception of campus safety derived from data reported as required by the Clery Act, a consumer protection law designed to provide crime statistics and information about U.S. colleges to prospective students. Colleges with 10 or more total reported criminal

[4]Our primary research interest concerns the treatment effects of felony-level records, so we designed the study to maximize the power to detect these effects. Although we could have tested additional race-record combinations, subdividing the subsamples further would have diminished the power to detect differences. The absence of interracial within-college covariance in this design, however, means that our results regarding racial discrimination are reliant on randomized nonpaired data rather than on paired comparisons (cf. Pager et al. 2009).

TABLE 2 Descriptive statistics of colleges by competitiveness and sample (2015–2016)

Variable	Barron's Competitiveness Levels						Total	Sampling Frame	Audit Sample
	Most	Highly	Very	Comp	Less	Non			
N of Colleges	89	98	304	625	175	59	1,350	1,163	280
Total Enrolled Students	575,519	880,708	2,159,039	3,534,908	706,617	256,877	8,113,668	6,657,441	1,388,983
Public	10.1%	32.7%	31.3%	40.3%	46.9%	52.5%	37.1%	39.6%	37.5%
Acceptance Rate[a]	26.0%	53.1%	67.4%	69.6%	70.0%	69.2%	64.9%	69.1%	70.8%
Criminal History Required	89.9%	82.7%	81.3%	68.3%	64.0%	33.9%	71.6%	69.3%	71.8%
High Crime[b]	82.0%	64.3%	51.6%	48.5%	55.4%	49.2%	53.5%	50.4%	47.9%
Race Considered	92.1%	72.4%	36.8%	13.0%	7.4%	0.0%	26.6%	17.7%	11.4%
ACT Composite									
75th percentile[a]	33.0	30.3	27.3	24.5	22.4	22.4	25.8	24.9	24.3
25th percentile[a]	29.6	25.5	21.9	19.1	17.3	16.7	20.7	19.6	18.9
Undergraduates, Black	5.5%	4.9%	8.5%	14.6%	25.4%	26.3%	13.8%	15.2%	12.5%
Undergraduates, White	53.4%	62.3%	63.8%	59.5%	46.6%	44.2%	57.9%	57.9%	63.3%
Grad. Rate (150% Time)	88.1%	77.3%	62.9%	49.2%	39.8%	29.6%	54.8%	50.4%	46.9%
Pell Recipients	15.9%	21.7%	30.0%	38.4%	46.2%	49.2%	35.3%	37.9%	38.0%
Setting									
City	56.2%	51.0%	52.6%	42.7%	42.3%	44.1%	46.4%	45.3%	34.3%
Suburb	30.3%	27.6%	23.7%	24.6%	21.1%	15.3%	24.1%	23.4%	27.5%
Town/Rural	13.5%	21.4%	23.7%	32.6%	36.6%	40.7%	29.4%	31.3%	38.2%
Size									
Large	37.1%	30.6%	23.7%	17.4%	7.4%	10.2%	19.5%	17.2%	15.0%
Medium	16.9%	21.4%	29.3%	31.0%	32.6%	20.3%	28.7%	30.3%	28.6%
Small	41.6%	40.8%	38.2%	38.2%	40.0%	45.8%	39.2%	38.9%	40.7%
Very small	4.5%	7.1%	8.9%	13.3%	20.0%	23.7%	12.6%	13.7%	15.7%

[a]Because some colleges choose not to provide data for information not required as part of their admissions process, those colleges are not included in these categories.

[b]High crime is defined as 10 or more reported crimes per 1,000 students over previous 3 years.

Sources: Barron's Profiles of American Colleges (2015); Integrated Postsecondary Education Data System (2015–16); College Board's Annual Survey of Colleges (2017); U.S. Department of Education.

offenses over the previous 3-year period (2013–2015) were coded as "high crime" and fewer than 10 reported offenses as "low crime" (Hart, 2007; Hughes et al., 2014; Sloan III, Fisher, & Cullen, 1997). Admittedly, Clery Act crime data are flawed indicators of campus safety because they are based on self-report and internal college processes, and we cannot determine how these data are produced at each institution (Fisher, Hartman, Cullen, & Turner, 2002). Nonetheless, college leaders are sensitive to metrics and relative rankings, often despite their shortcomings or validity concerns, and Clery crime data are a common, publicly reported indicator to which university administrators and policymakers often attend (Sauder & Lancaster, 2006). Thus, colleges with higher reported crime rates may be more cautious when reviewing applicants with criminal records. We incorporate the crime rate measure as a standalone covariate and as an interaction term with felony record, which we discuss in more detail next.

Tester application pairs successfully applied as Black to 150 randomly assigned colleges and as White to 130 randomly assigned colleges. Each college represents a cluster with two repeated measures, one no-record application and one record application. Responses within clusters are therefore positively correlated, so treating within-cluster observations the same as between-cluster observations would be problematic (Agresti, 2013, p. 489).

For bivariate analysis of the dichotomous acceptance outcome, we use McNemar's (1947) test of difference for matched pairs. Following Agresti (2013, pp. 413–414; see also Uggen et al., 2014), π_{ab} denotes the population probability of outcome a (accepted) for the first tester and outcome b (not accepted) for the other tester at the same college. The count of the number of pairs in each cell is represented by n_{ab}, and the sample proportion is represented by p_{ab}. The test is aimed at assessing 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 (that is, 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 paired designs with dichotomous outcomes, statistical power depends on more than the magnitude difference in treatment and control acceptance rates (Vuolo et al., 2016). First, the proportion of pairs in the concordant cells (i.e., neither or both testers were accepted) compared with the discordant cells (i.e., the testers received different outcomes) contributes to the power. Second, power is lower as the discordant proportions simultaneously approach .5. Although these quantities are difficult to determine *a priori*, we computed power and sample size following the recommendations and R functions in Vuolo et al. (2016) in the design phase. Balancing these estimates against the average anticipated cost of each application, our study design should be sufficient to detect a 5- to 10-percentage point difference between treatment and control groups.

For multivariate analysis, we used the random effects logit model, or generalized linear mixed model (GLMM), to estimate effects that vary between colleges (e.g., selectivity and cost) and the heterogeneity between colleges. For a dichotomous outcome in a block design (Agresti, 2013, p. 491), this model is represented by the following equations:

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

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

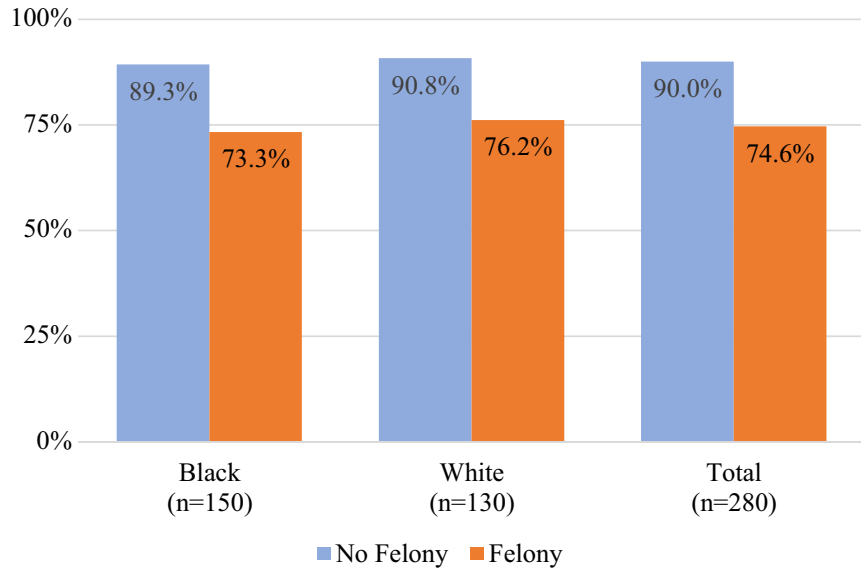


FIGURE 1 Acceptance rate by race and Felony record [Color figure can be viewed at wileyonlinelibrary.com]

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 college will accept one applicant relative to the other applicant. The first equation is thus the probability that a particular college will accept the tester with no record; the second is the probability the same college will accept the tester reporting a felony record. A predictor that does not vary within a college, 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, 2013, p. 495). GLMM models were estimated with the melogit procedure in Stata 14.1 (StataCorp, College Station, TX), and we calculated predicted values as average marginal effects (Muller & MacLehose, 2014). To check model robustness, we used a bootstrapped approach by resampling from the 280 paired tests across 10,000 replications. The bootstrapped standard errors changed slightly but were consistent with the standard errors presented below.

6 | RESULTS

In figure 1, we show the proportion of applicant profiles rejected by race and felony record status. Tester applicant profiles with felony records (that is, "Record testers") were rejected ~2.5 times more than their counterparts without felony records (that is, "No Record testers") across both race conditions. White No Record testers had the lowest rejection rate (9.2 percent) followed by Black No Record testers (10.7 percent). The rejection rates for the Record testers were significantly higher, 23.8 percent when applying as White and 26.7 percent when applying as Black, for differences of 14.6 and 16.0 percentage points, respectively (both differences were significant in a two-sample t test at $p < .001$). Black

TABLE 3 Distribution of accepted applications for each paired audit and McNemar's test

		Felony Record		
Black (n=150)		**Accepted**	**Not Accepted**	**Total**
No Felony Record	**Accepted**	.673	.220	.893
	Not Accepted	.060	.047	.107
	Total	.733	.267	1.000
McNemar's Test: $p_{1+} - p_{+1} = .16$; OR = 3.667; SE = .041; $p < .001$ (two-tailed); $\chi 2 = 13.71$				
		Felony Record		
White (n=130)		**Accepted**	**Not Accepted**	**Total**
No Felony Record	**Accepted**	.738	.169	.907
	Not Accepted	.023	.069	.092
	Total	.761	.238	1.000
McNemar's Test: $p_{1+} - p_{+1} = .146$; OR = 7.333; SE = .036; $p < .001$ (two-tailed); $\chi 2 = 14.44$				
		Felony Record		
Total (n=280)		**Accepted**	**Not Accepted**	**Total**
No Felony Record	**Accepted**	.704	.196	.900
	Not Accepted	.043	.057	.100
	Total	.746	.254	1.000
McNemar's Test: $p_{1+} - p_{+1} = .154$; OR = 4.583; SE = .056; $p < .001$ (two-tailed); $\chi 2 = 27.6$				

Abbreviations: OR = odds ratio; p_{1+} = proportion in row 1; p_{+1} = proportion in column 1; SE = standard error.

applicants in both record conditions were rejected at slightly higher rates (1.5 percentage points higher for those without felony records and 2.9 percentage points higher for those with records), although a two-sample t test of the difference by race between the Black and White applicants with records is not statistically significant.

6.1 | Bivariate audit analysis

As noted, McNemar (1947) provided a test of whether the proportional differences in rejection rates between No Record testers and Record testers are significant. For this analysis, each college is treated as a single experimental unit, or a case, and placed in one of the four cells of a 2 × 2 contingency table. As reported in the first panel of table 3, when our testers applied as Black, both were accepted by 67.3 percent of colleges and both were rejected by 4.7 percent of colleges. In the discordant cells, 6 percent of colleges rejected the No Record tester but accepted the Record tester, whereas 22 percent accepted the No Record tester but rejected the Record tester. A McNemar's two-tailed test of the discordant cells, in which consistency is examined across two variables, is statistically significant at the $p < .001$ level with an odds-ratio (interpreted as "only No Record tester accepted to only Record tester accepted") of 3.667.

When our testers applied as White (second panel of table 3), we find a similar and somewhat stronger pattern. Both White testers were accepted by 73.8 percent of colleges (slightly higher than our Black applicants), and both were rejected by 6.9 percent of colleges. On the off-diagonal, the No Record tester was rejected and the Record tester was accepted by 2.3 percent of colleges, whereas the opposite scenario—No Record tester accepted; Record tester rejected—occurred at 16.9 percent of colleges. A two-tailed McNemar's test is again significant at the $p < .001$ level with an odds-ratio of 7.333. Although it may seem that a felony record has a stronger negative impact for White applicants, we caution against this interpretation (and return to this issue in the mixed-effects models that follow). The

odds ratio is a comparison of "only the No Record tester accepted to only the Record tester accepted," which is dependent on the proportions in the off-diagonal cells. Here, the lower odds ratio for our Black sample results from the combination of the slightly lower overall acceptance rates (as seen in figure 1) and higher variability in outcomes when our testers applied as Black (compare p_{10} and p_{01} of the Black panel to the White panel in table 3).

Although this study's paired design is best understood as consisting of two separate experiments by race, pooling the two samples can be informative for the present study and future research. We present the pooled results in the third panel of table 3. When pooled, both testers were accepted at 70.4 percent of colleges, both were rejected at 5.7 percent, the No Record tester was rejected and the Record tester was accepted at 4.3 percent, and the No Record tester was accepted and the Record tester was rejected at 19.6 percent. As with the Black and White results separately, a two-tailed McNemar's test of the pooled results is significant at the $p < .0001$ level with an odds ratio of 4.583. To summarize the bivariate results, a felony record is not a categorical disqualifier in the higher education context, but having a felony record leads to significantly higher rejection rates for both Black and White applicants.

6.2 | Logistic mixed-effects regression models

We next present logistic mixed-effects models predicting college acceptance, shown in table 4 for the Black sample (models 1–3), the White sample (models 4–6), and the pooled sample (models 7–10). As previously noted, we used a bootstrapped approach by resampling from the 280 completed pairs across 10,000 replications for each model, and the resulting bias-corrected standard errors were consistent with those presented in table 4. For each, we present models with and without covariates. Models 1 and 4 are race specific and include only the fixed effect of the felony record and a random effect for college. As with the bivariate results in table 3, we find strong felony record effects for both race groups, although the felony record coefficient is larger for the White tester sample (–1.97 vs. –1.30). A comparison of the intercepts shows a greater probability of acceptance when our testers, all of whom surpassed the minimum admissions standards of the college, applied as White than when they applied as Black, which likely explains in part the larger felony record coefficient.

The addition of covariates in models 2 and 5 potentially moderates the influence of confounding variables not accounted for in the sampling process, adjusting for college characteristics and audit design characteristics. The added covariates reduce the random effect variance, indicating that a substantial proportion of the college-level variance can be explained by institutional characteristics. The main effect of the felony record is robust for both samples and again is a bit larger for the White applicants (–2.02; $p < .01$, two-tailed) compared with the Black applicants (–1.35; $p < .001$, two-tailed). Additionally, controlling for other covariates (including whether the tester had a felony record), the presence of a criminal history disclosure question was negatively associated with acceptance for both samples, although the relationship was marginally significant for the White applicants (–1.74; $p < 0.1$, two-tailed) compared with Black applicants (–1.38; $p < .01$, two-tailed). For our Black applicants, acceptance was more likely at colleges with larger proportions of undergraduates who were White or who were receiving Pell grants.

College administrators, concerned about campus crime and public safety – or the perceptions of campus safety, often support apprehension about admitting applicants with criminal records (Pierce et al., 2014). In models 3 and 6, we add an interaction term to test whether applicants with felony records fare worse at colleges with higher perceived crime rates by using 10 or more reported incidents of crime per 1,000 students according to Clery Act data in the previous 3 years as a rough indicator of a high crime rate. Indeed, on campuses that report more crime, Black record applicants were significantly less likely ($p < .05$, two-tailed) to be accepted compared with Black no-record applicants. We observed

TABLE 4 Logistic mixed-effects regression for college acceptance

N (colleges/applications)	Black (150/300)			White (130/260)			Pooled (280/560)			
Effects	**Model 1**	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**	**Model 9**	**Model 10**
Fixed Effects										
Felony record	−1.30***	−1.35***	−.75	−1.97***	−2.02**	−1.46*	−1.52***	−1.52***	−1.52***	−.93*
	−(.38)	−(.39)	−(.47)	−(.60)	−(.63)	−(.75)	−(.32)	−(.32)	−(.32)	−(.39)
High crime		−.18	.90		−.21	.64			−.15	.87
(10+/1,000 students)		−(.41)	−(.71)		−(.70)	−(1.02)			−(.35)	−(.58)
Felony record*High crime			−1.60*			−1.21				−1.51*
(Interaction)			−(.81)			−(1.06)				−(.63)
Black								−.24	−.32	−.34
								−(.35)	−(.34)	−(.36)
Race/Ethnicity Considered		−.64	−.73		.60	.59			−.10	−.12
		−(.55)	−(.61)		−(1.05)	−(1.06)			−(.50)	−(.54)
Criminal History		−1.38**	−1.48**		−1.74+	−1.77+			−1.34**	−1.41**
Disclosure Question		−(.52)	−(.56)		−(.95)	−(.96)			−(.45)	−(.48)
Public (ref: Private)		−.59	−.64		2.40*	2.39+			.64	.68
		−(.56)	−(.60)		−(1.22)	−(1.22)			−(.51)	−(.53)
Competitiveness: Moderate		1.32	1.41		−1.10	−1.11			.12	.13
(ref: Less)		−(.96)	−(1.04)		−(1.18)	−(1.19)			−(.66)	−(.70)
Competitiveness: High		.22	.23		.73	.74			.44	.46
(ref: Less)		−(.51)	−(.54)		−(.90)	−(.90)			−(.44)	−(.46)
City		.02	.02		−.27	−.27			−.13	−.14
(ref: Rural)		−(.45)	−(.48)		−(.87)	−(.87)			−(.42)	−(.44)
Suburban		−.03	−.02		−.70	−.66			−.24	−.23
(ref: Rural)		−(.48)	−(.52)		−(.92)	−(.92)			−(.43)	−(.46)
Size: Large		.64	.73		−4.29*	−4.28*			−1.30	−1.34
(ref: Very Small)		−(.90)	−(.96)		−(1.93)	−(1.93)			−(.81)	−(.86)

(Continues)

TABLE 4 (Continued)

N (colleges/applications)	Black (150/300)			White (130/260)			Pooled (280/560)			
Effects	**Model 1**	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**	**Model 9**	**Model 10**
Size: Medium		.44	.45		−1.95	−1.92			−.66	−.70
(ref: Very Small)		−(.71)	−(.77)		−(1.42)	−(1.42)			−(.63)	−(.67)
Size: Small		.69	.75		−.52	−.52			.04	.04
(ref: Very Small)		−(.59)	−(.64)		−(1.22)	−(1.22)			−(.55)	−(.58)
Undergraduate enrollment		−2.79	−3.00		4.55	4.41			.41	.41
Black (proportion)		−(2.43)	−(2.64)		−(3.93)	−(3.91)			−(1.89)	−(2.00)
Pell Grant recipients		6.74*	7.27*		−4.28	−3.90			1.63	1.79
(proportion)		−(3.01)	−(3.25)		−(4.78)	−(4.77)			−(2.38)	−(2.51)
Six-Year Graduation rate		−3.90+	−4.27+		−.64	−.60			−2.50	−2.70
		−(2.24)	−(2.46)		−(3.54)	−(3.56)			−(1.87)	−(1.98)
More qualified tester pair		.97*	1.04*		1.35+	1.36+			1.15***	1.20**
		−(.38)	−(.42)		−(.72)	−(.72)			−(.35)	−(.37)
First to apply		−.17	−.15		.60	.60			.12	.14
		−(.35)	−(.36)		−(.54)	−(.54)			−(.28)	−(.29)
Month submitted		.11	.14		.12	.11			.09	.10
		−(.14)	−(.15)		−(.29)	−(.30)			−(.13)	−(.14)
(Intercept)	2.54***	2.10	1.74	4.25***	6.09+	5.61	3.11***	3.25***	3.79*	3.55*
	−(.43)	−(1.82)	−(1.95)	−(.95)	−(3.47)	−(3.49)	−(.41)	−(.47)	−(1.67)	−(1.76)
Random Effects										
College (variance)	1.18	.26	.57	7.05+	4.13	4.20	2.82*	2.83*	1.69+	2.12+
	−(1.00)	−(.69)	−(.87)	−(4.05)	−(2.95)	−(2.98)	−(1.25)	−(1.25)	−(.96)	−(1.14)
Log-likelihood	−136.70	−117.20**	−115.00*	−103.30	−87.19*	−−86.58	−242.30	−242.10	−217.00**	−214.00*

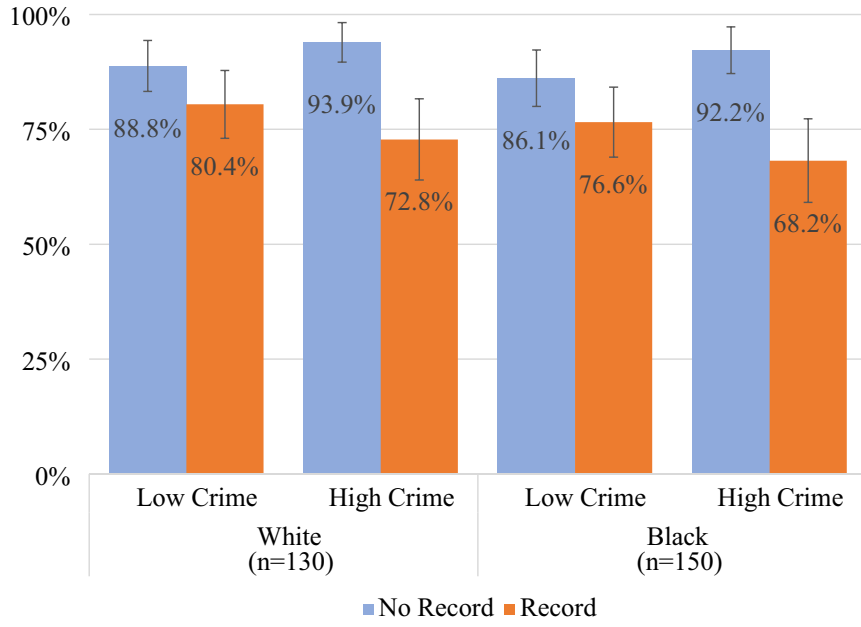
Numbers in parentheses are standard errors.

*$p < .05$;

**$p < .01$;

***$p < .001$ (two-tailed)

Log-likelihood significance refers to likelihood-ratio test comparing to previous model within sample.



Note: Error bars indicate 95% confidence intervals

FIGURE 2 Predicted probabilities of admission by race for record status and crime rate [Color figure can be viewed at wileyonlinelibrary.com]

a similar, although nonsignificant pattern for White felon applicants. Under this coding, the main effect of felony record (indicating its effect on campuses with lower crime rates) is significant and negative in model 6 (–1.46, $p < .05$, for White applicants) but not in model 3 (for Black applicants).

We pool our two race-specific samples and present the logistic mixed-effects estimates models 7–10 in table 4. Although we did not include cross-race pairs (that is, one Black and one White applicant within a pair) applying to the same colleges in our design, the same pairs of applicants with the same academic credentials and records applied to the colleges of both samples but varied their race. Estimates for models that include only a fixed effect for felony record and a random effect for college are presented in model 7, and a fixed effect for Black race is added in model 8. In both models, felony record is negative and statistically significant ($p < .001$, two-tailed). The nonsignificant race effect indicates that differences between Black and White applicants are not distinguishable from zero, net of the felony record condition. Results for pooled models with covariates (in model 9) and the felony record/high crime rate interaction (in model 10) generally parallel the race-specific models.

The effects under the interaction coding in model 10 indicate that a felony record becomes a moderate hurdle for applicants at colleges with low crime rates but a bigger hurdle at colleges with high crime rates overall. To elaborate and clarify these results, figure 2 presents predicted probabilities of admission by race, record, and college crime rate. We find that applicants without records fared well regardless of race and the local college crime rate, with acceptance probabilities of at least 86 percent. This probability declines to 76.6 percent for Black testers and to 80.4 percent for White testers with records in low-crime contexts. In higher crime contexts, the marginal probability of admission drops to 68.2 percent for Black applicants with records and to 72.8 percent for White applicants with records. In short, Black applicants with felony records applying to colleges with higher crime rates face the greatest likelihood of rejection.

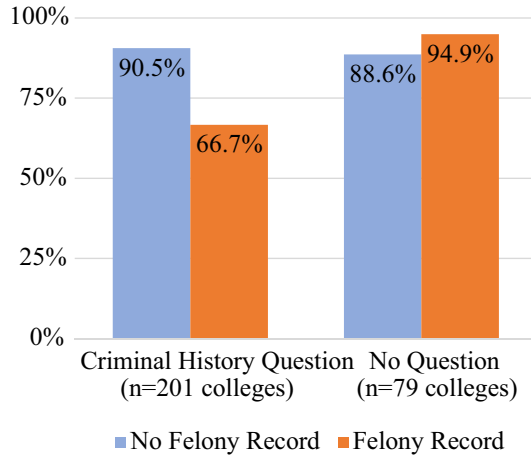


FIGURE 3 Acceptance rate by criminal history application question and Felony record [Color figure can be viewed at wileyonlinelibrary.com]

6.3 | Impact of criminal history disclosure questions

Evaluations of recent BTB initiatives in which criminal history questions are banned on employment applications have resulted in findings that indicate potential adverse effects on Black men without records (Agan & Starr, 2018; Vuolo et al., 2017). The results of our analysis may provide some initial reassurance that such adverse outcomes are less likely in the higher education context. Because our design included both colleges that required criminal history information and colleges that did not, we can directly compare how our Record and No Record testers fared under each condition. We did not sample on whether a college required criminal history disclosure, so we caution readers that this comparison is not perfectly interpretable and should be understood as exploratory. Nevertheless, we show in figure 3 the rejection rates of applicants with records and without records from the pooled sample at colleges with "the box" and colleges without "the box." When required to disclose their felonies, the Record testers were rejected by a full third of colleges in the sample. But when they were not required to disclose their felonies, their rejection rate dropped precipitously to only 5 percent, whereas the No Record testers fared slightly worse at colleges without the box (11.4 percent rejected without the box compared with 9.5 percent with the box). This low rate of rejection for the Record testers is to be expected as our study was designed to ensure that the testers presenting records should have been somewhat more attractive to admissions officials than the No Record testers.

Based on these results, we next explored whether Black applicants without records were harmed by removal of criminal history information from college applications in figure 4. We again estimated marginal probabilities of acceptance, although we departed from our paired design to include all complete No Record testers ($n = 330$) using a logit equation that interacts race and CHQ with the same controls from figure 4 constant at their means. As a result, we find that differences within race in the probability of acceptance for colleges with and without the box are not statistically significant. The difference between college categories is indeed larger for Black applicants who saw a decrease in acceptance of 5.8 percentage points at colleges that do not ask compared with an increase of nearly 1.8 percentage points for White applicants, however these differences were not statistically different from each other. Although we again caution that we did not sample on whether colleges required criminal history information, we did not find strong evidence indicating that BTB in higher education admissions would have a pronounced adverse effect on outcomes for Black males.

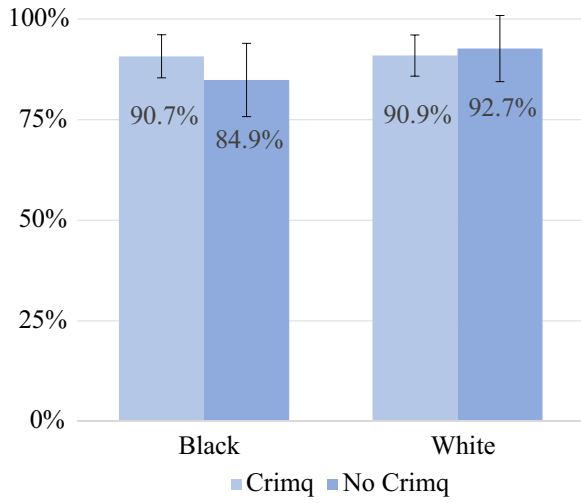


FIGURE 4 Predicted probabilities of acceptance for No-record applicants by race ($n = 330$) [Color figure can be viewed at wileyonlinelibrary.com]

7 | DISCUSSION AND LIMITATIONS

In this study, we posed three basic questions regarding criminal records and college admissions, which we discuss in turn. First, we find that 4-year colleges clearly discriminate on the basis of criminal records. By comparing rejection rates of college applicants with and without criminal records, we find that applicants with criminal records were rejected at a rate approximately three times higher than applicants without records from colleges that require criminal history information. Moreover, as a result of asymmetric matching, our results are conservative by design. Had we instead included testers with equally or matched educational credentials, it is likely the impact of the felony record would be larger. Similarly, we would also expect a greater impact for individuals presenting more extensive or more serious criminal histories than the single low-level simple robbery and burglary felonies presented by our testers. These results should thus be interpreted as a baseline measure of discrimination against college applicants with criminal records.[5]

There are several reasons for caution in generalizing our findings. Because we left the field with 280 completed pairs from our original sample of 400, these results yield a local average treatment effect. These findings are further limited because they cannot speak to all possible combinations of criminal record type/severity, academic background, gender, and race/ethnicity. We made the design choice to exclude the top two competitiveness tiers out of concern that our testers would not be competitive because all of our testers' ACT composite scores were below the 25th percentile for those top tiers. The field would benefit from future research aimed at exploring these combinations, particularly different criminal record types (e.g., drug, violent, and sex offenses; Ott & McTier, 2019), severities, gender differences at different levels of admissions selectivity, and academic qualifications.

[5]Precisely estimating how many applicants are denied because of their records is difficult because of a lack of reliable data. But based on data from the SUNY system (Rosenthal et al. 2015), arrests and convictions among young people (Brame, Bushway, Paternoster, & Turner, 2014; Hepburn, Kohler-Hausmann, & Zorro Medina, 2019), and felony convictions (Shannon et al., 2017), we estimate that at least 102,000 college applicants were required to report a conviction in 2016. Applying our findings from figure 2, we estimate that at least 25,000 were denied.

Although most of our applications were ultimately accepted in both the record and no-record conditions, we note that it was far more difficult to *complete* applications that requested criminal history information. Higher rejection rates are thus not the only effects of these questions. The Center for Community Alternatives (CCA; Rosenthal et al., 2015; Weissman et al., 2010) has suggested that CHQs on college applications have a chilling effect on applicants with criminal records. CCA analyzed data from nearly half of the institutions in the SUNY system and found that the median attrition rate, or rate at which applicants start but do not finish an application, is almost three times higher for applicants who disclose a criminal record than for the general population. Our experience attempting to comply with the requirements asked of our felony-record applicants confirms this finding: It is fatiguing and frustrating to compile and report all the documentation, information, and one-to-one correspondence required of applicants with records. CCA also compared the attrition rate to the rejection rate for applicants with criminal records and found that for every applicant rejected, 15 applicants dropped out of the application process and did not complete their applications. Although there is no way to assess the extent to which these questions might deter potential applicants with records, the deterrent effect is likely substantial. Although we were diligent in completing the requirements that were feasible within the confines of our field experiment, it is entirely likely that we went far above and beyond what a typical applicant may have been willing to complete. Further examination of the admissions process from the perspective of applicants, including how these questions are interpreted and how the process is navigated, would provide insight into how these questions and associated processes may be indirectly exclusionary.

Our second research question is aimed at whether discrimination on the basis of criminal records varied by race and institutional context. Relative to the large race gaps observed in studies of employment discrimination (Pager, 2007a; Uggen et al., 2014), we find smaller race differences in college admissions. In general, educational institutions responded in similar ways to our applicants, regardless of whether they self-identified as Black or White on their applications. It is impossible to know, however, whether our race signal—checking a box on the application—was itself a strong or salient enough signal or whether colleges admissions officers were privy to race information. Although we did include whether a college considers race in admissions drawn from responses to The College Board's *Annual Survey of Colleges*, we do not know *how* they consider race, particularly at nonelite colleges (kehal, Hirschman, & Berrey, 2018).

Nevertheless, we did observe racial differences when examining the interaction of criminal records with college crime rates. As we had expected, colleges with higher levels of reported crime were more cautious but particularly when reviewing applications from Black males with records, who had the lowest probability of acceptance at higher crime colleges and the largest differential when compared with their counterparts without criminal records. Here, race interacts with institutional context, in the form of campus crime issues, to affect how applicants with records are appraised, and Black applicants. More pronounced race signals and additional institutional settings could provide important insights into how racial identification and perceptions of campus crime operate within the higher education context.

Finally, we considered the likely impact of a BTB strategy to remove criminal records questions from college application forms. The findings from post-BTB analyses of employment reveal that the policy may have unintended consequences, particularly for Black male applicants without criminal records (Agan & Starr, 2018). Our findings indicate that banning the box on college applications would be less likely to produce such large detrimental impacts. Relative to employment audits, we find far less overall racial discrimination in college admissions decisions and small and nonsignificant differences in the appraisal of Black applicants without criminal records in the presence or absence of these questions. We would suggest, however, that any BTB policy—particularly reforms that would delay asking to a later point in the process—must be accompanied by vigilance and monitoring to ensure that prohibitions

on racial discrimination are strictly enforced. Future researchers could pursue an alternative design to address directly the potential impact of BTB by sampling on the criminal history disclosure question. We offer further advice on the modified audit approach in appendix B.

Of course, other strategies are commonly discussed when focusing on the harms of criminal record stigma, including record expungement, sealing, and other similar remedies. We do not, however, emphasize these strategies in the present study because they often require a waiting period of 5 or more years after the end of the sentence before an applicant is eligible to apply. Thus, applicants would have to put off college for a considerable amount of time, often until their late 20s or later, before they could take advantage of these mechanisms. We also do not engage expungements and similar approaches because of their low uptake (see, e.g., Prescott & Starr [2019] who found that only a fraction of people eligible for expungements in Michigan pursued them), the typically narrow scope of eligible criminal histories, and the lack of an organized movement centered on record sealing and college admissibility. We therefore note these as potential remedies but focus more explicitly on the currently popular approaches to removing criminal records questions from admissions. Our study and design are best suited to address the impact of BTB, although subsequent research with an older sample of sealed or expunged testers certainly seems merited.

8 | CONCLUSION

Our results expose a significant aftereffect of criminal justice involvement. Education has classically been viewed as a pathway to escape poverty and enhance social mobility, but it cannot fulfill this function if institutions systematically disqualify applicants disproportionately drawn from communities of color. Although the race differences observed in the educational setting are less pronounced compared with previous audits of employers, we note that the *level* of applicants with records is far greater among Blacks even if the *effects* of these records are roughly proportional to those for Whites.

Furthermore, this study extends prior work on the stigma of criminal records, testing whether it limits educational opportunities as it does in many other areas of life (Pager, 2003; Uggen & Stewart, 2014). Despite high overall acceptance rates for the qualified applicants in our study, we nevertheless observe significant discrimination in higher education admissions. The path toward a postsecondary degree is thus much rockier for the 19 million Americans with felony-level criminal records, a disproportionate proportion of whom are young Black men (Shannon et al., 2017).

These findings also highlight fundamental questions in the sociologies of education and knowledge, providing new evidence and insight regarding the role, duty, and accessibility of higher education institutions. Many colleges claim a commitment to diversity to foster a rich learning environment for students and wide-ranging perspectives in the production of knowledge. By revealing how certain groups are excluded, the findings from this research reveal the limits of the growth and development of knowledge when segments of the population are barred from participating (Uggen, Horowitz, & Stewart, 2017). More broadly, such efforts facilitate understanding of the role of criminal justice institutions in educational attainment.

Finally, this research yields important and actionable information for policy makers and the public. Although more research is clearly needed to explicate the basic findings presented here, we now have clear evidence that colleges that inquire about criminal histories are using them to screen out applicants with records. And, perhaps unsurprisingly, those that do not inquire about this information do not discriminate on the basis of criminal records. For the 70 percent of colleges that request criminal history information on their application forms, it is timely and important that they consider narrowing the scope of such inquiries or removing the question altogether, particularly when it conflicts with

other stated goals of these institutions, including reducing the underrepresentation of men and women of color on campus. Apart from such "upstream" changes in policy, these findings also direct attention "downstream" toward improving the competitiveness of applicants with records—identifying reentry and reintegration strategies to prepare people with records for the application process. Ultimately, we hope that the results of this study can play some small part in facilitating the needed science and policy conversations on the stigmatizing effect of criminal records in higher education.

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AUTHOR BIOGRAPHIES

Robert Stewart is a sociology doctoral candidate at the University of Minnesota. His current work is focused on the intersection of criminal stigma, citizenship, and education.

Christopher Uggen is a Regents professor and the Martindale Chair in Sociology and Law at the University of Minnesota. He is currently writing on reentry from diverse institutions, monetary sanctions, the health effects of incarceration, and genocide.

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APPENDIX A: TESTER NAMES AND RACE

To determine whether the names of our testers would be strongly indicative of the testers' racial identity, we followed the guidance provided by Gaddis (2017). We randomly surveyed 200 workers on Mechanical Turk using a branched survey design and a list of 20 first names: our 4 testers' names, 6 names typically associated with Black men, 6 typically associated with White men, and 4 that were racially ambiguous (figure A1). We asked respondents first whether they associated the name with a particular racial group; if yes, we then asked respondents to identify which racial group (Asian, Black, Latino, White, Other). Those that the respondent did not associate with a specific racial group were categorized as "None," whereas those categorized as Asian, Latino, or Other were collapsed into "Other" for the purposes of figure A1. Although two of our testers' names did seem to have slightly more racial salience, nearly half of our survey respondents did not associate specific race groups with four of our names. Thus, tester names alone are not likely to convey strong racial signals.

APPENDIX B: MODIFIED AUDIT AND FUTURE RESEARCH

Here we offer advice for future researchers about how the modified audit approach used in this study could be adapted to other questions. We came to this topic with two specific questions: What effect does a criminal record have on college admissibility, and to what extent does race mediate that effect? The topic was worthy of investigation, but the context presented a difficult situation for a field experiment that relies on total fabrication. The modified audit is not without its limitations, but we hope the design presented in the main text demonstrates the viability of an experimental approach that foregoes the use of fabricated materials and instead uses real records from real participants. Even though adapting the modified audit requires additional layers of planning and strategizing, it also offers several benefits and opens doors to areas of inquiry that might not otherwise be available. Although outside of the scope of what we present here, using this modified audit design allowed us to collect additional data, in the

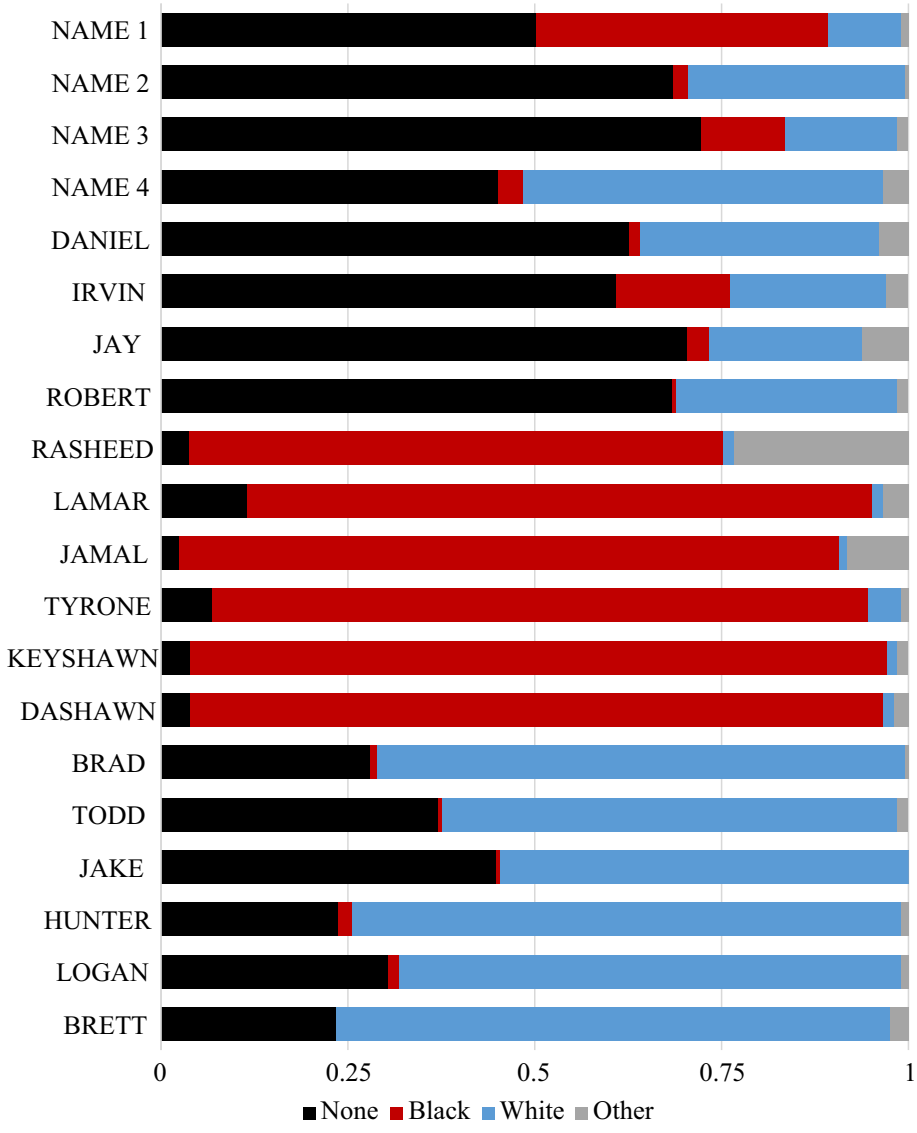

FIGURE A1 Comparison of racial sssociations of tester names by 200 mechanical turk workers [Color figure can be viewed at wileyonlinelibrary.com]

form of thousands of e-mails, postal mail, text messages, and other materials that can be used to answer questions that the quantitative results are not suited for, such as how the application process differs for applicants with records. Here, we offer three pieces of advice to consider when adapting the modified audit to another context.

First, in our experience, this approach requires a significant amount of design decision-making and, thus, requires necessary trade-offs. Researchers will be continually confronted with forks in the road, many of which will be unexpected, and different options or paths will deliver different benefits and drawbacks. To maintain consistency and guide design choices, we suggest future researchers be deliberate in the predesign phase of the project by outlining the goals of the study and processes they are emulating. For example, for this study, we pursued a design that was as realistic as possible,

reflecting the real application experiences and behaviors of applicants with profiles like our testers, while maintaining internal integrity. This informed all of our choices, including how we defined our sampling frame (focusing on 4-year colleges, including colleges that do and do not require criminal history information, not including elite or near-elite colleges), how we conveyed race (checking the race box on the application, not including race-based affinity groups that would likely be unrealistic for students like our testers), and of course the use of real records (average students instead of high-achieving students, low-level felony records that are common for this age group). Although not determinative or fool-proof, establishing goals and ideals ahead of time will provide valuable guidance in the design phase and particularly when in the field and unforeseen circumstances arise.

Second, we suggest thinking through how initial barriers might be turned into advantages. In the college admissions audit, we could not fabricate standardized test scores, high school transcripts, or criminal records, which led us to enlist real participants with real records. Of course, using real records opens this context for study, which would otherwise be inaccessible for a field experiment, and we were far less concerned of arousing suspicion than we would have been if we had used fabricated records. When approaching the matching process, we recognized that it would be impossible to match perfectly any two people's educational backgrounds, but we also had control over how to match our testers. As we could not create perfectly matched pairs, we purposely created mismatched pairs that would thus produce a conservative estimate of criminal record discrimination by intentionally building in differences between our testers within each pair. We did this by ensuring our Record testers were always more qualified students academically within each pair when creating our matches. But an extension of this design could amplify the asymmetry when creating matches to find, for example, how large deficiencies in an applicant's academic background must be to impede college admissibility at levels on par with the discriminatory effects of a criminal record. Moreover, even though the records could not be randomized, we could still randomize elements such as race, essays, and other information that could be similarly leverage in future field experiments.

Finally, we strongly urge future researchers to pay close attention to the ethics involved with using real records from real participants. The protection of the participants who willingly allow their information to be used for research should certainly be prioritized. This includes not just current impacts of the project but also any long-term effects. We took several steps to address these ethical considerations. When presenting the project to potential participants, we spent a lot of time discussing the possible ramifications (e.g., if a participant decided to apply at one of the colleges in the sample at some point in the future). This in part motivated us not to include colleges in their home state in our sample. Although our participants told us that they were not planning on moving out of state or going to college any time soon, we nevertheless established contingency plans if problems arose. Moreover, we sought to mitigate future issues by withdrawing our applicants' applications immediately after receiving any notifications of acceptance while in the field because colleges typically only retain admissions records of applicants who do not attend for a few years. This strategy also ensured that our testers would not be taking a seat that could have otherwise gone to another student.