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Evaluating the Effectiveness of Ohio's Certificate of Relief

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EVALUATING THE EFFECTIVENESS OF OHIO'S CERTIFICATE OF RELIEF

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ABSTRACT

Employment has been cited as a factor that can aid one's desistance from criminal activity. However, research has consistently demonstrated that those with criminal history face significant barriers to securing employment. In recognition of this problem, most states have implemented various rights restoration mechanisms aimed to increase employment opportunities for ex-offenders. One of these mechanisms, the certificate of relief, aims to aid ex-offenders in their job search by ensuring employers that certificate holders are not a safety risk, providing employers with negligent hiring immunity, and removing occupational licensing bans. A handful of studies have examined whether this mechanism improved hiring outcomes for ex-offenders, but these studies produced mixed results and suffered from important methodological limitations. The goal of the current study was to address the limitations of previous research to provide a more comprehensive test of one state's (Ohio) certificate. This goal was achieved with the use of two field experiments. Both experiments utilized a correspondence approach where hypothetical applicants submitted resumes to entry-level job postings. The first portion of the study utilized a mixed experimental design that included a within-subject criminal record variable and a between-subject race variable. The second portion of the study utilized a between-subjects experimental design that included a between-subjects criminal record variable and a between-subjects race variable. Results showed that certificate holders received significantly fewer callbacks for interviews than those with no criminal record. Results also showed that certificate holders fared no better in terms of callbacks than those with a criminal record and no certificate. Further, African American applicants received significantly fewer callbacks than White Applicants in all criminal record categories. These results were supported in several robustness checks. Policy implications of these findings are discussed in detail along with study limitations, directions for future research, and technical notes on correspondence studies.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

LIST OF SYMBOLS

p	Probability of finding that observed results are correct, assuming the null hypothesis is true.
n	Sample size.
z	A value used to measure the amount of standard deviation from the mean.
*	Statistically (or marginally) significant using a one-sided test.

LIST OF ABBREVIATIONS

AIC .... Akaike Information Criterion

BIC .... Bayesian Information Criterion

BTB ....Ban-The-Box

CI...Confidence Interval

CQE..Certificate of Qualification for Employment

EEOC ....Equal Employment Opportunity Commission

OR .... Odds Ratio

SE ....Standard Error

CHAPTER 1

INTRODUCTION

The number of those with some sort of criminal record now stands at approximately 85 million (Bureau of Justice Statistics, 2015). Such numbers are of crucial importance when considering the issue of collateral consequences. A collateral consequence can be defined as any "legal penalty, disability or disadvantage, however denominated, that is imposed on a person automatically upon that person's conviction for a felony, misdemeanor or other offense, even if it is not included in the sentence" (American Bar Association, 2004, p. 15). It is also important to note that collateral consequences can emanate from early processes such as arrest (see Uggen, Vuolo, Lageson, Ruhland, & Whitham, 2014).

There is now a growing body of literature examining various collateral consequences and their impacts on ex-offenders (see Gunnision & Helfgott, 2013; Love, Roberts, & Klingele, 2013; Petersilia 2003; Travis, 2005 for reviews). For example, those with criminal records face difficulties in securing housing, finding employment, participating in civic opportunities such as voting and jury service, securing occupational licenses, and dealing with stigma itself (Gunnision & Helfgott, 2013; Love et al., 2013; Petersilia 2003; Travis, 2005). Though each of these collateral consequences are important, ex-offenders and other related parties such as probation and parole officers consistently cite that one of the most punitive collateral consequences is the barrier to employment arising from criminal record stigma (Bahr, Harris, Fisher, & Armstrong, 2010; Garland, Wodahl, & Mayfield, 2010; O'Brien, 2011; Ray, Grommon, & Rydberg, 2016; Western, Braga, Davis, & Sirois, 2015). These studies demonstrate that ex-offenders face significant barriers seeking employment (e.g., lack of ability to travel), being hired, and being promoted to better paying jobs (Bahr et al., 2010; Garland et al., 2010; O'Brien, 2011; Pager, 2003; Ray et al., 2016; Western et al., 2015).

Such findings are of crucial importance here because employment is a key factor for desistance from criminal behavior (Baron, 2008; Laub & Sampson, 2003; Tripodi, Kim, & Bender, 2010; Verbruggen, Blokland, & Van der Geest, 2012; Wang, Mears, & Bales, 2010; Wright & Cullen, 2004; and see Lageson & Uggen, 2013; Uggen & Wakefield, 2008 for thorough reviews). Authors of such research argue that employment provides a pro-social bond and also economic resources needed to secure fundamental necessities and maintain positive relationships (Berg & Huebner, 2011; Petersilia, 2003; Travis, 2005).

Recognizing the barriers created from collateral consequences, all jurisdictions have created collateral consequence relief mechanisms meant to provide some sort of collateral sanction relief (Collateral Consequence Resource Center, 2017). One of the newest mechanisms, specifically created to combat collateral consequences related to employment, is the certificate of relief (sometimes called certificate of recovery or certificate of qualification for employment) (Love, 2011; Love & Frazier, 2006; O.R.C. 2953.25). Certificates of relief are intended to aid ex-offenders in their job search by providing a stamp of good character/employability from a court, lifting occupational licensing restrictions, and sometimes providing tort immunities to employers (Love, 2006; Love & Frazier, 2006; O.R.C. 2953.25). Only two studies have attempted to examine the effectiveness of these certificates, with one finding that they were effective in improving hiring outcomes (Leasure & Andersen, 2016) and the other finding that they were not (Leasure & Andersen, 2019).

The purpose of this study was to provide a further test of the effectiveness of certificates of relief with the use of two field experiments. This study built upon previous research in several ways. First, previous research in this area largely focused on perceptions of effectiveness and the process for securing a certificate of relief. Like Leasure and Andersen (2016, 2019), this study provides an actual test of the statute's effectiveness. Second, this study built upon Leasure and Andersen (2016, 2019) by testing an amended version of Ohio's certificate of relief which is theoretically more beneficial to ex-offenders (see Ohio Rev. Code Ann. § 2953.25(D)(2)).

Third, previous research only examined the effectiveness of certificates of relief for those possessing a drug conviction. This study used a criminal record condition that contained convictions of varying crime types (drug and theft). Relatedly, previous research only examined the impact of a single conviction. This study used a criminal history that included previous convictions. Including multiple and previous convictions was important, as previous research showed that most offenders have previous criminal convictions (Beck, 1993; Cuyahoga Intake, 2014) and that many of these offenders do not specialize in one particular crime-type (Cuyahoga Intake, 2014; Piquero, Farrington, & Blumstein, 2007).

Fourth, Leasure and Andersen (2016, 2019) included few variables in their robustness checks (only job type in the 2016, 2019 study). This study tested the robustness of its results primary results by including several control variables in later sensitivity analyses (see Uggen et al., 2014 for a similar approach).

Fifth, the hypothetical applicants in Leasure and Andersen (2016, 2019) did not have official certificates of relief and were absent from an online list of current Ohio certificate holders. This study was conducted in collaboration with the State of Ohio, and hypothetical applicants possessed official certificates. Further, hypothetical applicant names were also added to the online list of current certificate holders.

Finally, this study tested the geographic generalizability of certificates of relief by using Cleveland, Ohio for data collection. Ohio was chosen because of its recent implementation of the CQE legislation and because this state consistently has one the highest number of individuals under correctional supervision (Kaeble & Glaze, 2016). Cleveland was elected as this jurisdiction consistently produced more individuals coming under correctional supervision than any other in Ohio (Bennie, 2017).

Such a study is important for several reasons. First, certificates of relief are becoming a popular legislative collateral consequence relief instrument (Garretson, 2016). This is so largely because these mechanisms provide legislators an intermediate collateral consequence relief mechanism (i.e., one that does not permanently seal the offender's criminal history such as expungement) (Garretson, 2016). Because of their growing popularity, it is crucial to test the effectiveness of these mechanisms to justify their current forms. Second, as noted above, the only two tests of these certificates produced mixed results (Leasure & Andersen, 2016, 2019). We should remember the cautionary lessons from early experimental studies on domestic violence mandatory arrests and scared straight programs which had a large influence on policy, but were unsuccessfully replicated (e.g., Berk, Campbell, Klap, & Western, 1992; Petrosino, Turpin-Petrosino, & Buehler, 2003). Therefore, as employment is a critical factor which can aid desistance from criminal behavior (Bahr et al. 2010; Bushway & Apel, 2012; Garland et al. 2010; O'Brien 2011; Ray et al. 2016; Uggen 2000; Western et al. 2015), further study is necessary to account for the disparate findings in Leasure and Andersen (2019) and to determine which, if any, of the previous findings are supported.

The implications of this study can inform policymakers on several accounts. First, as the early tests of this mechanism produced mixed results, the findings of this study can be used to determine whether certificates should be supplemented or modified. Relatedly, should this study find that certificates are effective in improving employment outcomes, such results could influence other jurisdictions to create similar mechanisms. Finally, should this study find that certificates are not effective in improving early employment outcomes, such results could be used to justify discarding these mechanisms in favor of tools such as pardon and expungement, which can completely seal one's criminal history, or other tools that are meant to increase employer incentive for hiring ex-offenders such as the Work Opportunity Tax Credit.

This study continues with Chapter 2 which provides a detailed review of literature and the research questions for the current study. Specifically, Chapter 2 explores the relationship among employment, offending, and desistance; the proliferation of criminal records; the barriers to employment created by criminal records; collateral consequence relief mechanisms; the theoretical framework for the current study; and proposed research hypotheses. Next, Chapter 3 details the specific methodology of the study including the type of design, context, design specifics, and analytic approach. Chapter 4 presents the results of the two field experiments. Chapter 5 discusses the results in relation to previous certificate research and also policy and theoretical implications. Chapter 6 presents the conclusion.

CHAPTER 2

REVIEW OF THE LITERATURE

2.1 THE IMPORTANCE OF RESEARCHING EMPLOYMENT OUTCOMES FOR THOSE WITH CRIMINAL RECORDS

Exploring employment outcomes for those with criminal records is an important area of research for several reasons. First, employment provides ex-offenders, a group who has generally not accumulated substantial assets, with the economic resources needed to secure fundamental necessities such as housing and food (Austin & Irwin, 1990; Petersilia, 2003; Travis, 2005). Second, research has generally found that employment is related to lower rates of recidivism (Baron, 2008; Tripodi et al., 2010; Verbruggen et al, 2012; Wang et al, 2010; Wright & Cullen, 2004; and see Uggen & Wakefield, 2008 for a thorough review). For example, using longitudinal data that tracked 1,000 boys, Laub and Sampson (2003) found that adult employment (particularly stable jobs) was associated with a lower likelihood of re-offending.

However, studies have found differing impacts of employment on recidivism by gender and job quality. For example, Denver, Siwach, and Bushway (2017a) examined hazard rates for ex-offenders seeking to work in healthcare for both men and women. The authors found that those who had secured employment in this industry had an overall 2.2% decrease likelihood of re-offending after one year, and a 4.2% decrease after three years. They also found that men were 8.4% less likely to be arrested over the three year period, while women were 2.4% less likely to be arrested during this period. Further, Uggen (1999) found that those who held a higher quality job (quality was measured by type of occupation, skill level, and industry) were less likely to offend than those with a lower quality job (see also Allan & Steffensmeier, 1989; but see Bunting, Staton, Winston, & Pangburn, 2019 finding no significant difference in recidivism rates between those in full-time and part-time employment).

Finally, employment is often cited by ex-offenders, scholars, and practitioners such as probation/parole officers as one of the most important components to a successful reentry (Bahr et al., 2010; Francis, 2018; O'Brien, 2001; Petersilia, 2005; Ray et al., 2016; Western et al., 2015). For example, Bahr and colleagues (2010) interviewed 51 parolees over three years following their release from prison. Many of the ex-offenders in that study noted that securing a level of employment that would enable them to support themselves or their families was a major concern during their reentry.

2.2 THE NUMBER OF INDIVIDUALS POSSESSING CRIMINAL RECORDS

The number of those possessing a criminal record in the U.S. is staggering. In fact, some estimate that approximately 85 million individuals have some form of a criminal record (Bureau of Justice Statistics, 2015). Focusing on arrests, Brame, Bushway, Paternoster, and Turner (2014) found that approximately 49% of African American males were arrested at least once by age 23. The authors found that approximately 38% of White males had experienced an arrest by that age. Examining convictions, Shannon and colleagues (2017) showed that people with felony convictions account for 8% of all adults and 23% of the African American adult population (see also

Uggen, Manza, & Thompson, 2006 finding that those with felonies account for 33.4% of the African American male adult population).

State level data show a similar pattern. For example, previous research showed that one in six Ohioans, over 1.9 million people, had a felony or misdemeanor record (Ohio Poverty Law Center, 2017). Interestingly, recent research which disentangled felony and misdemeanor convictions found that a large number of those criminal records come from misdemeanors, rather than felonies (Hepburn, Kohler-Hausmann, & Medina, 2019). In fact, those authors found that, depending on the cohort, between 34 % and 83 % of individuals located in New York City were convicted of a misdemeanor and never convicted of a felony (Hepburn, Kohler-Hausmann, & Medina, 2019).

2.3 BARRIERS TO EMPLOYMENT

A. Access and Use of Criminal Records by Employers

Coupled with the vast increase in the number of those possessing criminal records is the fact that such records now play a large role in the hiring process (see Levashina & Campion, 2009 for a review). While there are absolute prohibitions against considering race and gender as factors in hiring decisions (Title VII of the Civil Rights Act of 1964), employers are granted a great deal more latitude to consider one's criminal history (see El v. Southeastern Pennsylvania Transportation Authority, No. 05-3857 (2007). However, the Equal Employment Opportunity Commission (EEOC) and subsequent federal case law has set some limitations (42 U.S.C. §2000e-5(a)).

For example, the Federal Eighth Circuit Court stated that an employer violates Title VII when the potential employee with a criminal record demonstrates that the employer's neutral policy or practice has the effect (disparate impact) of disproportionately screening out a protected group, and the employer fails to demonstrate that the policy or practice is job related for the position in question and consistent with business necessity (*Green v. Missouri Pacific Railroad,* 549 F.2d 1158 (8th Cir. 1977; see also Levashina & Campion, 2009 arguing that the scope of background checks should be tailored for each position). In *Green*, the court used three factors to determine whether an exclusion based upon a criminal record is job-related for the position in question and consistent with business necessity. First, the nature and gravity of the offense must be considered. Second, the amount of time that has passed since the offense (and sanction completion) must be considered. And third, the type of employment position must be considered in light of the previous two factors.

The EEOC has also specified two circumstances in which employers will consistently meet the "job related and consistent with business necessity" requirement. The first circumstance arises when the criminal conduct screen is conducted pursuant to the 2010 Uniform Guidelines on Employee Selection Procedures. The second circumstance arises when the employer has developed a targeted criminal conduct screen that takes into consideration the *Green* factors and also provides an opportunity for an individualized assessment to determine whether the policy as applied is job-related and consistent with business necessity (EEOC Enforcement Guide, 2012).

Further, because numerous private companies now exist to provide quick online viewing of criminal records (Jacobs, 2005; Roberts, 2015), the government saw fit to regulate such background checks via the Fair Credit Reporting Act (FCRA) (15 U.S.C. § 1681 et seq). This act governs criminal records checks that are conducted by a credit reporting agency (CRA). The act states that a CRA may not report arrests which are more than seven years old, unless the underlying position has an annual salary of $75,000 or more (15 U.S.C. §§ 1681c(a)(5), 1681c(b)(3)). Further, CRAs must use "reasonable procedures" to insure "maximum possible accuracy" (15 U.S.C. §1681e(b)). A CRA cannot report information which "is likely to have an adverse effect on the consumer's ability to obtain employment" to an employer before it either notifies the applicant of its findings (so that they can be contested if erroneous) or unless strict procedures are already in place to ensure that the information it reports is accurate (15 U.S.C. §1681k). Finally, if a CRA fails to follow any of the FCRA provisions, a cause of action may be created for the applicant.[1]

There are also some limitations at the state level. In Ohio, employers cannot question an applicant about an expunged juvenile arrest record (Ohio Rev. Code Ann. § 2151.358(I)). Additionally, the Ohio Civil Rights Commission's Pre-Employment Inquiry Guide cautions that employers should avoid any inquiry that would reveal an arrest without a conviction, unless a bona fide occupational qualification is certified in advance by the Commission (Ohio Rev. Code Ann. § 4112.02). Employers also cannot question applicants about sealed convictions unless the question bears a direct and substantial relationship to the position for which the person is being considered (Ohio Rev. Code Ann. §§ 2953.32, 2953.33, 2953.55).

Some research also examines the interpretation and application of rules that govern employer responsibilities regarding applicant criminal history in the hiring process. Such research helps to ensure that fair hiring laws are being applied uniformly

[1] See Philin v. Trans Union Corp., 101 F. 3d 957, 963 (3rd Cir. 1996) for required elements for a cause of action under this provision.

and fairly. For example, one study found that some organizations set explicit standards to guide hiring decisions relating to criminal records, while others use a more informal approach and turn to a micro-rational decision process (Lageson, Vuolo, & Uggen, 2015). In another study, an author found that 33% of surveyed employers ignored requirements regarding criminal record questions in the hiring process (Day, 2019).

Given the inconsistencies in how employers consider criminal record issues, some authors are now providing employers with practical frameworks which help employers make better informed decisions about candidates with criminal history. For example, some researchers used an actuarial method which attempted to predict a candidates' risk of future offending. The first of these studies compared the risk of re-offense over time of a group of ex-offenders to a group of individuals in the general population (Blumstein & Nakamura, 2009; Kurlycheck, Brame, & Bushway, 2006, 2007). However, a later study by DeWitt and Colleagues (2017) argued that ex-offenders should be compared with non-offenders who were applying to the same types of jobs. DeWitt and Colleagues (2017) argued that using the general population as a comparison group would result in many of those with no records having an inflated arrest risk. Proponents of the actuarial approach argue that such methods would allow ex-offender hiring frameworks to be evaluated and critiqued (Gottfredson, 2017). However, Nakamura (2017) cautioned that actuarial systems are problematic because they assign risk to even those with no records, meaning some life-time non-offenders would be denied employment due to an actuarial prediction.

Regardless of the company's approach to dealing with criminal history, subsequent case law shows that employers are granted a wide amount of discretion when deciding to exclude an employee because of their criminal record. For example, in one case, a man who had been hired and who was satisfactorily performing his duties was dismissed from a job solely because of a company's discovery of a 40 year old conviction (see El v. Southeastern Pennsylvania Transportation Authority, No. 05-3857 (2007)).

Despite limitations on the use of criminal records in employment decisions, research shows that a majority of employers ask about criminal history in the hiring process (Bushway 2004; Duane, La Vigne, Lynch, & Reimal, 2017; Freeman, 2008; Holzer & Stoll, 2001; Martin, Huffman, Koons-Witt, & Brame, 2019; Mukamal & Samuels 2002; Vuolo, Lageson, & Uggen, 2017). For example, one study found that nearly 90% of organizations under study conducted criminal background checks on at least some job candidates, and nearly 70% reported conducting criminal background checks on all job candidates (Society for Human Resource Management, 2012). Further, Denver, Pickett, and Bushway (2018) used a national probability sample and found that over 31 million U.S. adults were asked about a criminal record during the application process in 2016. According to their survey, virtually all of the criminal record inquiries occurred at the application stage.

These results could be explained by survey research which explored employer reasoning related to ex-offender hiring and background check practices. For example, research shows that employers were concerned about negligent hiring laws which could hold employers liable for employees' actions in the workplace (Pager, 2007a; Levashina & Campion, 2009). Further, employers are concerned about the safety, security, and comfort of other employees (Giguere & Dundes, 2002; Harris & Keller, 2005; Pager, 2007a). Finally, employers fear that if any incident were to occur, such negative publicity could affect the reputation of the company (Giguere & Dundes, 2002).

B. The Impact of a Criminal Record on Employment

Criminal records can automatically disqualify a person for many state, municipal, federal, and private jobs as well as certain occupational and professional licenses (Love et. al., 2013; Zhang, 2018).[2] Even if not absolutely disqualifying, the above literature shows that both federal and state law permits the consideration of a criminal record for purposes of hiring decisions in many occupations (Love et. al., 2013). Given that such information can be considered, research has consistently found that contact with the criminal justice system leads to worse employment opportunities (Ahmed & Lang, 2017; Griffith, Rade, & Anazodo, 2019; Holzer, 2007; Pager, 2008). Such findings help explain one state's ex-offender unemployment rate which was nearly eight times that of the general population (Nally, Lockwood, & Ho, 2011). The specific outcomes under study have been one's earning potential and early employment outcomes such as being hired or being invited to continue in the hiring process.

As to the first outcome, various studies show that an ex-offender's employment earnings are significantly lower than those without a criminal record or even other traditionally socially-disadvantaged groups (Freeman, 1991; Grogger, 1995; Harding, Siegal, & Morenoff, 2017; Lyons & Pettit, 2011; Nagin & Waldfogel, 1998; Waldfogel, 1994). For example, Western (2002) used the National Longitudinal Survey of Youth (NLSY) and found that overall wage levels were about 16% lower for offenders compared to non-offenders (see also Raphael, 2007 with similar results). Nagin and Waldfogel (1998) used federal offender data and found that even a first-time conviction

[2] See Love et al (2013) and the National Inventory of Collateral Consequences for a comprehensive list of disqualifications and hindrances.

had a large negative and significant impact on wage amounts for offenders over age 30 (see also Grogger, 1995 finding similar results with state administrative data and unemployment insurance earnings data). Western and Sirois (2018) survey data from a Boston, Massachusetts reentry program and found that average ex-offender earnings were below the poverty level. Interestingly, some studies find a slight increase in earnings immediately after incarceration (Harding, Siegal, and Morenoff, 2017; Kling, 2006; Sabol, 2007). However, this increase dissipates over time. Further, one recent study suggests that wage differences are not as pronounced with offenders who have been arrested and not convicted (Apel & Powell, 2019).

As to early employment outcomes such as being hired or being invited to continue in the hiring process, numerous studies of various designs have found that ex-offenders are less likely to be hired or called back for an interview. The first set of studies discussed here used survey or administrative data. For example, Western and Sirois (2018) found that approximately half of their sample of reentering offenders was unemployed during the period of study. Using data from the NLSY and comparing offenders with non-offenders, Freeman (1991) found that incarceration reduced employment by approximately 20 to 40% (see also Carter, 2019; Curcio & Pattavina, 2018; Grogger, 1992). Grogger (1995), as noted above, used state administrative data and unemployment insurance data and found that criminal justice contact (arrest, jail/prison, conviction) reduced an offender's employment rate by about 8%. As with earnings, some studies show a slight increase in employment immediately after incarceration (Sabol, 2007). However, like the increase in earnings, the upturn in employment immediately after incarceration dissipated over time. One recent study by Harding and colleagues (2018) utilized a natural experiment (randomization of judges to criminal cases) to examine the impact of incarceration and probation on employment. The authors found that both sanctions significantly reduced employment. Interestingly, the authors also found that those sentenced to incarceration had higher levels of employment than those sentenced to probation.

Qualitative research has examined the experiences of ex-offenders and those associated with ex-offenders (parole/probation officers and social workers) during reentry. This line of research has largely found that employment is a highly important factor to a successful reentry and also that a criminal record is a major hindrance to securing employment (Bahr, Harris, Fisher, Armstrong, 2010; Gunnison & Helfgott, 2011; Garland, Wodahl, & Mayfield, 2010; Ispa-Landa & Loeffler, 2016; O'Brien, 2011; Palmer & Christian, 2019; Ray, Grommon, & Rydberg, 2016; Sviridoff & Thompson, 1983; Walter, Caudy, & Ray, 2016).

Survey research has also found that employers are much less likely to hire those possessing a criminal history (Holzer, 2007; Kuhn, 2019; Pager & Quillian 2005). The majority of these surveys were collected by Holzer and colleagues in 1996 and 2007. The 1996 survey was distributed to about 3,000 employers in Atlanta, Boston, Detroit, and Los Angeles from 1992 to 1994, with a follow-up survey being distributed to 600 Los Angeles employers in 2001 (Holzer, 2007). The surveys asked employers about their willingness to hire ex-offenders compared to other disadvantaged groups (welfare recipients, poor workers) and also about their perceptions of ex-offender workers (Holzer, 2007). Approximately 40% of employers stated that they would "definitely" or "probably" hire applicants with criminal records (Holzer, 2007). However, approximately

80-90% of the same employers stated that they would hire welfare recipients or workers with spotty work experience (Holzer, 2007; see also Holzer and Stoll (2001) with similar results in Cleveland, Ohio). Other research suggests that employers are less willing to hire ex-offenders who have more recent, severe, and job-related criminal history (Albright & Denq, 1996; Kuhn, 2019).

However, a sizable limitation with survey research is that it only gauges an employer's opinion on their likelihood of hiring ex-offenders rather than actual hiring practices. In fact, Pager and Quillian (2005) found that employers were just as likely to hire African American ex-offenders as White ex-offenders when using a survey design. However, Pager's (2003) experimental audit study (discussed below) comprised these same employers, and employers were significantly less likely to hire African American applicants with criminal records.

Because of the methodological issues with survey studies seeking to predict hiring practices, several sought to employ experimental designs to examine the impact of a criminal record on hiring. In one of the first of these experimental studies, Schwartz and Skolnick (1962) utilized an experimental correspondence approach (presenting employers with fictitious resumes and gauging positive responses such as callbacks for interviews) with the following treatment conditions: assault conviction, assault acquittal, assault acquittal and letter from a judge, and one with no criminal record. The results of this study were as follows: no record (56%); acquitted with letter (32%); acquitted without letter (14%); conviction (4%).

Pager's (2003) study was perhaps the most noteworthy in this area as it showed the negative effects of criminal record stigma combined with racial stigma and she was the first to do so using an in-person audit approach (sending pairs of actual testers to a location to apply for jobs in person, while randomly varying a treatment condition). Pager (2003) and its progeny included race variables given the plethora of research showing that African Americans were consistently less likely to advance in the hiring process than equally situated Whites (see Bendick, Jackson, & Reinoso, 1994; Bertrand & Mullainathan, 2004). For example, Zschirnt and Ruedin (2016) utilized meta-analysis of 42 separate correspondence studies from 1990 to 2015 to examine discrimination in hiring practices and found that discrimination against minorities was present across time, jurisdiction, gender, and economic contexts (see also the meta-analysis by Quillian, Pager, Hexel, & Midtboen, 2017, finding no change in the levels of discrimination against African Americans since 1989, but some evidence of declining discrimination against Latinos).

Pager (2003) sent actual White and African American male testers to apply for entry-level positions in Milwaukee, Wisconsin. The testers conveyed an incarceration criminal history by indicating prison work on a resume, by noting a parole officer reference on a resume, or most commonly by noting a cocaine conviction with an 18-month sentence on an application. Pager's (2003) results showed that blacks without criminal records received callbacks at only 14% while whites in the same group received callbacks at 34%. Further, blacks with a criminal record received callbacks at 5% while whites of the same group received callbacks at 17%. This means that whites with a criminal record received more callbacks than equally situated blacks with no criminal record (this result was not statistically significant).

Pager's (2003) original findings have largely been replicated in later studies conducted in different jurisdictions. For example, conducted in Arizona using a correspondence and in-person audit study, Decker, Ortiz, Spohn, & Hedberg (2015) found the following callback percentages for White, Hispanic, and African American testers with a high school diploma: White no record (8.4% correspondence, 30.8% in-person); White with record (8.5% correspondence, 14.3% in-person); Hispanic no record (8.4% correspondence, 0.0% in-person); Hispanic with record (8.2% correspondence, 0.0% in-person); African American no record (6.5% correspondence, 21.1% in-person); (African American with record (6.2% correspondence, 12% in-person).

Conducted in New York City, Pager, Bonikowski, and Western (2009a) employed an in-person audit study which used White testers with a criminal record and African American and Hispanic testers with no criminal record. The authors found the following positive callback rates: White felon (17.2%); Latino no record (15.4%); African American no record (13%). During the same data collection project, Pager, Western, and Sugie (2009b) employed an in-person audit study that examined differences between Whites with and without criminal records and African Americans with and without criminal records. The authors found the following positive callback rates: White no record (31%); White record (22%); African American no record (25%); African American record (10%). Interestingly, the authors also found that applicants who interact with the hiring authority at the job location were 4-6 times more likely to receive a callback or job offer.

In Minnesota, Uggen and colleagues (2014) conducted an in-person audit study to examine the effect of a low-level arrest (disorderly conduct misdemeanor) on hiring outcomes. White and African American testers were sent to 300 employers. The callback rate for the arrest treatment (34.7% for White applicants and 23.5% for African American applicants) was about 4 percentage points lower (non-significant) than the no record group (38.8% for White applicants and 27.5% for African American applicants). The differences in callbacks by race were statistically significant. Finally, as found by Pager and colleagues (2009b), applicants who came into contact with the hiring authority at the job location were 6-10 times more likely to receive a callback.

Agan and Starr (2017a) used a correspondence approach that focused on entry-level positions in the chain restaurant and retail sectors in New York and New Jersey. The authors found that those with a criminal record (non-violent drug and property offenses) were 60% less likely (statistically significant) to receive callbacks than those without criminal records (13.6% versus 8.5%). While the authors tested for racial differences in callback rates, no statistically significant or substantive differences were found.

Leasure and Andersen (2017) used an experimental correspondence study which was conducted in Ohio's entry-level employment market in 2015. Their results showed that 28.97% of those with no criminal record received a callback for an interview, 19.15% of those with ten-year-old drug convictions were called back, and that 9.8% of those with one-year-old drug convictions were called back. The difference between the no record group and the one-year-old conviction was statistically significant. The difference between the one and ten-year-old felony groups was marginally significant. A limitation of this study was that it used only White applicants.

Expectedly, employer reasoning for disfavoring ex-offenders is similar to reasoning for instituting background checks. Again. those reasons included fears of negligent hiring liability, employee and customer safety, and company reputation (Bushway et al., 2007; Giguere & Dundes, 2002; Harris & Keller, 2005; Levashina & Campion, 2009; Pager, 2007a). However, some research showed that employers questioned whether ex-offenders have the skills to perform adequately in the position or if they have the commitment to remain in the position (Bushway, 2004; Graffam, Shinkfield, & Hardcastle, 2008).

2.4 MECHANISMS THAT REDUCE THE NEGATIVE IMPACT OF A CRIMINAL RECORD ON EMPLOYMENT

As noted above, offenders and other involved parties consistently identify employment as both difficult to acquire and a major factor for successful reentry (Bahr et al. 2010; Garland et al. 2010; Gunnison & Helfgott 2011; Ispa-Landa & Loeffler, 2016; O'Brien, 2001; Western et al. 2015). In recognition of such findings, researchers have sought to identify mechanisms that could reduce the negative impact of a criminal record on employment opportunities. In addition to this research, a majority of jurisdictions have created mechanisms aimed at relieving the barriers to employment created by criminal record stigma (Love, 2011; Subramanian, Moreno, & Gebresellassie, 2014).[3] The following two sections provide a detailed discussion of both formal and informal mechanisms that were created or shown to improve employment opportunities for ex-offenders. A formal mechanism is one that is legislatively created to address collateral

[3] Though rights restoration mechanisms are available in all jurisdictions, one recent study which surveyed South Carolina employers found little awareness of programs like Work Opportunity Tax Credit and Federal Bonding (Martin et al., 2019; see also Visher et al., 2008, 2011).

consequences such as employment. Examples include pardon, expungement, and certificates of relief. An informal mechanism is any non-legislative tool that could be used to improve employment outcomes for ex-offenders. Examples include education, work history, and reentry programming.

A. Informal Mechanisms

Some research has shown that education improves employment outcomes for ex-offenders. Cundiff (2016) utilized a correspondence study that employed testers with and without criminal records (drug convictions) who possessed either a high school diploma, an associate's degree, or a bachelor's degree. The study focused on entry-level employers and sought to determine whether callbacks for interviews would increase for ex-offender job seekers as their level of education increased. The study did not examine racial differences. The results were as follows: high school with record (4%); high school without record (16%); associate's with record (8%); associate's without record (22%); bachelor's with record (22%); bachelor's without record (24%). Regarding the first two comparisons (ex-offender and no criminal record in the high school and associate's degree groups), the differences were statistically significant. These results show that the impact of a criminal record could be diminished once a bachelor's degree is achieved (see also Albright & Denq, 1996 conducting a survey of employers and finding an increased willingness to hire ex-offenders who earned a college degree while incarcerated).

Reich (2017) used an experimental factorial vignette design which was sent to employers to determine their willingness to hire various candidates. Some of the vignettes listed criminal records but also soft and hard skills. Examples of soft skills were being well dressed and having a positive attitude, while examples of hard skills were previous work experience, job training, and a certificate from a court stating that the individual was rehabilitated. The results showed that the possession of soft and hard skills were significantly and positively associated with willingness to hire offenders. Similarly, research shows that incarceration and post-incarceration employment stints increase an employer's willingness to hire ex-offenders (Holzer, 2007) and increase an ex-offender's likelihood of being employed (Flatt & Jacobs, 2018; Visher et al., 2008, 2011). Other qualitative and survey research has found that volunteer work and third parties/previous employers who "vouch" for the ex-offender can be factors that increase an employer's willingness to hire one with a criminal record (DeWitt & Denver, 2019; Cherney & Fitzgerald, 2016; see also Holzer, Raphael, & Stoll, 2002).

While many studies have failed to find that job training programs increase employment for ex-offenders (see Bushway & Apel, 2012; Davis et al., 2013 for reviews), some studies have found a higher likelihood of employment for ex-offenders who have completed job training (Albright & Denq, 1996; Baloch & Jennings, 2018; Saylor & Gaes, 1997). For example, one study using a focus group of employers found that employers were 90% more likely to hire an individual with a criminal record if they had successfully completed a reentry work program (Fahey, Roberts, & Engel, 2006). Similarly, Visher and colleagues (2008, 2011) found that those who were incarcerated and participated in trade or employment readiness training were more likely to be employed eight months post-release. Finally, Formon and Colleagues (2018) found that ex-offender graduates of community-based job training programs obtained employment at equal rates of non ex-offenders.

In a comprehensive effort to identify mechanisms that improve employment outcomes for ex-offenders, Denver (2019) used state-mandated criminal background checks in the New York health care industry. Applicants in the sample received an initial denial and petitioned for reconsideration to submit evidence of rehabilitation. Denver (2019) sought to examine the various methods that applicants demonstrated rehabilitation to determine if any particular method was more likely to predict approval. Denver (2019) found that prior employer recommendations, program completions (reentry program, drug/alcohol rehabilitation program, or anger rehabilitation program), and "other"[4] evidence were positively correlated with clearance to work. This result was similar to the findings of Denver and Ewald (2018) which showed the importance of prior employer recommendations after examining judicial licensing decisions for ex-offender applicants seeking to become unarmed security guards in New York State.

B. Formal Mechanisms

A pardon or clemency is an executive device which can certify one's character, clear a criminal record, and or remove automatic restrictions (Love, 2011). Jurisdictions differ in what types of collateral consequence relief that pardons can provide. For example, in Ohio, a pardon provides a stamp of good character and can remove many automatic restrictions that are imposed upon conviction (State v. Boykin, 2013). However, the granting of an Ohio pardon does not automatically qualify an ex-offender for expungement (Restoration of Rights Project, 2018). In states such as Arkansas, a pardon does automatically qualify an ex-offender for expungement (Restoration of Rights Project, 2018).

[4] The "other" category was not defined in the study.

In many jurisdictions, a pardon is the only available collateral consequence relief mechanism (Love, 2011). Unfortunately, it is argued that our current political landscape has rendered pardon a "phantom" remedy because politicians avoid granting such relief for political expediency (Love 2011). Further, some also note that the pardon process can be difficult to navigate for the average reentering offender (Love, 2011).

Expungement, which is available in about half of U.S. states, seals an offender's criminal record and is usually for low level and first time offenders (Murray, 2016). Unfortunately, technology has relegated expungement to a near ineffective status (Jacobs, 2005). Roberts (2015) notes that the overall effects of expungement will be quite limited in terms of actually clearing one's record because of the proliferation of online record depositories who do not conform to sealing requirements. Several public internet companies that offer access to criminal records are not covered by expungement laws as such laws usually only require government agencies to seal the offender's criminal record (Love, 2011). Further, sealed records can remain available to the public for ex-offenders aiming to work with children, the elderly, and other vulnerable populations (Love, 2011). To demonstrate the potential importance of expungement, Adams et al. (2016) used semi-structured interviews with 40 past offenders to examine the expectations of individuals who seek record clearance and the extent to which completion of the process facilitates efforts to reintegrate into society and desist from crime. The authors found that record clearance benefits ex-offenders through external effects, such as the reduction of barriers to employment, and internal processes, such as the facilitation of cognitive transformation and the affirmation of a new identity. These benefits accrue from both the outcomes of the record clearance process and from the process itself. Further, Selbin, McCrary, and Epstein (2016) found that record clearing (or reduction) boosted employment rates and average real earnings. In the most recent study in this area, Prescott and Starr (2019) found that offenders received an approximate 25% increase in wages (compared to offenders without expungement) within two years of expungement. The authors also found that re-offense rates for ex-offenders granted expungement were comparable offense rates of the general population. However, the authors noted that only 6.5% of Michigan ex-offenders sought expungement within five years of eligibility.

Ban the box laws, which prohibit employers from asking about criminal history on applications and dictate when background checks can be completed, have become popular policy in recent years in both the public and private employment sectors (Henry & Jacobs 2007; Rodriguez & Avery 2017). Ban the box laws applying to public and or private companies have been implemented in 31 states and approximately 150 more localized jurisdictions (see Avery & Hernandez, 2018). For example, Ohio passed a statewide law in 2015 that banned criminal record questions on applications in the public sector and over 10 local jurisdictions have similar policies (Avery & Hernandez, 2018). Interestingly, empirical tests have generally shown that such laws do aid those with criminal records in securing better employment outcomes (Agan, 2017b; Atkinson & Lockwood 2014). One study sent out 15,000 fictitious resumes in two states that recently passed ban the box legislation (New York and New Jersey) and found that such laws made it more likely that individuals with criminal records would receive call-backs from prospective employers (Agan & Starr, 2017b). Further, one analysis examining the impact of the Massachusetts Criminal Offender Record Information reforms (enacting BTB policies) found small reductions in recidivism (Jackson & Zhao, 2017).

Other research, however, found that minorities with no criminal record are negatively impacted by BTB laws likely due to statistical discrimination (Agan & Starr 2017b; see also Sugie, 2017). Aigner and Cain's (1977) statistical discrimination theory states that employers will make assumptions about potential employees based on their race. Pager and Karafin (2009) extended statistical discrimination to also explain poor ex-offender outcomes (see Ortiz, 2014 for a full discussion on statistical discrimination, employment, and criminal records). Specifically, it is argued that employers will assume that ex-offenders and minorities, on average, will not be as productive or employable as whites or non-offenders (Moss & Tilly, 2001; Pager & Karafin, 2009; Zamudio & Lichter, 2008). This results in employers not hiring an individual minority or ex-offender because said employers impute their assumptions about ex-offenders or minorities as a whole to specific individuals applying for a position. For example, Eberhardt et al (2004) found that respondents associated African Americans with the term "criminal." Numerous studies have linked negative views of African Americans and offenders to employers' unwillingness to hire such individuals (Anderson, 2012; Neckerman & Kirschenman, 1991; Pager & Karafin, 2009). Further, others have found that BTB affected companies raise other requirements such as experience levels (Shoag & Veuger, 2016), an attribute which ex-offenders (especially formerly incarcerated ones) generally lack (Austin & Irwin, 1990). Finally, one study found that 33% of surveyed employers ignored BTB requirements and included criminal record questions on applications (Day, 2019).

The Work Opportunity Tax Credit (WOTC), enacted by the Small Business Job Protection Act of 1996, was created to help disadvantaged groups secure employment

(U.S. Dept. of Labor, 2017). This mechanism does so by offering employers a tax credit for hiring members of these disadvantaged groups. One of the disadvantaged groups are ex–felons who have a conviction or release from prison that is no more than one year old. The amount of the tax credit is determined by the amount of hours worked by the disadvantaged employee during the first year of employment; however, the maximum credit is $2,400 per employee. Further, to receive the credit, either the employee must be pre-certified before applying to the job (uncommon), or the employer must certify the employee post-employment (most common) (Hamersma, 2003).

Another mechanism called the Federal Bonding Program, created in 1996 by the Department of Labor, sought to ease employer concerns that ex-offender job applicants would be untrustworthy workers (Ohio Department of Correction, 2017). The program is essentially an insurance policy (fidelity bond) that protects an employer from losses associated with employee dishonesty. The bond is given to the employer, free-of-charge, and serves as an incentive to the employer to hire the job applicant who has a "risk" factor in his or her personal background such as an ex-offender with a felony record. Recent survey research of South Carolina employers suggests that employers may be more willing to hire an ex-offender who is eligible for a WOTC or bonding (Martin et al., 2019).

While it is a federal program, individual states manage this mechanism. For example, the Ohio Department of Rehabilitation and Correction has managed the Federal Bonding Program since 1998 (Ohio Department of Correction, 2017). For the bond to be processed and issued, four factors must be satisfied: (1) the ex-offender's criminal history must be verifiable; (2) the ex-offender cannot be self-employed or on a personal service contract; (3) the employment must be full-time or part-time; and (4) the applicant must receive a job offer and the employer must schedule a start or hire date. Once issued, the bond is effective for six months with a coverage amount of $5,000. After the six months, continued coverage will be made available if the worker has exhibited job honesty under the program's bond (Ohio Department of Correction, 2017). One study has found that employer willingness to hire ex-offenders was 51% for those with bonding, compared to 12% for those who lacked this incentive (Albright & Denq, 1996).

One of the newest mechanisms for relieving collateral consequences, meant to avoid the shortcomings of pardons and expungement, is the certificate of recovery/relief.[5] Certificates of recovery/relief are meant to demonstrate former offenders have been rehabilitated, remove automatic licensing bars for those with criminal records, protect employers who hire ex-offenders from negligent hiring claims, and help decision-makers make better-informed decisions about hiring individuals with criminal records (Green, 2014; Love & Frazier 2006; McCann, Kowalski, Hemmens & Stohr, 2018). The earliest certificate of relief was created in New York (Radice, 2012). Several other jurisdictions have since created varying versions of certificates (see Garretson, 2016; McCann et al., 2018 for reviews).[6]

The theoretical underpinning for such certificates can most directly be traced to labeling and other related theories. Labeling theorists argue that early experimentation in criminal activity (primary deviance) could be exacerbated in individuals who were given

[5] This section largely follows Leasure and Andersen (2019).
[6] For other examples of certificates of relief, see Ariz. Rev. Stat. Ann. §§ 13-904 to -908 (2016); Cal. Bus. & Prof. Code § 480(b) (West 2016); 730 Ill. Comp. Stat. Ann. 5 / 5-5.5-25 (West 2016); N.J. Stat. Ann. § 2A:168A-7 (West 2011); and N.Y. Correct. Law §§ 700-706 (McKinney 2016).

a criminal label because they would begin to identify and act out in accordance with this label (secondary deviance or the self-fulfilling prophecy) (see Lemert 1951). The main purpose of a certificate of relief is therefore to help an individual shed this label. Such reasoning is also consistent with Braithwaite's (1989) reintegrative shaming where reintegration involves the use of some formal or social mechanism to show the deviant that they are still a member of society (see also Maruna, 2001; Maruna & Immarigeon, 2013 for the similar concept of "delabeling").

While certificates are a relatively new collateral consequence relief mechanism, some research has examined issues regarding their accessibility, awareness, and effectiveness. Early research on New York certificates of relief utilized interviews with court actors, offenders, and other related parties and examined whether the mechanism was accessible to ex-offenders, relevant for employment purposes, or uniformly awarded (Ewald 2016; Garretson 2016). Ewald (2016) interviewed judges and probation officers to explore how these actors understood and awarded certificates. Ewald (2016) found considerable differences in responses on the purpose or utility of certificates as well as the procedure for awarding certificates (i.e., granting the certificate during sentencing or post-supervision). The varying opinions and practices regarding New York's Certificate were said to result from informal local agreements and individualized discretionary judgments resulting from the statute's ambiguity.

In another study examining New York's certificate, Garretson (2016) interviewed judges, people with certificates or those eligible but without one, attorneys, current and former probation officials, service providers, and advocates. The author's goal was to examine the certificate's accessibility and perceived utility. Garretson found that the process of educating offenders about the existence of certificates varied widely between all these groups and individuals within these groups. She also found that interviewees had differing opinions on whether certificates were even relevant for employment purposes. One respondent stated "[c]ertificates are irrelevant for a lot of employers." (Garretson, 2016: 34). Another respondent stated "[e]mployers don't even know that certificates exist" (Garretson, 2016: 34). Therefore, the results of this research show that there is a wide range of perceptions about the awareness of employers about certificates and on a certificate's perceived effectiveness.

Here, the focus is on Ohio's certificate of relief, the certificate of qualification for employment (CQE) which became effective in 2012 (Ohio Rev. Code Ann. § 2953.25). Ohio's CQE was primarily created to remove occupational licensing barriers for those possessing criminal history. However, a CQE can also be used for "general employment opportunities" (Ohio Department of Rehabilitation and Correction, 2017). General employment opportunities refer to any position that does not require an occupational license. Interestingly, most CQEs are used for general employment purposes (Ohio Department of Rehabilitation and Correction, 2016).

A CQE is meant to increase the employability of an ex-offender by providing employers who hire CQE holders with negligent hiring immunity and an assurance that the individual has cleared a rigorous background check process. The background check process requires an applicant to provide a document listing identifying information (name, address, date of birth, social security number), prior convictions, prior employment, professional and personal references, reasons why the CQE is needed and should be granted, and one or more employment collateral consequences (Ohio Rev.

Code Ann. § 2953.25). Staff at the Ohio Department of Rehabilitation and Correction, prosecutors, and judges all review and provide input on applications (Ohio Rev. Code Ann. § 2953.25). However, the ultimate decision belongs to judges on whether or not to grant a CQE (Ohio Rev. Code Ann. § 2953.25).

As of January 2017, 588 CQE applications were granted, 140 were denied, and 1,559 were in progress (Certificate of Qualification for Employment Annual Review, 2017). Important here, in September 2017, Ohio amended their certificate statute to include a rebuttable presumption in favor of applying offenders. The amendment states that "[t]he certificate constitutes a rebuttable presumption that the person's criminal convictions are insufficient evidence that the person is unfit for the license, employment opportunity, or certification in question" (Ohio Rev. Code Ann. § 2953.25(D)(2)). Such language certainly would seem to strengthen the ability of the certificate to combat employment-related collateral consequences.

Ohio's certificate has threshold elements that must be met before it is officially granted to the ex-offender. The specific code section states as follows:

> [A] court that receives an individual's petition for a certificate of qualification for employment … may issue a certificate of qualification for employment, at the court's discretion, if the court finds that the individual has established all of the following by a preponderance of the evidence: (a) Granting the petition will materially assist the individual in obtaining employment or occupational licensing. (b) The individual has a substantial need for the relief requested in order to live a law-abiding life. (c) Granting the petition would not pose an unreasonable risk to the safety of the public or any individual (Ohio Rev. Code Ann. § 2953.25(C)(3)).

Further, to receive the certificate, ex-offenders must complete all sanctions and then wait one year for felony convictions and six months for misdemeanor convictions (Ohio Rev. Code Ann. § 2953.25(B)(4)(a)-(b)). As to the negligent hiring immunity, it is important to note that Ohio requires that employers know the employee possesses a certificate before they can claim immunity (Ohio Rev. Code Ann. § 2953.25(G)(2)). Further, if the employee demonstrates dangerousness or is subsequently convicted of a felony and is not terminated, the immunity does not apply (Ohio Rev. Code Ann. § 2953.25(G)(3)). To aid employers in determining whether a CQE is valid, the Ohio Department of Rehabilitation and Correction manages and publishes a list of all current CQE holders on their website. The list is updated every 2-6 weeks.

Two studies have specifically explored the effectiveness of Ohio's certificate. One study, conducted in 2015, surveyed Ohio certificate holders about whether the certificate was effective in improving their employment opportunities (Sahl, 2016). The survey of certificate holders received a response rate of 22% (about 90 individuals). Results showed an overall employment rate of 47%. Further, 48% of respondents noted that a certificate made no difference in their employment search. However, 25% noted that they did not even present the certificate to the employer. Forty-two percent said that a certificate did indeed help them secure or keep a job. These results indicate that ex-offenders have mixed perceptions regarding the effectiveness of certificates of relief as a collateral consequence relief mechanism.

Another study assessed the effectiveness of Ohio's certificate of relief with an experimental correspondence study. Leasure and Andersen (2019) utilized an experimental correspondence approach and sent fictitious resumes with identical educational backgrounds, employment experiences, and key skills to entry-level employers in Columbus, Ohio. Resumes differed only with a racially distinct name (Matthew O'Brien for White and Tyrone Williams for African American) and with an affirmative statement self-disclosing a criminal record. On one resume, no criminal record was discussed. On the second resume, the applicant noted a single one-year-old felony drug conviction. On the third resume, the applicant noted a single one-year-old felony drug conviction and a certificate of relief.

For White applicants, results showed the following predicted probabilities of a callback: no record, 29%; one-year-old felony, 9.8%; one-year-old felony plus CQE, 25.6%. The difference between the CQE group and the recent felony group was statistically significant. The difference between the CQE group and the no record group was not statistically significant. These findings suggested that certificates of relief offer a substantial benefit to White individuals with criminal records seeking employment. The predicted probabilities of a callback for African American applicants were as follows: no record, 25.2%; one-year-old felony, 8.4%; one-year-old felony plus CQE, 11%. The difference between the CQE group and the recent felony group was not statistically significant. However, when compared to African Americans with a recent felony and no CQE, African American certificate holders had over a seven-percentage point increase in callbacks when applying for office positions (those with more customer contact and less labor) versus labor positions (those with less customer contact and more labor).[7] This was a substantively significant finding. The difference between the CQE group and the no record group was statistically significant. Finally, the difference between Whites and African Americans who possessed a CQE was statistically and substantively significant.

[7] Office and labor positions were the only two job categories used in Leasure and Andersen (2019).

These results indicated that certificates of relief may be more effective for White applicants.

There were several noteworthy limitations from Leasure and Andersen (2016, 2019). For example, the study was not conducted in collaboration with the Ohio Department of Rehabilitation and Correction, meaning the hypothetical applicant names were not on Ohio's official CQE list. The authors argued that because the CQE list was updated periodically, it would not be uncommon for an applicant to possess a CQE and be absent from the published list. However, a stronger approach would be to add hypothetical applicants to the official CQE list. Further, the hypothetical applicants did not possess the actual PDF certificate that is awarded upon completion of the CQE application process. Instead, Leasure and Andersen (2016, 2019) included a simple statement on the resume noting that the certificate was recently awarded. Here again, a stronger approach would have been to attach the official PDF certificate to the resume.

2.5 THEORETICAL FRAMEWORK

There are several theories which are useful to understand why certain mechanisms are more effective at increasing employment outcomes for ex-offenders. Two of those theories, prospect and signaling theory, are intended to provide a framework to understand how certificates of relief, the focus of the current study, could be used to reduce employment barriers. In prospect theory, an ex-offender would use a certificate to reduce the risk associated with their hiring. In signaling theory, an ex-offender would use a certificate to signal the unobservable trait of productivity to an employer. The third theory, attribution theory, is intended to provide a framework to understand how explanatory statements accompanying a criminal record can reduce employment barriers.[8] In attribution theory, an ex-offender would provide an explanatory statement meant to lower the culpability for their actions and thus increase their employability.

A. Prospect Theory

Prospect theory is a theory about how individuals make decisions and was introduced by Kahneman and Tversky (1979). Prospect theory was developed to deal with unsupported components of utility/rational choice theory such as an assumption that individuals make decisions that that are in their best interest after a rational weighing of costs and benefits (Kahneman & Tversky, 1979). For example, if given the option of a 50% chance of winning $1,000 over a 100% chance of winning $400, utility/rational choice theory would argue that individuals should choose the second sure option. However, research shows that this is often not the case and prospect theory is an attempt to remedy this issue (Kahneman & Tversky, 1979). While this theory is descriptive and meant only to identify preferences (not to explain them), several components are useful in guiding the current study.

Prospect theory has two primary phases with multiple components in each for the actual decision-making process. The *editing* or *framing* phase involves analysis and refinement or simplification of the available prospects. A large portion of this phase is influenced by heuristics. Heuristics are essentially methods/mechanisms that one uses to answer a particular question (Tversky & Kahneman, 1986). For example, in the current context, an employer may ask “What is the probability that applicant A will be more

[8] It is important to note that the current study does not test attribution theory. Attribution theory is only meant to provide theoretical support for the inclusion of the explanatory statement.

productive than employee B?" Kahneman and Tversky (1973) would argue that individuals would typically rely on a representative heuristic.

Representativeness is when one tries to determine if object A is a representation of object B (Tversky & Kahneman, 1974). For example, an employer may see that an applicant possesses a recent criminal record and assume that this person is unstable or risky because of the behavioral characteristics commonly associated to those who have criminal history. In this example, the employer determined that the applicant was representative of a negative population (unstable or risky individuals) because the applicant possessed a criminal record. This is very similar to the procedures in statistical discrimination theory noted above in the discussion of ban-the-box (see Aigner & Cain, 1977). The strength of this heuristic largely depends on the level of representation (the quality of the match between object A and B) (Tversky & Kahneman, 1974).

Another common heuristic is called availability (Tversky & Kahneman, 1974; Tversky & Kahneman, 1973). Availability means that larger populations are more often used as a frame of reference than smaller populations, that likely events are more often used than unlikely events, and that one associates often co-occurring events (Tversky & Kahneman, 1974). Interestingly, both of the above heuristics are subject to multiple cognitive biases as people often begin the decision-making process with incomplete or inaccurate information and fail to properly adjust for new information (anchoring) (Tversky & Kahneman, 1974). For example, many individuals will make decisions based on limited personal or media observations and fail to recognize that such events could be rare and not typical (Tversky & Kahneman, 1974). In the current context, an employer may have had a single negative experience with a previous employee possessing a criminal record. This lone experience, which may or may not have been typical behavior from an ex-offender, could now influence their future practices about all other candidates with criminal records and result in a strong preference not to hire ex-offenders.

There are also several formal components of the editing or framing phase that are useful in the current study (see Kahneman & Tversky, 1979 for a full discussion of other components). One of these relevant components is called *coding,* which involves an individual identifying an outcome as either a loss or a gain. Kahneman and Tversky (1979) argued that an individual's perception of loss or gain could be affected by the formulation of particular contexts, particular prospects, the expectations of a particular decision-maker, and by the reference point used by the decision-maker. For example, an employer may have a strong need for an employee (particular context) and a preference not to hire candidates with criminal records (expectation of decision-maker). However, if all candidates possess criminal history (formulation of particular prospects), the employer may be likely to relax their criminal history preference and select a candidate according to other factors (particular reference point).

Another relevant component was meant to deal with situations of two or more prospects and was called *cancellation*. Cancellation involves the discarding of the similar traits of prospects in the decision-making process. For example, an employer may have a requirement of a high school diploma for employment. Because this factor is required and will be present in all candidates, the employer is likely to discard this factor in the decision-making process.

Finally, *detection of dominance* is when an individual rejects a prospect due to a dominating alternative without further evaluation. For example, consider the following two choices: (1) a 10% chance to win $20 and (2) a 10% chance to win $30. In this scenario, the dominated prospect (the lower dollar amount) is unanimously rejected (see Tversky & Kahneman, 1986). In the current study, the dominated prospect is very likely the applicant with a criminal record. This is evidenced by the plethora of research noting significantly lower hiring outcomes for those with criminal history compared to those without (see Ahmed and Lang, 2017 for a review). The key to improving hiring outcomes for ex-offenders, therefore, is to identify a mechanism that can remove the label of dominated prospect. Interestingly, some research on prospect theory shows that framing a choice in positive terms can cause an individual to select a dominated alternative (Schelling, 1981; Tversky & Kahneman, 1986).

In the second phase called *evaluation*, the prospect with the perceived highest value is selected (Kahneman & Tversky, 1979; Tversky & Kahneman, 1986). Kahneman and Tversky (1979) argue that decision-makers in this phase are prone to loss aversion, meaning that individuals fear equal losses more than gains. Kahneman and Tversky (1979, p.278) provide an example of this process:

> the difference in value between a gain of 100 and a gain of 200 appears to be greater than the difference between a gain of 1,100 and a gain of 1,200. Similarly, the difference between a loss of 100 and a loss of 200 appears greater than the difference between a loss of 1,100 and a loss of 1,200.

Several studies have supported this portion of the theory (Fishburn & Kochenberger 1979; Kahneman & Tversky 1979; Hershey & Schoemaker1980; Payne, Laughhunn, & Crum 1980; Tversky 1977; Eraker & Sox 1981; Tversky & Kahneman 1981; Fischhoff 1983). In the current context, this means that if an employer were given the option to choose between an applicant without a criminal record and another applicant without such a record, the employer would be more likely to hire the applicant without a criminal record, even with similar work histories and education. Again, the key to improve hiring outcomes for ex-offenders would be to find a mechanism that reduces their perceived riskiness.

B. Signaling Theory

Signaling theory largely came from labor economics (DeWitt, 2018; Spence, 1973). As noted by Maruna (2012, p.73), "signaling theory is very well suited as a framework for understanding how ex-offenders navigate and fare in the hiring process." Signaling theory first originated from a work by Spence in 1973. In essence, signaling theory involves an employer (receiver) trying to identify traits which signal a productive or non-productive employee and a job seeker (signaler) who is attempting to signal productivity and hence employability to an employer (Spence, 1973). Bushway and Apel (2012, p.30) provide a scenario which shows the potential benefit of signaling in the reentry and employment context:

> The person reentering the community from prison may know that he or she has desisted from crime or that he or she is a good employee—but no one else does. Moreover, she now belongs to an observable group (ex-prisoners) that is known to have poor employment outcomes and high recidivism rates. Are there ways that the desister can signal to the employer her true identity?

Spence (1973) defined signals as traits that could be altered by the job seeker. While recognizing the potential impact of non-alterable traits (generally race or sex), the focus of the theory is on alterable traits as these are ones that the job seeker controls (and could therefore demonstrate higher levels of productivity). The employer uses these signals of productivity because productivity itself is not an observable trait. There are many factors that influence the ability of a signal to be effective. First, it must be seen as rare or at least uncommon. For example, some argue that education is a very good signal, as it takes a good deal of work to achieve a degree, and these qualities are expected to be translated into work productivity (Bushway & Apel, 2012). However, if all applicants to a particular position note a particular level of education, the employer will seek other signals to determine productivity (Spence, 1973). Further, if all applicants to a particular position noted a criminal record, then the employer would turn to other signals to make their hiring decision. This is a crucial point for signaling theory as it means that what counts as a signal of productivity or non-productivity can shift over time and in different market conditions (DeWitt, 2018; Spence, 1973).

Second, the signal must fit the unobservable quality (productivity or reliability in the employment context) (Connelly, Certo, Ireland, & Reutzel, 2011; DeWitt, 2018). Fit is the amount of correlation between the signal and the unobservable quality (Connelly et al., 2011). As stated by DeWitt (2018, p.24) in the criminological context, "a signal of productivity or a signal of desistance should also be correlated with attitudes or behaviors we believe to be related to productivity or desistance." Third, if a job seeker sends multiple signals, those signals must be consistent (Connelly et al., 2011). Consistency is the amount of agreement between signals from a single individual (Connelly et al., 2011). Research in economics examining the consistency of signals has found support for this proposition. For example, Chung and Kalnins (2001) found that hotels which send consistent marketing signals receive higher profits than those that do not. Interestingly, DeWitt (2018) argues that consistency creates a difficult situation for job seekers with criminal records because such candidates would be obligated to send multiple positive signals to overcome the single negative signal of the criminal record.

Fourth, an effective signal must be viewed as honest.[9] An honest signal is one that is in agreement with the quality that the signaler is trying to convey (Connelly et al., 2011). For example, if an employee who is trying to convey education and thus productivity makes several grammar errors on their application, the employer may not view the signal as on honest one. In the current context, if an employee with a criminal record states that they have been crime-free for only a few months, the employer may not view that signal as honest.

Another component of signaling theory is noise. Noise can occur through varying interpretation of signals and also through preconceived perceptions of signals or signal categories (DeWitt, 2018). DeWitt (2018, p.22) stated as follows in regards to noise for those with criminal records seeking employment:

> [P]opular media is flush with references to dangerous criminals, collectively contributing to a disparaging view of ex-offenders by the general public and, thereby, a noisy, uncertain signaling environment. Probabilistically speaking, the criminological world knows that these depictions are caricatures of reality and it might even be said that employers are (un)consciously aware of this as well. However, the proverbial "bell" cannot be un-rung, and the damage to the signaling environment (i.e., labor markets) is unlikely to be undone without greater effort than it took to damage it in the first place.

Interestingly, some authors have argued that some signals may be more effective at reducing the negative signal of a criminal record. For example, Bushway and Apel (2012, p.33) note that signals which could overcome criminal record stigma "must be voluntary, . . . must be attainable by a comparatively small proportion of the population of interest, and . . . must have opportunity costs for the individual that vary inversely with desistance probabilities or work productivity" (see also Maruna, 2012).

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[9] Another component of signaling theory is reliability or credibility. This is essentially the combination of fit and honesty (see Connelly et al., 2011).

C. Reducing Risk or Signaling Productivity with Certificates of Relief

Bushway and Apel (2012) stated that certificates of relief may be an effective mechanism for signaling the productivity/employability of an ex-offender (see also DeWitt, 2018; Maruna, 2012). Further, prospect theory would also predict that certificates of relief should be an effective mechanism at reducing the risk of hiring one with a criminal record. First, the wait times required by CQEs demonstrate that an individual refrained from criminal behavior for a substantial period of time. Ex-offenders satisfying this component demonstrate a reduced risk of further offense (see Blumstein & Nakamura, 2009; Kurlychek et al., 2006, 2007) and thus a higher level of productivity/employability. Second, all individuals seeking a certificate must voluntarily undergo a rigorous background check process (CQE Information Flyer, 2016). This satisfies the voluntary component of signaling theory noted by Bushway and Apel (2012). Third, the certificate application process requires offenders to show that the mechanism is needed and deserved (meaning there is no danger to the community) by a standard of proof used in civil law (preponderance of the evidence). This court determination or stamp of good character should be an effective component which reduces the risk associated with hiring one with a criminal record. Further, satisfying that burden of proof could be viewed as difficult to achieve as required by signaling theory, especially because some courts have denied CQE applications submitted by otherwise eligible applicants (see In re Bailey 28 N.E.3d 578, 2015). Finally, the negligent hiring immunity was specifically created as a risk reduction component for employers. Given many employers point to liability fears for not hiring ex-offenders (see Levashina & Campion, 2009), this component may be a particularly effective risk reduction mechanism for employers. However, it is important to note that these requirements would need to be known or communicated to the employer in order for a CQE to be an effective signal or risk reduction mechanism.

As noted above, Leasure and Andersen (2016, 2019) and Sahl (2016) found that Ohio's certificate of relief improved hiring outcomes for ex-offenders (not statistically significant for African American applicants in Leasure and Andersen). Such results lend initial support for the argument that certificates are an effective signal of productivity (signaling theory) or an effective risk reduction factor (prospect theory).

D. Attribution Theory

The study design used here relied on the submission of resumes noting various criminal record conditions. The criminal record conditions were supplemented with a brief ex-offender explanation for the underlying criminal behavior. Such a practice is consistent with previous experimental studies examining employment and criminal record stigma as well as ex-offender practices (see Ahmed & Lang, 2017; Harding, 2003; Myrick, 2013; Ricciardelli & Mooney, 2019; Winnick & Bodkin, 2008). The substance of statement pointed to a drug dependency issue and subsequent job loss as the cause for the criminal behavior. These reasons were chosen because of the large prevalence of drug use and job instability with offenders (Cuyahoga Intake, 2014). The theoretical support for supplying an explanatory statement largely comes from attribution theory.

The primary argument of attribution theory was that a perceiver would attribute higher responsibility to an actor if the actions were viewed as the result of internal factors and that a perceiver would relieve the actor of responsibility if the act was viewed as the result of external or environmental factors (Heider 1944, 1958; Jones & Nisbett, 1971;

Jones et al., 1972; Kelley 1967; Kelley & Michela, 1980; Shaver, 1975). Examples of external factors are all factors beyond the individual's control such as socio-economic status and upbringing and examples of internal factors are personality, attitude, or greed (Cullen et al., 1985; Grasmick & McGill, 1994; Hawkins, 1981; Kelley, 1967).

The primary argument of the theory has received a good deal of support (Carroll, 1978; Carroll & Payne, 1977; Carroll, Perkowitz, Lurigio, & Weaver, 1987; Cullen, Clark, Cullen, & Mathers, 1985; Graham, Weiner, & Zucker, 1997; Grasmick & McGill, 1994; Hawkins, 1981; Ostrom, Ostrom, & Kleiman, 2004; Shaver, 1975). Research in the criminal justice area has found that those who attribute actions to internal factors give offenders a higher level of culpability and a harsher sentence recommendation (Grasmick and McGill 1994; Woolfolk et al., 2006), while those who attribute actions to external factors lower an offender's level of culpability and their respective punishment (Carroll et al., 1987; Cochran, Boots, & Chamlin, 2006; Cullen et al., 1985; Graham, Weiner, & Zucker, 1997; Hawkins, 1981; Unnever, Cullen, & Jones, 2008). For example, Carroll (1978a, 1978b) and Carroll and Payne (1977) found that the most lenient parole decisions were reserved for individuals who were viewed as committing crimes due to external, rather than internal factors. Further, Cochran and colleagues (2003) found that respondents using internal factors were significantly more likely to recommend a death sentence than were those using external factors (see also Boots & Cochran, 2011).

Interestingly, a few projects have indirectly begun to examine the relationship between attributions, criminal history, and employment. Ali and colleagues (2017) conducted three studies to examine the impact of apology, justification, and excuse on one's likelihood to hire one with a criminal record.[10] In the first study which used vignettes on a student sample, the authors found that providing an employer an apology or a justification in relation to a criminal conviction resulted in a higher likelihood of hiring. However, providing an excuse resulted in a lower likelihood of hiring an individual with a criminal record. These results were consistent across job and crime type (Ali et al., 2017).

The second study used a sample of hiring managers and confirmed the findings from the first study. In the third phase, the authors collected qualitative data from hiring managers. This data, supporting the other two studies, showed that the provided justification was an acceptable mechanism to decrease culpability, that the excuse showed lack of responsibility and thus questionable character, and that the apology showed acceptance of responsibility and thus employable character.[11]

[10] In the control condition, candidates gave the following response: "I would be happy to discuss in the interview." In the excuse condition, candidates gave the response: "I was convicted of [aggravated assault/burglary]. I was not responsible for the incident because I was in the wrong place at the wrong time. It was not my fault." In the justification condition, candidates gave the response: "I was convicted of [aggravated assault/burglary]. I accept responsibility because I should not have been involved but I got involved in the incident because I was trying to help out a family member." And, in the apology condition, the candidates gave the response: "I was convicted of [aggravated assault/burglary]. I should not have been involved and I understand what I did caused harm. I apologized and promised it would never happen again."

[11] Another study in this area showed that attributions could differ depending upon the geographic location of the employer. Cohen and Nisbett (1997) sent employers in different geographic areas varying letters seeking an opinion on employability. A treatment condition contained a statement explaining the circumstances surrounding a manslaughter conviction (killing a spouse's lover). A control condition conveyed a theft conviction. The results indicated that the treatment letters sent to western and southern areas received more favorable responses (statistically significant) than those sent to northern areas.

In the current context, this means that employers who identify internal factors as responsible for criminal acts will likely attribute a higher level of culpability (and thus unemployability) to the actor. Conversely, if the criminal act was viewed as a result of an external factor such as poor education or socio-economic status, then the employer would lower the applicant's level of culpability and determine that they are capable of rehabilitation and thus employable.

Drug dependency has been largely classified as a medical condition in recent literature (Leshner, 1997), which may mean this condition could be viewed as an external factor. The loss of employment could also be viewed as an external factor (without evidence of a with cause termination). Further, the applicant's simple forthrightness in voluntarily disclosing their criminal history may also be seen as a positive (EmployeeScreenIQ, 2013). Therefore, voluntarily including these two factors in an explanatory statement should increase the attractiveness of an ex-offender candidate. However, even if the two factors are not viewed as external, it is important to note again that supplying an explanatory statement with these conditions is both a more practical and a more generalizable approach for ex-offender jobseekers.

2.6 THE PRESENT STUDY

Because of the mixed findings on certificates of relief, the dearth of research on the subject, and previous findings that show the importance of employment to desistance (Bahr et al. 2010; Bushway & Apel, 2012; Garland et al. 2010; O'Brien 2011; Ray et al. 2016; Uggen 2000; Western et al. 2015), further study was necessary to test the utility of certificates of relief in combatting barriers to employment. The purpose of this study was to provide a more comprehensive test of certificates of relief. This objective was achieved with two field experiments. Specifically, this study built upon previous research in several ways. First, previous research in this area largely focused on perceptions of effectiveness and the process for securing a certificate of relief. Like Leasure and Andersen (2016, 2019), this study provides an actual test of the statute's effectiveness. Second, this study built upon Leasure and Andersen (2016, 2019) by testing the amended version of Ohio's certificate of relief (containing the presumption that one's criminal record is insufficient evidence to deny them an employment opportunity) which is theoretically more beneficial to ex-offenders (see Ohio Rev. Code Ann. § 2953.25(D)(2)).

Third, previous research only examined the effectiveness of certificates of relief for those possessing a drug conviction. This study used a criminal record condition that contained convictions of varying crime types (drug and theft). Relatedly, previous research only examined the impact of a single conviction. This study used a criminal history that included previous convictions. Including multiple and previous convictions was important, as previous research showed that most offenders have previous criminal convictions (Beck, 1993; Cuyahoga Intake, 2014) and that many of these offenders do not specialize in one particular crime-type (Cuyahoga Intake, 2014; Piquero et al., 2007).

Fourth, Leasure and Andersen (2016, 2019) included few variables in their robustness checks (only job type in the 2016 study). This study tested the robustness of its results primary results by including several control variables in later sensitivity analyses (see Uggen et al., 2014 for a similar approach).

Fifth, the hypothetical applicants in Leasure and Andersen (2016, 2019) did not have official certificates of relief and were absent from an online list of current Ohio certificate holders. This study was conducted in collaboration with the State of Ohio, and hypothetical applicants possessed official certificates. Further, hypothetical applicant names were also added to the online list of current certificate holders.

Finally, this study tested the geographic generalizability of certificates of relief by using Cleveland, Ohio for data collection. Ohio was chosen because of its recent implementation of the CQE legislation and because this state consistently has one the highest number of individuals under correctional supervision (Kaeble & Glaze, 2016). Cleveland was elected as this jurisdiction consistently produced more individuals coming under correctional supervision than any other in Ohio (Bennie, 2017). As certificates of relief have become a popular legislative collateral consequence relief instrument (Garretson, 2016), such research is crucial to justify the continued use of current versions of this mechanism.

The specific hypotheses were developed after careful consideration of previous research and theory in the employment area, new amendments to the certificate statute, and by the specific formulation of the base criminal record used in this study. Further, because of the design of the second experiment in the current study, it was possible to analyze the impact of a multiple conviction record on employment outcomes compared to one with no record. While the current study is primarily focused on the effectiveness of certificates of relief, this secondary analysis also contributes to the above literature given the dearth of research on the impact of multiple conviction records on employment outcomes. The hypotheses are as follows:

> H1: The probability of a callback for applicants with a CQE will be less than those with no record.

H2: The probability of a callback for applicants with a CQE will be greater than those with a record and no CQE.

H3: The probability of a callback for applicants with no record will be greater than those with a record.

H4: The probability of a callback for African American applicants will be less than White applicants in all criminal record conditions.

CHAPTER 3

METHODOLOGY

3.1 RESEARCH DESIGN

To answer the above research questions, two field experiments were used. The first field experiment used a between-subjects correspondence approach (see Lahey & Beasley, 2018; Vuolo, Uggen, & Lageson, 2018). This design called for sending an employer a single resume that is randomly assigned[12] a criminal record condition and a racially distinct name. There were several benefits to using a between-subjects design. One of the advantages was that an additional criminal record treatment condition could be used for further comparison. In the instant case, this meant that outcomes of a hypothetical applicant with a criminal record and CQE can be compared to one with no criminal record and one with an identical criminal record and no CQE. Sending two nearly identical resumes to a single employer would have likely raised suspicion and biased any results (see Vuolo et al., 2018). Further, using only one base resume guarantees equivalence of base resume information (education, work history, and skills) (Heckman, 1998; Heckman and Siegelman, 1993). Limitations of this design include the inability to examine within employer differences (Vuolo, Uggen, & Lageson, 2016) and a

[12] Randomization in the current study was accomplished using randomizer.org and random.org. Both platforms have been widely used for randomization in previous experimental studies (see Haahr, 2019; Urbaniak & Plous, 2013).

greater potential for error variance, where unobserved individual employer differences could affect the dependent variable.

To account for these limitations, a mixed (both within-subjects and between-subjects treatment variables used) correspondence design was also used (see Lahey & Beasley, 2018; Vuolo, Uggen, & Lageson, 2018). This portion of the design involved sending a single employer a treatment resume noting a criminal record and CQE and a control resume that noted no criminal history. Both resumes were matched on education, work history, and skill. Sending one employer both a treatment and control resume allowed one to examine within employer differences (Pager, 2003; Vuolo et al., 2016) and reduced the amount of error variance. Limitations of this approach include the lack of pre-treatment equivalence, carryover/spillover effects where one treatment's impact can affect a subsequent treatment's impact (a violation of the Stable Unit Treatment Value Assumption)[13], and the inability to send additional comparison treatments to a single employer (Berk, 2005; Lahey & Beasley, 2018; Phillips, 2016). Fortunately, one can control for the lack of pre-treatment equivalence in resumes of within-subjects designs with random assignment of base resume information (Bertrand & Mullainathan, 2004; Neumark, 2012).

[13] For example, an employer, who despises those with criminal history, may receive one treatment resume with a criminal record statement and one treatment resume without such a statement. Receiving the criminal record resume may result in concern about the candidate history of other candidates, causing the employer to cancel the job posting (see Phillips, 2016). This would negatively bias callback rates for other conditions. Carryover/spillover effects could also positively bias callback rates where the beneficial attribute of one resume is imputed to others. Interestingly, one study has provided evidence of carryover/spillover effects in correspondence studies (Phillips, 2016).

Alluded to above, the correspondence approach sends resumes or other mailings to employers. The racial and criminal record conditions are then communicated via a racially distinct name and by an indicative statement on the resume. The correspondence approach has several advantages over other designs. For example, contrary to the audit approach, which sends actual testers to interviews, the correspondence approach requires no actual job applicants, is cost-effective, and offers greater control over experimental treatment and control conditions (Lahey & Beasley, 2009; Pager, 2007). For example, in Uggen and colleagues (2014) audit study, the authors assessed their testers' conveyance of information and discovered some inconsistencies between tester reports and the quality check reports. Further, many employers now require online applications, and most U.S. adult job seekers utilize online resources when searching for employment (Nakamura et al., 2009; Smith, 2005; Stevenson, 2009). Other limitations of audit research such as problems in effective matching, the use of "overqualified" testers (testers who may seem more educated or well-spoken than the population under study), experimenter effects (individual traits, mannerisms, and or communications that would bias an experiment as they would vary from tester to tester), and issues of sample size and adequate statistical power (Fix & Struyk, 1993; Heckman & Siegelman, 1993; Vuolo et al., 2016).

The experimental correspondence approach is also superior to non-experimental designs for several reasons. First, random assignment, which is the hallmark of experimental design, creates groups that are probabilistically similar to each other with any differences being left to chance (Shadish, Cook, & Campbell, 2002). Because of this fact, we can infer that any differences in groups is due to the treatment, not differences between groups that were present before the implementation of treatment (Shadish et al., 2002). This means that properly implemented experiments can yield unbiased estimates of the average treatment effect (Shadish et al., 2002). Second, when properly implemented, this design best deals with internal validity by satisfying all three of the requirements for unpacking a causal relationship (the cause proceeds the effect, the cause is associated with the effect, and there is no plausible alternative explanation for the effect other than the cause) (Campbell & Stanley, 1963; Shadish et al., 2002). In fact, properly implemented randomized experiments are the only design to definitionally prevent selection bias (Berk & Ray, 1982).

Third, while one could survey employers or ex-offenders on their employment practices or experiences, such designs make it difficult to isolate causal mechanisms (Pager, 2003; Shadish et al, 2002), and it is also difficult to rule out selection bias and many other validity threats (Winship & Morgan, 1999; Rubin, 1990; Heckman et al., 1998). Further, Pager and Karafin (2009) have already shown that many employers in their survey design stated that they would consider minorities with a criminal record and the same employers were later found not to do so in an experimental audit study. Similarly, though qualitative research provides a wealth of information useful for framing various concepts and providing in depth understandings of individual experiences, it lacks in its ability to determine causal relationships (Shadish et al, 2002).

Finally, some studies have used secondary data such as the NLSY and techniques such as propensity score matching and fixed effects models to examine various questions surrounding criminal records and employment (see Ortiz, 2014 for examples). However, secondary data can be limited in terms of explanatory variables regarding employment outcomes and can suffer from low power (see Ortiz, 2014). Some have also shown that techniques such as propensity score matching can introduce bias by creating imbalance in groups (King & Nielsen, 2018). Further, and most important in the current context, no secondary data exists that could be used to adequately gauge the effectiveness of certificates of relief.

3.2 STUDY CONTEXT

The study was conducted in Cleveland, Ohio. In 2019, Cleveland had an estimated population of 383,793 (U.S. Census Bureau, 2018). Of that population, 52.1% were female. Further, 33.8% identified as White, 50.4% identified as Black or African American, and 11.2% identified as Hispanic or Latino (U.S. Census Bureau, 2018). According to Bureau of Labor Statistics data (2019a), Cleveland had an unemployment rate of 5.2% in January 2019, 4.8% in February 2019, 4.4% in March 2019, 3.8% in April 2019, 3.9% in May 2019, and 4.7% in June 2019. National averages during that time were 4% in January, 3.8% in February, 3.8% in March, 3.6% in April, 3.6% in May, and 3.7% in June (Bureau of Labor Statistics, 2019b).

One in six Ohioans—over 1.9 million people—has a felony or misdemeanor record (Ohio Poverty Law Center, 2017). Further, in 2015, more than 70,000 persons were incarcerated in Ohio jails and prisons, and 262,000 persons were under some form of community supervision (Kaeble & Glaze, 2016). These numbers result in a correctional supervision rate of nearly 3,000 per 100,000 adults (Kaeble & Glaze, 2016). In fact, only California, Florida, Pennsylvania, Georgia, and Texas had more individuals under some form of correctional supervision (Kaeble & Glaze, 2016). Further, 251,500 of these individuals were male and 80,000 were female (Kaeble & Glaze, 2016). Cuyahoga County, which comprises Cleveland area, consistently sends the most individuals to Ohio prisons. For example, in 2016, Cuyahoga County sent about 2,500 individuals to Ohio prisons, about 5 percentage points higher than any other county (Bennie, 2017).

Of these incarcerated individuals, 60.5% were White, 36.6% were black or African American, and about 2% were Hispanic (Bennie, 2017). Approximately 76% of Ohio inmates are 39 years old or younger (Bennie, 2017). Further, drug offenses (possession the most common) make up the largest single group of Ohio commitments at approximately 28%[14], while other common categories are crimes against persons (robbery most common) at 24%, property offenses (theft most common) at 22%, and fraud offenses (forgery most common) at 2%. However, some individual offenses worthy of note are weapons under disability which makes up 4% of Ohio commitments and resisting arrest at 2%. These statistics mirror data for Ohioans under some form of community supervision as well (Galli, 2016). Important in the current study, intake data also shows that at least 70% of Cuyahoga County offenders had one or more prior felony or misdemeanor convictions and with varied crime types (Cuyahoga Intake, 2014).

Cuyahoga county's correctional demographic trends track those at the state level (Cuyahoga County Snapshot, 2014). Further, approximately 3,250 offenders were released back into Cuyahoga County in 2014, with about 60% of those offenders on parole (Ohio Department of Rehabilitation and Correction, 2015). The most recent numbers show a three-year recidivism rate of 26.5% for the Cleveland area for offenders who entered the Department of Correction (Ohio Department of Rehabilitation and Correction, 2011). This rate is similar to other counties in Ohio.

[14] Crimes against a person would be the most common if sex offenses (rape most common) were counted in this category. However, Ohio separates these two crime types.

3.3 EMPLOYER SAMPLE

The data used in the current study was derived from a census of Cleveland, Ohio lower-level employment listings posted on the websites craigslist.org and careerbuilder.com and a random sample of indeed.com.[15] Only postings that required the submission of a resume were included.[16] [17]Employers that requested applicants to apply in person or those in industries which generally prohibit hiring those with criminal records were eliminated from the sampling frame (healthcare, childcare, eldercare, and security) (see Pager, 2003 for a similar approach). Lower-level employment was defined as a position requiring no previous work experience, training, or skills that were specific to a particular position. Further, postings were included if they contained minimal

[15] The number of postings and layout of careerbuilder.com and craigslist.org allowed for a census approach, while the amount of positions and layout of indeed.com was much greater and required a random draw. However, it was very difficult to create a full population list on indeed.com as the site repopulates postings after each page change or refreshing. Therefore, only postings listed within the last two weeks were included to try and approximate a census approach. Further, using more recent postings should help to increase the likelihood of callbacks. Focusing on more recent applications is a common practice in actual ex-offender job searches and in previous correspondence studies (see Decker et al., 2015).

[16] Submitting only a resume is a common practice. For example, a search of careerbuilder.com just before data collection showed that 159 out of 288 lower-level jobs only required submission of a resume.

[17] Some postings on indeed.com and careerbuilder.com required an applicant to fill in required fields when attaching their resume. These fields were largely completed using the information supplied on the resume (e.g., name, email, work experience in a particular industry). However, some fields (primarily on indeed.com) sought information that was not supplied on the resume. Examples included availability, expected start dates, and willingness to submit to a background check. To help ensure the absence of bias from this approach, each hypothetical applicant answered these questions in the same manner (see Agan & Starr, 2017a, 2017b for a similar approach to dealing with formal application differences). Further, models were estimated with and without job website as an additional control variable and there were no statistically or substantively significant differences in the treatment results. Job website was ultimately not included as a control variable in the robustness checks below because several individual job listings were posted on multiple websites.

requirements that were satisfied by the work histories in the fictitious resumes. This type of employment was the focus here given previous research showing that ex-offenders were more likely to seek and find work in these sectors (Griffith et al., 2019; Nally et al., 2014; Visher et al., 2008, 2011).

Sample size calculations for in-person audit and correspondence studies are largely dependent on the number of callbacks received for a particular treatment (Decker et al., 2015; Vuolo, Uggen, & Lageson, 2016, 2018). For matched-pairs designs utilizing binary outcomes, sample size is determined from the proportions of the discordant pairs (Vuolo, Uggen, & Lageson, 2016). For between-subjects designs utilizing binary outcomes, sample size is determined from the proportions of positive responses for each experimental condition or from expected effect sizes (Cohen, 1988; Rosner, 2011).

In line with the recommendations of Vuolo, Uggen, and Lageson (2016), several estimates of callback rates were used for sample size calculations. The estimates were guided by callback/willingness to hire estimates from previous research, new amendments to the certificate statute, and by the formulation of the criminal record and base resume information used in this study. Sample size for the mixed design was largely determined by Vuolo, Uggen, and Lageson (2016) as these authors presented sample size suggestions for various discordant pair proportions. Sample size for the between-subjects design was determined from various sample size calculators (ClinCalc, 2019; Champely et. al., 2018). After several different estimates of callbacks, a conservative mixed design sample size was determined to be 400 employers and thus 800 resumes (two resumes to each employer). A conservative sample size for the between-subjects design was determined to be 1200 employers and 1200 resumes (one resume to each employer). In each design, there will be 200 resumes for each condition (e.g., White with no record, African American with no record, etc.). These calculations were based on a power of .8 to .9 to detect statistically significant effects ($p \leq .05$).

To reach this sample size, data was collected from January 2019 to June 2019. Further, if an employer listed multiple postings, only one posting was selected for each employer. If the study were to include multiple postings from one employer, it would be possible that the employer would be randomly assigned resumes with identical base resume information and different names or criminal record treatment conditions. Each week, all lower-level job listings on the above websites were selected and then randomly assigned to a racial and criminal record condition.

3.4 BASE RESUME CONSTRUCTION

A correspondence approach necessitates the creation of base resume information such as education, work history, address, phone number, and skills. Traditionally, two approaches have been used to assign this base information to resumes. In the first approach, researchers create sets of resumes with base information that is equally matched on each component (see Decker et al., 2015). For example, one resume would note a 2-year employment with Home Depot, while another would note a 2-year employment with Lowe's. Each base resume component would be matched in this fashion. Previous authors using this method argue that random assignment of treatment to resumes cures any pre-application differences (Decker et al., 2015). However, others note that failure to create effective matches could bias any results due to unobserved resume characteristics (Heckman, 1998; see Lahey & Beasley, 2018 for the similar problem of template bias). In an attempt to cure this potential bias from unobserved resume characteristics, some randomly assign base resume characteristics and treatment conditions so that they can analyze the impact of these factors (see Agan & Starr, 2017a, 2017b; Bertrand & Mullainathan, 2004; Neumark, 2012). In the current study, base resumes were matched and coded so that their potential impact could be assessed and controlled.

Regardless of the chosen approach, it is most important to ensure that the base resume information is generalizable to the population under study (Carlsson, Fumarco & Rooth, 2014; Lahey & Beasley, 2018). To ensure generalizability here, resumes of ex-offenders who participated in an Ohio work-readiness program were used to create the bulk of the fictitious base resume information. This information was also coupled with Ohio correctional data.

In line with this approach, resumes were designated with only a GED to satisfy the education requirement of the resume. Because of the commonality of an applicant with a GED, both resumes can be assigned this characteristic without raising suspicion. This education level was chosen because of its prevalence at the Ohio work-readiness facility and because approximately 64% of those under supervision in Ohio had at least a GED or high school diploma (Intake, 2016). A GED was chosen over a high school diploma because the majority of offenders under Ohio supervision had a GED rather than diploma (Intake, 2016). Assigning resumes with a GED rather than no GED or no high school diploma should increase employer response and thus increase the statistical power of the study (see Pager, 2003 for a similar approach). However, because approximately 40% of offenders lack a GED or high school diploma, it will be important to note as a descriptive statistic how many employers in the sample make this level of education a requirement.

As to the work history requirement on the resume, research has found that 31% of state inmates and 27% of federal inmates were unemployed in the month before their incarceration (Government Accounting Office, 2000). In Cuyahoga County Ohio, 49.7% of correctional commitments reported some type of employment before incarceration (Cuyahoga Intake, 2014). The most recent work in this area suggests that post-incarceration employment is indeed very irregular for ex-offenders (Sugie, 2018). Therefore, in the current study, resumes were assigned an inconsistent work history to increase external validity. Specifically, resumes indicated lower-level employment experience in general labor, customer service, and restaurant work. The key skills were derived from this work history.

Each resume will include equivalent gaps in work history to account for a period of incarceration. However, many ex-offenders with felony convictions do not face incarceration (Reaves 2013). Regardless, the approach here was designed to fit any sanction.

The addresses presented on the resumes were randomly chosen from a list of rental properties in the Central and Glenville neighborhoods of Cleveland (two addresses for the mixed design and one address for the between-subjects design). Residents of these neighborhoods were predominately African American populations (approximately 42% larger of an African American population compared to all of Cleveland) (Statistical Atlas, 2019a, 2019b).[18] Further, when compared to Cleveland averages, these neighborhoods had lower household incomes, higher unemployment rates, higher use of public benefits such as food stamps, higher proportions of individuals with no high school diploma, and lower proportions of individuals with advanced degrees (Statistical Atlas, 2019a, 2019b). While it may be interesting to explore whether there would be differences between disadvantaged and advantaged addresses, the addresses used here have been shown to receive the largest amount of reentering ex-offenders (La Vigne et al., 2003) and were used here to increase generalizability.

The resumes also noted a Cleveland, Ohio phone number which used a standard voicemail account. Email addresses were also presented on resumes. The email addresses were comprised of the applicants' name (i.e., sethwalsh135@gmail.com).

3.5 KEY INDEPENDENT VARIABLES

A. Criminal Record Conditions

The first key independent variable contained the criminal record categories. In the between-subjects portion of the study, employers were randomly assigned one of three criminal record categories. The specific crimes were formulated from commitment and release data from the Ohio Department of Rehabilitation and Correction noted above. The criminal record categories for the between-subjects design were as follows: (1) a condition noting a felony drug and theft conviction and a misdemeanor drug conviction (2) a condition noting a felony drug and theft conviction and a misdemeanor drug conviction with CQE; and (3) a condition noting no criminal record. The no criminal

[18] This entity combines and cross-references information from the 2010 U.S. Census Bureau and from the 2012-2016 American Community Survey.

record group is useful as it provides a baseline measure for the applicants with certificates. The specific criminal record categories for the mixed design were as follows: (1) a condition noting a felony drug and theft conviction and a misdemeanor drug conviction with CQE and (2) a condition noting no criminal record. Using a criminal history with multiple convictions provides a more comprehensive test of Ohio's certificate because this type of criminal history is more generalizable to Ohio's offender population.

The misdemeanor drug conviction was listed as approximately six years-old and the felony theft and drug convictions were listed as approximately three years-old. The three-year-old convictions noted a term of incarceration that ended a little over one year before the submission of the resume. This post-incarceration term allows for the one-year time clean requirement imposed by the CQE statute. Further, as noted above, this study was conducted in collaboration with the State of Ohio, and hypothetical applicants possessed official certificates and hypothetical applicant names were also added to the online list of current certificate holders.

Criminal histories in previous audit and correspondence studies were signaled by noting work experience in prison (see Decker et al., 2015, i.e., Arizona State Prison Complex, Laundry Crew), parole officer references (Pager, 2003), application questions (Agan & Starr, 2017a; Pager, 2003; Uggen et al., 2014), and through unsolicited statements in interviews (Uggen et al., 2014). Many of these studies confirmed these practices as common with local employment specialists, parole officers, and offenders (Pager, 2003; Uggen et al., 2014). The practice of self-disclosure on resumes and other early application materials, such as cover letters, is consistent with procedures recommended by a Columbus, Ohio work-readiness facility (where the author assisted in teaching for three years), professional resume writing firms, and previous correspondence studies (Ahmed & Lang, 2017; Grammar Chic, 2014). The criminal record conditions used here were conveyed via cover letter.

Self-disclosure of a criminal record is recommended practice for several reasons. First, research shows that an ex-offender's forthrightness helps combat the negative effects of criminal record stigma (Ross et al, 2011; Ali et al., 2017). In fact, one study found that 52% of the 992 employers surveyed would be inclined to hire someone who self-disclosed criminal history, while only 8% stated that they would not hire one who self-disclosed (EmployeeScreenIQ, 2013). Relatedly, Adler (1993) found that many offenders fear attempts to conceal their criminal history will likely harm their employment opportunities. Second, research has demonstrated that disclosure of one's criminal history can serve as a form of stigma management, as the offender controls when and how the information is discussed (Harding, 2003; Myrick, 2013; Ricciardelli & Mooney, 2019; Winnick & Bodkin, 2008).

B. Racial Conditions

The second key independent variable was the randomly assigned racially distinct name. This variable was coded 0 for African Americans and 1 for Whites. First names were randomly selected from Gaddis (2017) who surveyed users of Amazon's Mechanical Turk (an online crowdsourcing platform) to examine perceptions of which first names best conveyed race in correspondence studies (see Berinsky et al., 2012; Buhrmester, Kwang, & Gosling, 2011; Chandler, Mueller, & Paolacci 2014 for a discussion of representativeness/external validity issues with this platform). Gaddis

(2017) also paired first names with various last names to ensure that the racial perceptions of the first names were robust once paired with various last names. First names that best conveyed a White individual were Katelyn, Hunter, Claire, Jake, and Seth. More than 97% of respondents in that study perceived these names as White. First names that best conveyed an African American individual were DaShawn, Tanisha, Tremayne, Jamal, and Daquan. More than 95% of respondents perceived these first names as African American. Many of these first names were also used in the rigorous correspondence study by Bertrand and Mullainathan (2004) who confirmed racial distinctiveness from birth certificate data and an independent field survey.

Last name selection was guided by Crabtree and Chykina (2018) and frequency data from the U.S. Census Bureau (U.S. Census Bureau, 2012). Crabtree and Chykina (2018) adapted a method from political research to identify geographically robust racially distinct last names and found that last names such as Andersen, Walsh, Nielsen, and McGrath were consistently perceived as White. Further, frequency data from the Census showed that many last names are predominately used by one race. For example, the surname Washington was used by 89.9% of African Americans and only 5.2% of Whites. The surname Jefferson was used by 75.2% of African Americans and only 18.7% of Whites. The surname Booker was used by 65.6% of African Americans and only 30% of Whites. Census data also showed that the White names mentioned above from Crabtree and Chykina (2018) were used by approximately 95% Whites and only about 1% of African Americans.

Using these sources of racially distinct names, a list was created so that individual first and last names could be randomly drawn and applied to resumes. The randomly selected names for the African American applicants were as follows: (1) DaQuan Jefferson and (2) Tremayne Washington. The randomly selected names for the White applicants were as follows: (1) Jake Walsh and (2) Seth McGrath.

3.6 DEPENDENT VARIABLE

The dependent variable in both portions of the study was whether a hypothetical applicant was invited to advance in the hiring process (dichotomous yes or no). Generally referred to as a callback, this has been the outcome of interest in the majority of experimental studies examining racial and criminal record discrimination within the hiring process (see Decker et al., 2015; Lahey & Beasley, 2018; Leasure & Andersen, 2016, 2019; Pager, 2003, Pager et al., 2009; Uggen et al., 2014). Discussed further below, the mixed design includes an additional robustness check analysis for occasions when both resumes receive a callback. This analysis uses a dependent variable that noted which resume was called first (see Rodríguez Menés & Rovira, 2019 for a similar approach).

Focusing upon this stage of employment is ideal as research has found and argued that these early employment outcomes, such as hiring and interview decisions, account for a large amount of discrimination in the job market (Bendick et al. 1999; Pager 2003; Pager et al. 2009). Callbacks were measured by monitoring anonymous email accounts (which were made up of the hypothetical applicants' names, e.g., jakewalsh.1@gmail.com) and anonymous voicemail boxes for 30 days after submission of the resume. While voicemails and emails were monitored for 30 days after resume submission, virtually all callbacks occurred within one week of submission (see Baert et al., 2016 for a similar practice and result).[19]

3.7 ROBUSTNESS CHECKS

A. Control Variables

The first robustness check procedure involved including control variables into each regression model. There were several control variables included in both studies. The first variable noted the employment status of the position (full-time, part-time, and none listed). This variable was coded 0 for part-time, 1 for full-time, and 2 if neither could be determined from the posting.[20] Some postings did not directly specify an employment

[19] Sixteen voicemails or emails could not be traced to a specific job posting/company name (see Agan & Starr, 2017a, 2017b experiencing similar difficulties). However, it is important to note that some of these unidentified voicemails or emails may have been related to entries that were already recorded or were recorded later. Further, it is also possible that two or more of the unidentified voicemails and emails were related to the same entry. Failure to identify a voicemail or email occurred for two reasons. First, some voice messages were inaudible or left little detail and the job posting could not be identified through a Google search of the phone number. This first reason accounted for the majority of voicemails and emails that could not be traced. Second, some emails and voicemails noted company names that did not match any of those in the submitted list. It is possible that some unidentified companies had additional titles/affiliations such as parent companies. Such an explanation is plausible as Google searches sometimes uncovered these affiliations. However, some unidentified company names could not be traced to a parent company or separate affiliation. Some of the unidentified voicemails and emails could be narrowed to specific designs and conditions. Six can be traced to the mixed design (meaning one was CQE and one was no criminal record) as both phones received calls. Of those six, two pairs were White and African American (DaQuan and Jake and Seth and Tremayne). Five other calls were for Jake (two which must have been a CQE), two for Tremayne, one for Seth, and two that did not note a name (one for delivery driver and one for landscaping).

[20] During initial data collection, this variable was coded as 1 for part-time positions and 0 for all other positions. In the analysis phase, this variable was recoded in several different ways. The first recode was used in the results below. This recode had the following categories: 0 for part-time, 1 for full-time, and 2 for postings did not provide enough information to clearly determine whether the employment status was full-time or part-time. The second recode dealt with occasions when an employer gave the employee the option of both full-time and part-time employment. These occasions were initially coded

status. In many cases, additional language in the posting could be used to determine employment status. For example, if the posting noted a work week of 40 hours, such a posting would be labeled full-time. If a posting noted a 10-hour work week, it would be labeled part-time. This variable was important to include as employers may be more likely to offer ex-offenders less desired part-time positions with lower hours and less benefits (see Nally et al., 2011; Pager et al., 2009).

The second variable noted whether the employment was temporary or seasonal. This variable was coded 1 if the position listing noted temporary or seasonal work and 0 otherwise. Here again, it is possible that ex-offenders again will be offered less desired positions (see Nally et al., 2011).

The third variable noted whether the position was offered by a staffing company. This variable was coded 1 if the listing was produced by a staffing company and 0 otherwise. Many job postings included language that directly identified the listing company as a staffing firm. For postings without identifying language, internet searches of company names were used to determine if the business was a staffing firm. This variable was important to include, as a staffing company may be more inclined to hire

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as full-time. In the analysis phase, an attempt was made to recode these postings as a separate category. However, both recodes were largely only successful for the indeed.com postings as posting links for the other sites either no longer functioned or were removed. Nonetheless, the reliability of the recoded variable used below is supported by the fact that very few postings on craigslist.org and careerbuilder.com failed to identify an employment status. In fact, craigslist.org and careerbuilder.com require that employers enter an employment status. Most importantly, no coding formulation of this variable significantly or substantively altered the results of the treatment variables. Results with and without the employment status variable were also compared and no statistically or substantively significant differences were found.

riskier individuals such as ex-offenders as such firms are contracted to provide a steady workforce into jobs that are traditionally difficult to keep adequately staffed.

The fourth variable noted the hourly pay offered by the position. This variable was important to include as some have suggested that ex-offenders will be forced to accept lower paying positions (Griffith et al., 2019; Nally et al., 2011; Nally et al., 2014; Visher et al., 2008, 2011). After all data was collected, the hourly pay for the sample was coded as 0 for pay at or below the 50th percentile, 1 for pay above the 50th percentile, and 2 if hourly pay was not provided in the listing.[21]

The fourth variable noted the job type for which the resume was submitted. This variable was important to include, as many authors have shown that employment outcomes can also depend upon the type of job being sought (Bendick et al., 1991; Bendick et al., 1994; Decker et al., 2015; Galgano, 2009; Holzer et al., 2004; Pager, 2007; Pager et al., 2009; Purser, 2012). The categories were as follows: clerical, customer service in store, customer service call center, restaurant labor, restaurant customer service, sales in store, sales call center, driving, warehouse/shipping, manufacturing, general labor, and multiple positions. These categories were largely derived from Leasure and Andersen (2016). However, customer service, sales, and restaurant have two subcategories here because Pager and colleagues (2009) argued that minority and ex-offender applicants would be steered toward two types of positions, (a) positions with greater physical demands and or (b) positions with less contact with customers (see also Holzer et al., 2004).

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[21] Additional codings of this variable were explored (additional percentiles). These additional codings did not statistically or substantively alter any results discussed below.

The fifth variable noted the distance between the employer address and the hypothetical applicant address. This variable was important to include as an employer may be more likely to consider hiring an individual that lives closer to the workplace (see Phillips, 2018). Employer addresses were recorded and distance from the applicant's address was calculated in miles using the Google Maps Directions feature. This variable was coded as 0 for distances that were at and below the 50th percentile and 1 for distances above the 50th percentile.[22]

The sixth variable noted the county subdivision where the employer was located. This variable was important to include as some argue that communities which are more often exposed to convicted individuals are less affected by the stigma of a criminal record (Hirschfield & Piquero, 2010). The variable was coded 1 for employers located in Cleveland City, East Cleveland, Cleveland Heights, and Shaker Heights, 0 for those that were not, and 2 if no subdivision could be identified. This coding procedure was chosen

[22] The distance variable is only included in the descriptive statistics below due to missing data (36.75% missing in the mixed design and 41.50% missing in the between-subjects design). Analyses that used distance dummies (0 = not missing and 1 = missing) as dependent variables and other control variables noted in this section as independent variables showed several statistically significant predictors. Given this result, the data were likely not missing completely at random (see Social Science Research Cooperative, 2013). Because there are several plausible reasons for missing data that are not fully explained with other variables (i.e., locations with difficult commutes, locations within disadvantaged neighborhoods, and positions that require travel to several locations may be less likely to list an address), there may also be a question as to whether the data is missing at random. Further, some guidance suggests that imputation of around 50% of a variable would likely result in bias (Social Science Research Cooperative, 2013). Therefore, most imputation methods would not be recommended. Nonetheless, control models with and without a distance variable (coded as 0 for distances that were at and below the 50th percentile, 1 for distances above the 50th percentile, and 2 for missing) were compared and there were no significant or substantive differences in the results for the treatment variables. Additional codings of this variable were also explored (additional percentiles). These additional codings did not statistically or substantively alter any results discussed below.

as previous research demonstrated that a vast majority of offenders returning to Cuyahoga County returned to the Cleveland City, East Cleveland, Cleveland Heights, and Shaker Heights county subdivisions (La Vigne, 2003). Cleveland City, East Cleveland, Cleveland Heights, and Shaker Heights also possess the higher percentages of minority residents in Cuyahoga County (U.S. Census Bureau, 2018). This is important as previous research has demonstrated that neighborhoods with higher concentrations of White residents are more likely to stigmatize in the hiring process (see Agan & Starr, 2017a; Agan & Starr, 2017b).

The seventh variable noted the time of day that the application was submitted. This variable was coded 0 for AM and 1 for PM. The eighth variable noted the day of the week that the application was submitted. This variable was coded 0 for Sunday through Thursday and 1 for Friday and Saturday. The ninth variable noted the month in which the application was submitted. This variable was coded 1 for January, 2 for February, 3 for March, 4 for April, and 5 for May. Each of these temporal variables were important to include as some have argued that applications submitted during certain months, times, and days have a higher likelihood of a positive response (see ZipJob, 2017a, 2017b). For example, Chakrabarti (2017) found that applications submitted in the morning were five times more likely to receive an interview. Further, Chakrabarti (2018) found a higher likelihood of an interview for applications submitted Sunday through Thursday.

The tenth variable noted the age of the position listing. This variable was coded 0 for listings posted within one to 4 days and 1 for listings posted within 5 to fourteen days. This variable was important to include as applicants applying to more recent listings are more likely to receive a favorable response due to lower numbers of competing applications. In fact, Dalton (2017) noted that applicants were up to eight times more likely to get an interview if they applied within four days of the position being listed.

There were also additional control variables that were used only in the mixed design. The first variable in this category noted which base resume accompanied the criminal record and racial treatment variables. While the base resumes were closely matched on education, work history, and skills, it is still possible that the minor differences between resumes could impact results if left unobserved (see Neumark, 2012). To account for this issue, base resume 1 was coded as 0 and base resume 2 was coded as 1 so that any impact of the base resumes could be assessed and controlled.

The second variable in this category noted the order in which the resumes were submitted. This variable was coded 1 if the resume was to be submitted first and 0 if the resume was to be submitted second. This variable was important to include as an employer may be more inclined to contact a candidate who was earliest to submit an application (see Uggen et al., 2014). However, the design of the mixed design portion of the study was meant to limit any impact of this variable as the second resume was submitted no more than one hour after the first.

B. Individual Names

The second robustness check procedure involved an examination of the impact of individual names. This procedure was important to include because the two individual names used as a measure for each race may differentially impact callback rates. Similar analyses have taken place in previous correspondence studies that examine racial discrimination (see Bertrand & Mullainathan, 2004).

C. Interactions

The third robustness check procedure included tests for an interaction effect between race and criminal record. This procedure was important to include as previous research discovered a robust interaction between race and criminal record (see Pager, 2003; but see Agan & Starr, 2017a who did not find a significant race and criminal record interaction). Additional interactions between city location, criminal record, and race are also explored. While not shown below, all interactions were also examined graphically.

D. Racial Grouping

The mixed design includes an exploration of the impact of each racial grouping. A racial grouping is defined as the specific pairing of resumes sent to each employer (two African American resumes, two White resumes, and one African American and one White resume). Such a procedure has been recommended and conducted by previous research (see Heckman & Siegalman, 1993; Pager et al., 2009).

E. First Call Analysis

As mentioned above, the mixed design includes an additional analysis for occasions when both resumes receive a callback. This analysis uses a dependent variable that notes which resume was called first (see Rodríguez Menés & Rovira, 2019 for a similar approach). This procedure was important to include because it is possible employers would be more likely to first call White candidates and those without criminal records. Further, such a procedure reduces measurement error for within-subjects audit studies that examine discrimination (Rodríguez Menés & Rovira, 2019).

3.8 ANALYTIC STRATEGY

All analyses will be conducted with the software Stata 16 (StataCorp., 2019). The first stage of data analysis presented descriptive statistics. Specifically, distributions of variables across the criminal record and racial treatment levels are presented with frequencies and percentages (see Saint-Mont, 2015 noting the need for such statistics as randomization is not a guarantee of comparability between variables of interest).

The second stage utilized bivariate analyses to examine the impact of the criminal record variable and the race variables separately. The specific bivariate tests used were logistic regressions, Chi square tests, and McNemar's tests (see Uggen et al., 2014 for a similar approach).[23] Using both parametric and non-parametric tests help ensure the robustness of results.

The third stage of analysis utilized multiple logistic regression to examine the callbacks for criminal record categories across race. This stage examined differences in callbacks for the criminal record levels within each racial group individually (i.e., examining whether African American certificate holders fare better than African Americans who do not possess a certificate) and across race (i.e., does one racial group fare better in employment outcomes). The final stage of analysis applied the robustness checks noted above (see Uggen et al, 2014 for a similar approach).

The mixed design portion of the analysis required an adjustment for clustering as multiple resumes were sent to one employer (see Lahey & Beasley, 2018). Stata's cluster analysis commands were used to address this issue. Such adjustments were not necessary with the between-subjects portion of the study (Lahey & Beasley, 2018).

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[23] Logistic regression diagnostics are presented in Appendix A.

Results are presented using Stata's margins commands.[24] Reported first in all regressions were predicted probabilities (predicted probability of a callback in the current study) (Williams, 2012). The prime advantages of using predicted probabilities include ease of interpretation (including interaction effects which are often misinterpreted in non-linear regression models) and practical significance (Ai & Norton, 2003; Kam & Franzese, 2007; Long & Freese, 2006; Norton et al., 2004; Williams, 2012).

Statistically significant differences between these predicted probabilities were then reported using Stata's average marginal effects (discrete change from base level) (see Bartus, 2005; Cameron & Trivedi, 2010; Kam & Franzese, 2007; Williams, 2012 for authors preferring average marginal effects largely because all data is used in its computation rather than just means).[25] While statistical significance ($p < .05$) was noted throughout the results section, marginally ($p < .10$) and substantively significant findings were also discussed as recommended by Bushway, Sweeten, and Wilson (2006), McCloskey and Ziliak (1996), and Wasserstein, Schirm, and Lazar (2019).

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[24] Regression output using odds ratios is presented in Appendix B. Model fit statistics (AIC and BIC) are also provided to compare models within the same section using different predictors. Favored BIC values were identified using Raftery (1995) and favored AIC values were identified using Hilbe (2009).

[25] All p-values presented in the tables and text below are derived from default Stata output ($p < .05$). For directional hypotheses, however, these values should be divided in half (if in the expected direction) to determine statistical significance. Further, only the key independent variables are examined for one-sided significance. Results are discussed and interpreted in line with this approach.

CHAPTER 4

RESULTS

4.1 MIXED DESIGN RESULTS

A. Descriptive Statistics

A total of 800 resumes were sent to 400 employers. As shown in Table 4.1, data was balanced on the criminal record and racial treatment variables so that each level had 200 resumes. Overall, 150 out of 800 (18.75%) of the hypothetical applicants received an invitation to continue in the hiring process.

Table 4.1. Distribution of treatment variables

	No Record	CQE	Total
Race			
White	200 (50.0%)	200 (50.0%)	400 (100%)
African American	200 (50.0%)	200 (50.0%)	400 (100%)

Displayed in Table 4.2 are robustness check variables which could vary across criminal record categories. As shown in Table 4.2, randomization of these variables was successful. The minimum distance for the no record level was .4 miles, the median was 15.5 miles, and the maximum was 38.1 miles. The minimum distance for the record and CQE level was .8 miles, the median was 15.7 miles, and the maximum was 36.6 miles.

Table 4.2. Distribution of control variables which could vary across criminal record types.

Variables	No Record	CQE	Total
Base Resume			
Base Resume 1	200 (50.0%)	200 (50.0%)	400 (100%)
Base Resume 2	200 (50.0%)	200 (50.0%)	400 (100%)
Distance			
Below Median	127 (50.2%)	126 (49.8%)	253 (100%)
Above Median	126 (49.8%%)	127 (50.2%%)	253 (100%)
No Address	147 (50.0%)	147 (50.0%)	294 (100%)
Submitted First			
Yes	197 (49.3%)	203 (50.8%)	400 (100%)
No	203 (50.8%)	197 (49.3%)	400 (100%)

The remaining variables did not vary by criminal record treatment as both levels were sent to a single employer. Four percent of the resumes were submitted to employers that were staffing firms. Seven percent of the resumes were submitted to temporary employment positions. The minimum hourly pay was $8, the median hourly pay was $11, and the maximum hourly pay was $18.58 (24% below the median, 23% above the median, and 53% with no hourly pay listed). The distribution for month submitted was as follows: January, 8.3%, February, 33.8%, March, 27.3%, April, 13.0%, and May, 17.8%. The distribution for day submitted was as follows: Monday, 3.5%, Tuesday, 38.8%, Wednesday, 12.8%, Thursday, 32.8%, Friday, 7.5%, Saturday, 2.8%, and Sunday, 2.50% (89.8% Sunday through Thursday and 10.3% Friday and Saturday). Approximately 61% or resumes were submitted in the AM and approximately 39% in the PM. Approximately 23% of resumes were submitted to companies located inside of the Cleveland, East Cleveland, Shaker Heights, and Cleveland Heights county subdivisions, while about 66% were submitted to other subdivisions and about 12% of the company location could not be identified. The distribution of job type was as follows: customer service call center, 5.8%, customer service in store, 15.0%, manufacturing, 5.5%, general labor, 29.3%, restaurant labor, 5.8%, restaurant server/host, 3.50%, driver, 7.0%, clerical, 10.0%, sales in store, 4.3%, warehouse/shipping, 9.0%, sales call center, 3.0%, and multiple job types, 2.0%. Approximately 73% of these positions noted full-time employment, while about 21% noted part-time employment and 7% did not note either. Finally, eighty three percent of the resumes were submitted to postings that were 1-4 days old and 17.00% were submitted to postings that were 5-14 days old.

Displayed in Table 4.3 are the distribution of control variables which could vary across the racial categories. As shown in Table 4.3, randomization of racial categories was successful. The minimum distance for the African American names was .4 miles, the median was 15.8 miles, and the maximum was 36.6 miles. The minimum distance for the White names was 1 mile, the median was 15.3 miles, and the maximum was 38.1 miles. The minimum hourly pay for the African American names was $8, the median was $12, and the maximum was $18.58. The minimum hourly pay for the White names $8, the median was $11, and the maximum was $18.58.

Table 4.3. Distribution of control variables across race.

Variables	African American	White	Total
Base Resume			
Base Resume 1	197 (49.3%)	203 (50.8%)	400 (100%)
Base Resume 2	203 (50.8%)	197 (49.3%)	400 (100%)
Month Submitted			
January	36 (54.6%)	30 (45.5%)	66 (100%)
February	137 (50.7%)	133 (49.3%)	270 (100%)
March	106 (48.6%)	112 (51.4%)	218 (100%)
April	55 (52.9%)	49 (47.1%)	104 (100%)
May	66 (46.5%)	76 (53.5%)	142 (100%)
Day Submitted			
Sunday – Thursday	351 (48.9%	367 (51.1%)	718 (100%)
Friday - Saturday	49 (59.8%)	33 (40.2%)	82 (100%)
Time Submitted			
AM	237 (48.8%)	249 (51.2%)	486 (100%)
PM	163 (51.9%)	151 (48.1%)	314 (100%)
Hourly Pay			

Below Median	89 (45.9%)	105 (54.1%)	194 (100%)
Above Median	94 (50.5%)	92 (49.5%)	186 (100%)
No Pay Listed	217 (51.7%)	203 (48.3%)	420 (100%)
Submitted First			
Yes	201 (50.3%)	199 (49.8%)	400 (100%)
No	199 (49.8%)	201 (50.3%)	400 (100%)
Distance			
Below Median	124 (49.0%)	129 (51.0%)	253 (100%)
Above Median	129 (51.0%)	124 (49.00%)	253 (100%)
No Address	147 (50.0%)	147 (50.0%)	294 (100%)
Full-Time Employment			
No	78 (47.6%)	86 (52.4%)	164 (100%)
Yes	293 (50.5%)	287 (49.5%)	580 (100%)
Not Listed	29 (51.8%)	27 (48.2%)	56 (100%)
Cleveland City			
No	262 (50.0%)	262 (50.0%)	524 (100%)
Yes	90 (49.5%)	92 (50.56%)	182 (100%)
No City Location	48 (51.1%)	46 (48.9%)	94 (100%)
Job Type			
Customer Service Call Center	21 (45.7%)	25 (54.4%)	46 (100%)
Customer Service In-Store	55 (45.8%)	65 (54.2%)	120 (100%)
Manufacturing	21 (47.7%)	23 (52.3%)	44 (100%)
General Labor	114 (48.7%)	120 (51.3%)	234 (100%)
Restaurant Labor	26 (56.5%)	20 (43.5%)	46 (100%)
Restaurant Customer Service	14 (50.0%)	14 (50.0%)	28 (100%)
Driving	32 (57.1%)	24 (42.9%)	56 (100%)
Clerical	36 (45.0%)	44 (55.0%)	80 (100%)
Sales In-Store	18 (52.9%)	16 (47.1%)	34 (100%)
Shipping/Warehouse	40 (55.6%)	32 (44.4%)	72 (100%)
Sales Call Center	15 (62.5%)	9 (37.5%)	24 (100%)
Multiple Job Types	8 (50.0%)	8 (50.0%)	16 (100%)
Posting Age			
1-4 days old	334 (50.3%)	330 (49.7%)	664 (100%)
5-14 days old	66 (48.5%)	70 (51.5%)	136 (100%)
Staffing Agency			
No	383 (49.9%)	385 (50.1%)	768 (100%)
Yes	17 (53.1%)	15 (46.9%)	32 (100%)
Temporary			
No	375 (50.4%)	369 (49.6%)	744 (100%)
Yes	25 (44.6%)	31 (55.4%)	56 (100%)

B. Bivariate Analyses

Table 4.4 displays the predicted probabilities of a callback for the pooled criminal record and racial conditions (see Table B.1 for the full logistic regression output).[26] As shown, the predicted probabilities were as follows: no record = .223, CQE = .153, African American = .163, White = .213. Average marginal effects in Table 4.5 show that the difference between the no record and record plus CQE categories was statistically significant ($p <= .001$). This result was confirmed with McNemar's test which examines difference in discordance with matched pairs. Callbacks by pair were as follows: 52 for both CQE and no record, 9 for CQE only, 37 for no record only, and 302 for neither (Exact McNemar significance probability = .000; McNemar's Chi square = 17.04; $df = 1$; $p <= .001$). Given the above directional hypothesis about the impact of race, the difference between African Americans and Whites was statistically significant ($p = .069$*). This result was confirmed with a Chi square test, $\chi 2 = 3.28$; $df = 1$; $p = .070$*.

Table 4.4. Probability of a callback for pooled criminal record and racial conditions.

Variable	Margin	SE	CI Lower	CI Upper
Record Type				
No Record	0.223	0.021	0.182	0.263
Record and CQE	0.153	0.018	0.117	0.188
Race				
African American	0.163	0.021	0.121	0.204
White	0.213	0.023	0.167	0.258

Table 4.5. Average marginal effects for pooled criminal record and racial conditions.

Variable – Base Outcome	Difference	SE	z	P>z	CI Lower	CI Upper
White – African Am.	0.050	0.028	1.82	0.069	-0.004	0.104

[26] Pooled results examine the racial and criminal record conditions separately. For example, Whites with no record and Whites with a record plus CQE would be combined into one group to determine the predicted probability of a callback for Whites.

CQE – No Record	-0.070	0.017	-4.21	0.000	-0.103	-0.037

C. Multiple Regression Analysis

Table 4.6 and Figure 4.1 display the predicted probabilities of a callback for criminal record type conditioned on race (see Table B.1 for the full logistic regression output). As shown, the predicted probabilities were as follows: African American no record = .194, African American CQE = .131, White no record = .251, White CQE = .174. Average marginal effects displayed in Table 4.7 showed that the difference between African Americans with no criminal record and African Americans with a criminal record and a CQE was statistically significant ($p <= .001$). The difference between Whites with no criminal record and Whites with a criminal record and a CQE was statistically significant ($p <= .001$). Average marginal effects also showed that the difference between Whites with no criminal record and African Americans with no criminal record was statistically significant ($p = .069$*). The difference between Whites with a criminal record and a CQE and African Americans with a criminal record and a CQE was statistically significant ($p = .071$*).

Table 4.6. Probability of a callback for criminal record type conditioned by race.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
African American	0.194	0.025	0.145	0.243
White	0.251	0.027	0.198	0.304
Record and CQE				
African American	0.131	0.020	0.092	0.170
White	0.174	0.023	0.129	0.219

Table 4.7. Average marginal effects of criminal record type conditioned by race.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am. CQE -						

African Am. No Record	-0.063	0.016	-4.02	0.000	-0.093	-0.032
White CQE - White No Record	-0.077	0.019	-4.15	0.000	-0.114	-0.041
White No Record - African Am. No Record	0.057	0.031	1.82	0.069	-0.004	0.119
White CQE - African Am. CQE	0.043	0.024	1.81	0.071	-0.004	0.089

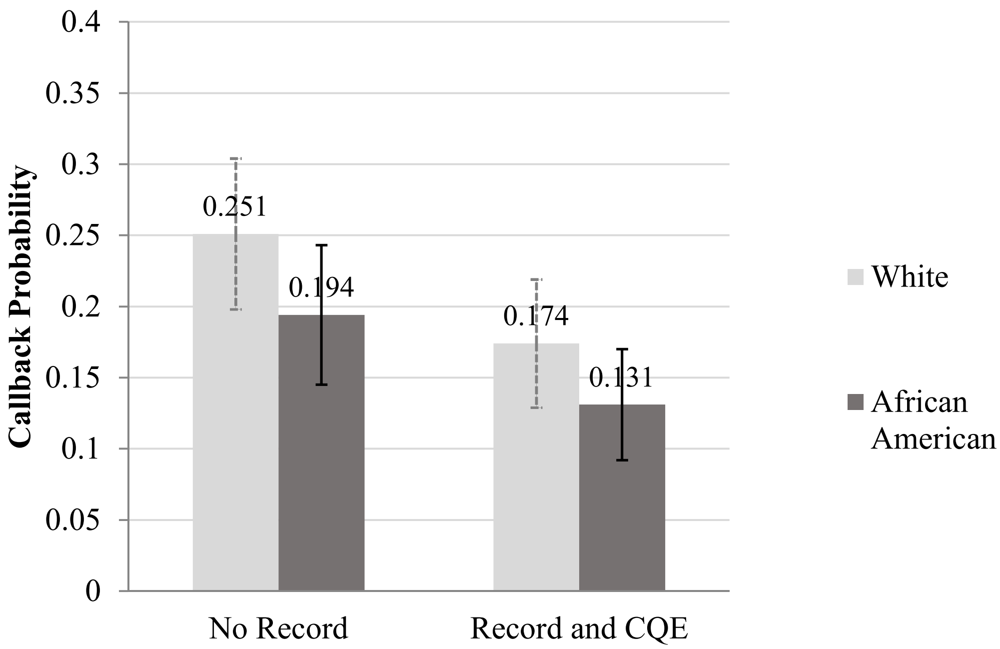

Figure 4.1. Probability of a callback for criminal record type conditioned by race with 95% confidence intervals.

D. Robustness Check Models

i. Control Variables

Table 4.8 displays the predicted probabilities of a callback for criminal record type conditioned by race with control variables included in the model (see Table B.2 for the full logistic regression output). As shown below in Table 4.8, the addition of the control variables did not significantly or substantively alter the above results. The predicted probabilities were as follows: African American no record = .196, African

American CQE = .133, White no record = .249, White CQE = .172. Average marginal effects which are displayed in Table 4.9 showed that the difference in predicted probabilities between African Americans with no criminal record and African Americans with a criminal record and a CQE was statistically significant (p <= .001). The difference between Whites with no criminal record and Whites with a criminal record and a CQE was statistically significant (p <= .001). Average marginal effects also showed that the difference between Whites with no criminal record and African Americans with no criminal record was statistically significant (p = .069*). The difference between Whites with a criminal record and a CQE and African Americans with a criminal record and a CQE was statistically significant (p = .070*).

Table 4.8. Probability of a callback for criminal record type conditioned by race with controls.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
African American	0.196	0.023	0.151	0.240
White	0.249	0.025	0.199	0.298
Record and CQE				
African American	0.133	0.018	0.097	0.169
White	0.172	0.022	0.130	0.215

Table 4.9. Average marginal effects of criminal record type conditioned by race with controls.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am. CQE - African Am. No Record	-0.063	0.016	-4.03	0.000	-0.094	-0.032
White CQE - White No Record	-0.076	0.018	-4.13	0.000	-0.112	-0.040
White No Record - African Am. No Record	0.053	0.029	1.82	0.069	-0.004	0.110
White CQE -	0.040	0.022	1.81	0.070	-0.003	0.083

Table C.1 in Appendix section C displays the predicted probabilities of a callback for the control variables and Table C.2 in appendix section C displays their average marginal effects. While the control variables did not alter the above results, a few points are worthy of note regarding overall impacts on callbacks. First, resumes submitted to staffing agencies had a probability of a callback of 66.6%, while those that were not submitted to a staffing agency had a probability of a callback of 16.7%, $p <= .001$. Second, resumes submitted to temporary positions had a probability of a callback of 26.8%, while all other positions had a probability of 18.1%, $p = 0.236$. Third, resumes submitted Sunday through Thursday had a probability of 20.1%, while those that were submitted Friday and Saturday had a probability of 7.9%, $p = 0.006$. Fourth, using general labor (callback probability = 24.9%) as the reference category, results indicated significant differences with clerical positions (7.1%, $p = 0.001$) and marginally significant differences with sales call center positions (52.8%, $p = 0.052$), customer service in store positions (15.3%, $p = 0.086$), manufacturing positions (12.9%, $p = 0.062$), and listings with multiple positions (10.3%, $p = 0.082$). Fifth, resumes submitted in February (21.2%) had a significantly higher probability of a callback than those submitted in April (11.8%), $p = 0.065$.

ii. Individual Names

Displayed in Table 4.10 are the predicted probabilities of a callback for criminal record type conditioned by individual name (see Table B.3 for the full logistic regression output). The predicted probabilities were as follows: DaQuan no record = .164, DaQuan CQE = .109, Jake no record = .244, Jake CQE = .168, Seth no record = .259, Seth CQE =

.180, Tremayne no record = .225, Tremayne CQE = .154. Average marginal effects displayed in Table 4.12 showed that the differences between the no criminal record and criminal record and CQE categories were statistically significant for each individual name ($p <= .001$). In Table 4.11, average marginal effects also showed that the difference between DaQuan with no record and Jake with no record was statistically significant ($p = .027$). The difference between DaQuan with a record and CQE and Jake with a record and CQE was statistically significant ($p = .027$). The difference between DaQuan with no record and Seth with no record was statistically significant ($p = .026$). The difference between DaQuan with a record and CQE and Seth with a record and CQE was statistically significant ($p = .030$). The difference between DaQuan with no record and Tremayne with no record was marginally significant ($p = .077$). The difference between DaQuan with a record and CQE and Tremayne with a record and CQE was marginally significant ($p = .080$). The difference between Jake with no record and Seth with no record was not statistically significant ($p = .685$). The difference between Jake with a record and CQE and Seth with a record and CQE was not statistically significant ($p = .686$). The difference between Jake with no record and Tremayne with no record was not statistically significant ($p = .692$). The difference between Jake with a record and CQE and Tremayne with a record and CQE was not statistically significant ($p = .692$). The difference between Seth with no record and Tremayne with no record was not statistically significant ($p = .371$). The difference between Seth with a record and CQE and Tremayne with a record and CQE was not statistically significant ($p = .373$).

Table 4.10. Probability of a callback for criminal record type conditioned by individual name.

Variable	Margin	SE	CI Lower	CI Upper
No Record				

DaQuan	0.164	0.028	0.109	0.219
Jake	0.244	0.033	0.178	0.309
Seth	0.259	0.033	0.195	0.324
Tremayne	0.225	0.033	0.161	0.290
CQE				
DaQuan	0.109	0.021	0.068	0.151
Jake	0.168	0.026	0.117	0.219
Seth	0.180	0.028	0.124	0.235
Tremayne	0.154	0.026	0.103	0.206

Table 4.11. Average marginal effects of criminal record type conditioned by individual name.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
Jake No Record v. DaQuan No Record	0.080	0.036	2.22	0.027	0.009	0.151
Jake CQE v. DaQuan CQE	0.059	0.027	2.21	0.027	0.007	0.111
Seth No Record v. DaQuan No Record	0.095	0.043	2.22	0.026	0.011	0.180
Seth CQE v. DaQuan CQE	0.071	0.033	2.17	0.030	0.007	0.134
Tremayne No Record v. DaQuan No Record	0.062	0.035	1.77	0.077	-0.007	0.130
Tremayne CQE v. DaQuan CQE	0.045	0.026	1.75	0.080	-0.005	0.096
Seth No Record v. Jake No Record	0.016	0.038	0.41	0.685	-0.06	0.091
Seth CQE v. Jake CQE	0.012	0.029	0.40	0.686	-0.046	0.069
Tremayne No Record v. Jake No Record	-0.018	0.046	-0.40	0.692	-0.108	0.072

Tremayne CQE v. Jake CQE	-0.014	0.034	-0.40	0.692	-0.081	0.054
Tremayne No Record v. Seth No Record	-0.034	0.038	-0.90	0.371	-0.107	0.040
Tremayne CQE v. Seth CQE	-0.025	0.029	-0.89	0.373	-0.082	0.031

Table 4.12. Average marginal effects of criminal record type conditioned by individual name continued.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
DaQuan CQE v. DaQuan No Record	-0.054	0.015	-3.73	0.000	-0.083	-0.026
Jake CQE v. Jake No Record	-0.076	0.019	-3.93	0.000	-0.113	-0.038
Seth CQE v. Seth No Record	-0.079	0.019	-4.16	0.000	-0.117	-0.042
Tremayne CQE v. Tremayne No Record	-0.071	0.018	-3.92	0.000	-0.107	-0.036

Displayed in Table 4.13 are the predicted probabilities of a callback for criminal record type conditioned by individual name with controls included in the model (see Table B.4 for the full logistic regression output). The predicted probabilities were as follows: DaQuan no record = .170, DaQuan CQE = .111, Jake no record = .253, Jake CQE = .173, Seth no record = .247, Seth CQE = .168, Tremayne no record = .225, Tremayne CQE = .152. The inclusion of the control variables did not significantly or substantively alter the above results. Average marginal effects displayed in Table 4.15 again showed that the differences between the no criminal record and criminal record and CQE categories were statistically significant for each individual name ($p <= .001$). In

Table 4.14, average marginal effects showed that the difference between DaQuan with no record and Jake with no record was statistically significant (p = .015). The difference between DaQuan with a record and CQE and Jake with a record and CQE was statistically significant (p = .014). The difference between DaQuan with no record and Seth with no record was now statistically significant (p = .058*) in this analysis. The difference between DaQuan with a record and CQE and Seth with a record and CQE was now statistically significant (p = .059*) in this analysis. The difference between DaQuan with no record and Tremayne with no record was marginally significant (p = .103). The difference between DaQuan with a record and CQE and Tremayne with a record and CQE was marginally significant (p = .098). The difference between Jake with no record and Seth with no record was not statistically significant (p = .866). The difference between Jake with a record and CQE and Seth with a record and CQE was not statistically significant (p = .866). The difference between Jake with no record and Tremayne with no record was not statistically significant (p = .502). The difference between Jake with a record and CQE and Tremayne with a record and CQE was not statistically significant (p = .503). The difference between Seth with no record and Tremayne with no record was not statistically significant (p = .540). The difference between Seth with a record and CQE and Tremayne with a record and CQE was not statistically significant (p = .542).

Table 4.13. Probability of a callback for criminal record type conditioned by individual name with controls.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
DaQuan	0.170	0.026	0.120	0.220
Jake	0.253	0.032	0.191	0.315
Seth	0.247	0.031	0.186	0.308
Tremayne	0.225	0.031	0.163	0.286

Record and CQE				
DaQuan	0.111	0.021	0.071	0.152
Jake	0.173	0.025	0.123	0.223
Seth	0.168	0.027	0.116	0.220
Tremayne	0.152	0.023	0.106	0.197

Table 4.14. Average marginal effects of criminal record type conditioned by individual name with controls.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
Jake No Record v. DaQuan No Record	0.083	0.034	2.43	0.015	0.016	0.151
Jake CQE v. DaQuan CQE	0.062	0.025	2.46	0.014	0.013	0.111
Seth No Record v. DaQuan No Record	0.077	0.041	1.90	0.058	-0.003	0.157
Seth CQE v. DaQuan CQE	0.057	0.03	1.88	0.059	-0.002	0.116
Tremayne No Record v. DaQuan No Record	0.055	0.034	1.63	0.103	-0.011	0.121
Tremayne CQE v. DaQuan CQE	0.040	0.024	1.66	0.098	-0.007	0.088
Seth No Record v. Jake No Record	-0.006	0.037	-0.17	0.866	-0.079	0.067
Seth CQE v. Jake CQE	-0.005	0.028	-0.17	0.866	-0.060	0.051
Tremayne No Record v. Jake No Record	-0.028	0.042	-0.67	0.502	-0.111	0.055
Tremayne CQE v. Jake CQE	-0.021	0.032	-0.67	0.503	-0.084	0.041
Tremayne No Record v.						

Seth No Record	-0.022	0.036	-0.61	0.540	-0.093	0.049
Tremayne CQE v. Seth CQE	-0.017	0.027	-0.61	0.542	-0.070	0.037

Table 4.15. Average marginal effects of criminal record type conditioned by individual name with controls continued.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
DaQuan CQE v. DaQuan No Record	-0.058	0.014	-4.1	0.000	-0.086	-0.030
Jake CQE v. Jake No Record	-0.080	0.02	-4.04	0.000	-0.119	-0.041
Seth CQE v. Seth No Record	-0.079	0.019	-4.19	0.000	-0.115	-0.042
Tremayne CQE v. Tremayne No Record	-0.073	0.019	-3.88	0.000	-0.110	-0.036

iii. Interactions

Table 4.16 below shows the predicted probabilities derived from of the logistic regression specified with an interaction between the race and criminal record treatment variables (see Table B.5 for the full logistic regression output). The predicted probabilities were as follows: African American no record = .180, African American CQE = .145, White no record = .265, White CQE = .160. These predicted probabilities possess only slight differences from the above main effects model. However, the interaction model produced several changes in statistical significance. Average marginal effects in Table 4.17 showed that the difference between African Americans with no criminal record and African Americans with a criminal record and a CQE was no longer statistically significant (p = .246). The difference between Whites with no criminal record and Whites with a criminal record and a CQE is still statistically significant (p = .002).

Average marginal effects also showed that the difference between Whites with no criminal record and African Americans with no criminal record was now statistically significant ($p = .040$). The difference between Whites with a criminal record and a CQE and African Americans with a criminal record and a CQE was no longer statistically significant ($p = .677$). However, it should be noted that both the Akaike information criterion (AIC) and the Bayesian information criterion (BIC) favored the main effects model. Additional interactions were also explored (race x city location; criminal record x city location; criminal record x job type; race x job type; race x criminal record x city location); however, results again indicated minimal differences and AIC and BIC still favored the main effects model.[27]

Table 4.16. Probability of a callback for the criminal record and race interaction.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
African American	0.180	0.027	0.127	0.233
White	0.265	0.031	0.204	0.326
Record and CQE				
African American	0.145	0.025	0.096	0.194
White	0.160	0.026	0.109	0.211

Table 4.17. Average marginal effects of the criminal record and race interaction.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am. CQE - African Am. No Record	-0.035	0.030	-1.16	0.246	-0.094	0.024
White CQE - White No Record	-0.105	0.034	-3.11	0.002	-0.171	-0.039
White No Record - African Am. No Record	0.085	0.041	2.05	0.040	0.004	0.166

[27] Race x Criminal Record x Job Type was also examined. However, this interaction caused significant instability in the model.

White CQE - African Am. CQE	0.015	0.036	0.42	0.677	-0.056	0.086

Table 4.18 below shows the predicted probabilities derived from of the logistic regression specified with an interaction between the race and criminal record treatment variables with the inclusion of the control variables (see Table B.6 for the full logistic regression output). The results of the control model did not significantly or substantively vary from the base interactive model. The predicted probabilities were as follows: African American no record = .184, African American CQE = .144, White no record = .260, White CQE = .161. These predicted probabilities possess only slight differences from the above main effects and interaction models. Average marginal effects in Table 4.19 showed that the difference between African Americans with no criminal record and African Americans with a criminal record and a CQE was now marginally significant (p = .166*). The difference between Whites with no criminal record and Whites with a criminal record and a CQE is statistically significant (p = .003). Average marginal effects also showed that the difference between Whites with no criminal record and African Americans with no criminal record was statistically significant (p = .049). The difference between Whites with a criminal record and a CQE and African Americans with a criminal record and a CQE was not statistically significant (p = .614).

Table 4.18. Probability of a callback for the criminal record and race interaction with controls.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
African American	0.184	0.025	0.135	0.233
White	0.260	0.030	0.202	0.318
Record and CQE				
African American	0.144	0.023	0.099	0.189
White	0.161	0.025	0.112	0.210

Table 4.19. Average marginal effects of the criminal record and race interaction with controls.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am. CQE - African Am. No Record	-0.040	0.029	-1.39	0.166	-0.096	0.016
White CQE - White No Record	-0.099	0.033	-2.98	0.003	-0.165	-0.034
White No Record - African Am. No Record	0.077	0.039	1.97	0.049	0.000	0.153
White CQE - African Am. CQE	0.017	0.034	0.50	0.614	-0.049	0.084

iv. Racial Groupings

Displayed in Table 4.20 are the callback probabilities for the African American only grouping (see Table B.7 for the full logistic regression output). The predicted probabilities were as follows: no record = .184, CQE = .126. Average marginal effects displayed in Table 4.21 show that the difference between those with a CQE and no record was statistically significant, p = .022. These results were confirmed with a McNemar's test. Callbacks by pair were as follows: 11 for both CQE and no record, 0 for CQE only, 5 for no record only, and 71 for neither (Exact McNemar significance probability = .063; McNemar's Chi square = 5.00; df = 1; p = .025).

Table 4.20. Probability of a callback for criminal record type for the African American only grouping.

Variable	Margin	SE	CI Lower	CI Upper
Record Type				
No Record	0.184	0.042	0.102	0.266
CQE	0.126	0.036	0.056	0.197

Table 4.21. Average marginal effects of criminal record type for the African American only grouping.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper

African Am. CQE - African Am. No Record	-0.057	0.025	-2.29	0.022	-0.107	-0.008

Displayed in Table 4.22 are the callback probabilities for the African American only grouping for a model including controls (see Table B.8 for the full logistic regression output). This model did not include job type as it caused instability in the model and observations to be lost.[28] The predicted probabilities were as follows: no record = .181, CQE = .130. Average marginal effects displayed in Table 4.23 show that the difference between those with a CQE and no record was statistically significant, p = .024. Therefore, including control variables resulted in only small differences.

Table 4.22. Probability of a callback for criminal record type for the African American only grouping with controls.

Variable	Margin	SE	CI Lower	CI Upper
Record Type				
No Record	0.181	0.036	0.109	0.252
CQE	0.130	0.029	0.072	0.188

Table 4.23. Average marginal effects of criminal record type for the African American only grouping with controls.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am. CQE - African Am. No Record	-0.05	0.022	-2.25	0.024	-0.094	-0.007

Displayed in Table 4.24 are the callback probabilities for the White only grouping (see Table B.9 for the full logistic regression output). The predicted probabilities were as follows: no record = .287, CQE = .195. Average marginal effects displayed in Table 4.25 show that the difference between those with a CQE and no record was statistically

[28] A model was also estimated that included job type as a predictor. Including this variable caused instability in the model and 30 observations were dropped. However, margins and average marginal effects varied only slightly and there was no change in statistical significance.

significant, p = .018. These results were confirmed with a McNemar's test. Callbacks by pair were as follows: 15 for both CQE and no record, 2 for CQE only, 10 for no record only, and 60 for neither (Exact McNemar significance probability = .039; McNemar's Chi square = 5.33; df = 1; p = .021).

Table 4.24. Probability of a callback for criminal record type for the White only grouping.

Variable	Margin	SE	CI Lower	CI Upper
Record Type				
No Record	0.287	0.049	0.192	0.383
CQE	0.195	0.043	0.112	0.279

Table 4.25. Average marginal effects of criminal record type for the White only grouping.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
White CQE - White No Record	-0.092	0.039	-2.37	0.018	-0.168	-0.016

Displayed in Table 4.26 are the callback probabilities for the White only grouping with controls added (see Table B.10 for the full logistic regression output). This model did not include job type as it caused instability in the model and observations to be lost.[29] The predicted probabilities were as follows: no record = .289, CQE = .194. Average marginal effects displayed in Table 4.27 show that the difference between those with a CQE and no record was statistically significant, p = .017. Therefore, including control variables resulted in very little differences.

Table 4.26. Probability of a callback for criminal record type for the White only grouping with controls.

Variable	Margin	SE	CI Lower	CI Upper
Record Type	0.289	0.047	0.197	0.380

[29] A model was also estimated that included job type as a predictor. Including this variable caused instability in the model and 8 observations were dropped. However, margins and average marginal effects varied only slightly and there was no change in statistical significance.

No Record				
CQE	0.194	0.035	0.126	0.263

Table 4.27. Average marginal effects of criminal record type for the White only grouping with controls.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
White CQE - White No Record	-0.094	0.040	-2.38	0.017	-0.172	-0.017

For the African and White grouping, callbacks by pair were as follows: 26 for both CQE and no record, 7 for CQE only, 22 for no record only, and 171 for neither (Exact McNemar significance probability = .008; McNemar's Chi square = 7.76; df = 1; p = .005). The callback rate for the no record group was .212, while the callback rate for the CQE group was .146. Displayed in Table 4.28 are the callback probabilities for the African American and White grouping by criminal record group and race (see Table B.11 for the full logistic regression output). The callback probabilities were as follows: no record African American = .200, no record White = .225, CQE African American = .137, CQE White = .155. Average marginal effects displayed in Table 4.29 show that the difference between African Americans with a CQE and no record was statistically significant, p = .006. The difference between Whites with a CQE and no record was statistically significant, p = .005. Average marginal effects also showed that the difference between African Americans and Whites with no record was not statistically significant, p = .344. The difference between African Americans and Whites with a CQE was not statistically significant, p = .348.

Table 4.28. Probability of a callback for criminal record type for the African American and White grouping.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
African American	0.200	0.030	0.141	0.259

White	0.225	0.031	0.165	0.285
Record and CQE				
African American	0.137	0.025	0.088	0.185
White	0.155	0.027	0.103	0.208

Table 4.29. Average marginal effects of criminal record type for the African American and White grouping.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am. CQE - African Am. No Record	-0.063	0.023	-2.77	0.006	-0.108	-0.018
White CQE - White No Record	-0.070	0.025	-2.84	0.005	-0.118	-0.022
White No Record - African Am. No Record	0.025	0.027	0.95	0.344	-0.027	0.078
White CQE - African Am. CQE	0.019	0.020	0.94	0.348	-0.021	0.058

Displayed in Table 4.30 are the callback probabilities for the African American and White grouping by criminal record group and race with controls added (see Table B.12 for the full logistic regression output). This model did not include job type as it caused instability in the model and observations to be lost.[30] The callback probabilities were as follows: no record African American = .199, no record White = .224, CQE African American = .137, CQE White = .156. Average marginal effects displayed in Table 4.31 show that the difference between African Americans with a CQE and no record was statistically significant, p = .007. The difference between Whites with a CQE and no record was statistically significant, p = .005. Average marginal effects also

[30] A model was also estimated that included job type as a predictor. Including this variable caused instability in the model and 60 observations were dropped. However, margins and average marginal effects varied only slightly and there was no change in statistical significance.

showed that the difference between African Americans and Whites with no record was not statistically significant, p = .350. The difference between African Americans and Whites with a CQE was not statistically significant, p = .356. Therefore, including control variables resulted in very little differences.

Table 4.30. Probability of a callback for criminal record type for the African American and White grouping with controls.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
African American	0.199	0.028	0.144	0.255
White	0.224	0.030	0.166	0.282
Record and CQE				
African American	0.137	0.024	0.091	0.184
White	0.156	0.027	0.103	0.209

Table 4.31. Average marginal effects of criminal record type for the African American and White grouping with controls.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am. CQE - African Am. No Record	-0.062	0.023	-2.71	0.007	-0.107	-0.017
White CQE - White No Record	-0.068	0.024	-2.78	0.005	-0.116	-0.020
White No Record - African Am. No Record	0.025	0.026	0.93	0.350	-0.027	0.076
White CQE - African Am. CQE	0.019	0.020	0.92	0.356	-0.021	0.058

v. First Call Analysis

Bivariate analysis showed that the difference between criminal record groups (60% of those with no record and 40% of those with a CQE were called first) was statistically significant, $\chi 2 = 3.60$; df = 1; p = .058*. The difference between the racial conditions (39.53% of African Americans and 59.57% of Whites were called first) was also statistically significant, $\chi 2 = 3.61$; df = 1; p = .058*. Displayed in Table 4.32 are the probabilities of being called first for the criminal record type conditioned by race (see Table B.13 for the full logistic regression output). The predicted probabilities were as follows: no record African American = .495, no record White = .676, CQE African American = .316, CQE White = .496. Average marginal effects displayed in Table 4.33 show that the difference between African Americans with a CQE and no record was not statistically significant, p = .216. The difference between Whites with a CQE and no record was not statistically significant, p = .216. Average marginal effects also showed that the difference between African Americans and Whites with no record was statistically significant, p = .087*. The difference between African Americans and Whites with a CQE was statistically significant, p = .085*.

Table 4.32. Probability of being called first for criminal record type conditioned by race.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
African American	0.495	0.098	0.303	0.687
White	0.676	0.082	0.516	0.837
Record and CQE				
African American	0.316	0.084	0.151	0.481
White	0.496	0.096	0.308	0.684

Table 4.33. Average marginal effects of being called first for criminal record type conditioned by race.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am. CQE - African Am. No Record	-0.179	0.145	-1.24	0.216	-0.463	0.105
White CQE - White No Record	-0.180	0.146	-1.24	0.216	-0.466	0.105
White No Record - African Am. No Record	0.181	0.106	1.71	0.087	-0.026	0.388
White CQE - African Am. CQE	0.180	0.104	1.72	0.085	-0.025	0.385

Displayed in Table 4.34 are the predicted probabilities of being called first for the criminal record type conditioned by race with the controls of submitted first and base resume included (see Table B.14 for the full logistic regression output). Only these variables were included to reduce instability in the model given the low number of observations. The predicted probabilities were as follows: no record African American = .507, no record White = .703, CQE African American = .287, CQE White = .481. Average marginal effects displayed in Table 4.35 show that the difference between African Americans with a CQE and no record was marginally significant, p = .114*. The difference between Whites with a CQE and no record was marginally significant, p = .116*. Average marginal effects also showed that the difference between African Americans and Whites with no record was statistically significant, p = .053*. The difference between African Americans and Whites with a CQE was statistically significant, p = .051*. Therefore, including control variables resulted in very little differences.

Table 4.34. Probability of being called first for criminal record type conditioned by race with controls.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
African American	0.507	0.090	0.331	0.683
White	0.703	0.081	0.543	0.862
Record and CQE				
African American	0.287	0.086	0.118	0.456
White	0.481	0.090	0.305	0.657

Table 4.35. Average marginal effects of being called first for criminal record type conditioned by race with controls.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am. CQE - African Am. No Record	-0.22	0.139	-1.58	0.114	-0.492	0.053

White CQE - White No Record	-0.222	0.141	-1.57	0.116	-0.498	0.055
White No Record - African Am. No Record	0.196	0.101	1.94	0.053	-0.002	0.394
White CQE - African Am. CQE	0.194	0.099	1.95	0.051	-0.001	0.389

4.2 BETWEEN-SUBJECTS DESIGN RESULTS

A. Descriptive Statistics

A total of 1200 resumes were submitted to 1200 employers. Table 4.36 shows the distribution of treatment variables. Overall, 176 of the 1200 (14.67%) hypothetical applicants received an invitation to continue in the hiring process.

Table 4.36. Distribution of treatment variables.

	No Record	Record	CQE	Total
Race				
White	200 (33.3%)	200 (33.3%)	200 (33.3%)	400 (100%)
African American	200 (33.3%)	200 (33.3%)	200 (33.3%)	400 (100%)

Table 4.37 displays the distribution of control variables across criminal record type. As shown in Table 4.37, randomization was successful. The minimum hourly pay for the no record group was $8.5, the median was $12, and the maximum was $23. The minimum hourly pay for the record group was $8, the median was $11, and the maximum was $20. The minimum hourly pay for the record and CQE group was $8.5, the median was $12, and the maximum was $18. The minimum distance for the no record group was 2.2 miles, the median was 17.4 miles, and the maximum was 37.5 miles. The minimum distance for the record group was 1.5 miles, the median was 15.6 miles, and the maximum was 36.5 miles. The minimum distance for the record and CQE group was 1.8 miles, the median was 14.3 miles, and the maximum was 38.7 miles.

Table 4.37. Distribution of control variables across criminal record type.

Variable	No Record	Record	CQE	Total
Month Submitted				
January	28 (26.2%)	41 (38.3%)	38 (35.5%)	107 (100%)
February	132 (33.1%)	133 (33.3%)	134 (33.6%)	399 (100%)
March	129 (33.6%)	131 (34.1%)	124 (32.3%)	384 (100%)
April	36 (34.3%)	37 (35.2%)	32 (30.5%)	105 (100%)
May	75 (36.6%)	58 (28.3%)	72 (35.1%)	205 (100%)
Day Submitted				
Sunday – Thursday	373 (33.0%)	380 (33.6%)	379 (33.5%)	1132 (100%)
Friday - Saturday	27 (39.7%)	20 (29.4%)	21 (30.9%)	68 (100%)
Time Submitted				
AM	256 (34.0%)	240 (31.8%)	258 (34.2%)	754 (100%)
PM	144 (32.3%)	160 (35.9%)	142 (31.8)	446 (100%)
Hourly Pay				
Below Median	114 (32.0%)	120 (33.7%)	122 (34.3%)	356 (100%)
Above Median	53 (36.1%)	44 (29.9%)	50 (34.0%)	147 (100%)
No Pay Listed	233 (33.4%)	236 (33.9%)	228 (32.7%)	697 (100%)
Distance				
Below Median	111 (31.4%)	122 (34.6%)	120 (34.0%)	353 (100%)
Above Median	129 (37.0%)	113 (32.4%)	107 (30.7%)	349 (100%)
No Address	160 (32.1%)	165 (33.1%)	173 (34.7%)	498 (100%)
Full-Time Employment				
No	108 (35.5%)	93 (30.6%)	103 (33.9%)	304 (100%)
Yes	262 (32.0%)	286 (35.0%)	270 (33.0%)	818 (100%)
Not Listed	30 (38.5%)	21 (26.9%)	27 (34.6%)	78 (100%)
Cleveland City				
No	262 (34.4%)	255 (33.4%)	246 (32.2%)	763 (100%)
Yes	99 (31.8%)	103 (33.1%)	109 (35.1%)	311 (100%)
No City Location	39 (31.0%)	42 (33.3%)	45 (35.7%)	126 (100%)
Job Type				
Cust. Serv. Call Center	16 (31.4%)	17 (33.3%)	18 (25.3%)	51 (100%)
Cust. Serv. In-Store	59 (36.2%)	49 (30.1%)	55 (33.7%)	163 (100%)
Manufacturing	23 (39.0%)	21 (35.6%)	15 (25.4%)	59 (100%)
General Labor	135 (34.1%)	132 (33.3%)	129 (32.6%)	396 (100%)
Restaurant Labor	43 (29.3%)	53 (36.1%)	51 (34.7%)	147 (100%)
Restaurant Cust. Serv.	20 (24.4%)	34 (41.5%)	28 (34.2%)	82 (100%)
Driving	18 (43.9%)	9 (22.0%)	14 (34.2%)	41 (100%)
Clerical	25 (28.4%)	35 (39.8%)	28 (31.8%)	88 (100%)
Sales In-Store	11 (31.4%)	10 (28.6%)	14 (40.0%)	35 (100%)
Shipping/Warehouse	29 (37.2%)	23 (29.5%)	26 (33.3%)	78 (100%)
Sales Call Center	11 (30.6%)	11 (30.6%)	14 (38.9%)	36 (100%)
Multiple Job Types	10 (41.7%)	6 (25.0%)	8 (33.3%)	24 (100%)
Posting Age				
1-4 days old	317 (33.8%)	312 (33.3%)	308 (32.9%)	937 (100%)
5-14 days old	83 (31.6%)	88 (33.5%)	92 (34.5%)	263 (100%)

Staffing Agency				
No	390 (33.3%)	390 (33.3%)	391 (33.4%)	1171 (100%)
Yes	10 (34.5%)	10 (34.5%)	9 (31.0%)	29 (100%)
Temporary				
No	378 (33.5%)	378 (33.5%)	371 (32.9%)	1127 (100%)
Yes	22 (30.1%	22 (30.1%)	29 (39.7%)	73 (100%)

Displayed in Table 4.38 is the distribution of control variables across race. As shown in Table 4.38, randomization was successful. The minimum hourly pay for African Americans was $8, the median was $11.5, and the maximum was $18. The minimum hourly pay for Whites was $8.55, the median was $12, and the maximum was $23. The minimum distance for African Americans was 1.5 miles, the median was 16.9 miles, and the maximum was 36.5 miles. The minimum distance for Whites was 1.7 miles, the median was 14.9 miles, and the maximum was 38.7 miles.

Table 4.38. Distribution of control variables across race.

Variable	African American	White	Total
Month Submitted			
January	55 (51.4%)	52 (48.6%)	107 (100%)
February	193 (48.4%)	206 (51.6%)	399 (100%)
March	201 (52.3%)	183 (47.7%)	384 (100%)
April	56 (53.3%)	49 (46.7%)	105 (100%)
May	95 (46.3%)	110 (53.7%)	205 (100%)
Day Submitted			
Sunday – Thursday	565 (49.9%)	567 (50.1%)	1132 (100%)
Friday - Saturday	35 (51.5%)	33 (48.5%)	68 (100%)
Time Submitted			
AM	375 (49.7%)	379 (50.3%)	754 (100%)
PM	225 (50.5%)	221 (49.6)	446 (100%)
Hourly Pay			
Below Median	174 (48.9%)	182 (51.1%)	356 (100%)
Above Median	64 (43.5%)	83 (56.5%)	147 (100%)
No Pay Listed	362 (51.9%)	335 (48.1%)	697 (100%)
Distance			
Below Median	164 (46.5%)	189 (53.5%)	353 (100%)
Above Median	181 (51.9%)	168 (48.1%)	349 (100%)
No Address	255 (51.2%)	243 (48.8%)	498 (100%)
Full-Time Employment			
No	167 (54.9%)	137 (45.1%)	304 (100%)
Yes	398 (48.7%)	420 (51.3%)	818 (100%)

Not Listed	35 (44.9%)	43 (55.1%)	78 (100%)
Cleveland City			
No	376 (49.3%)	387 (50.7%)	763 (100%)
Yes	157 (50.5%)	154 (49.5%)	311 (100%)
No City Location	67 (53.2%)	59 (46.8%)	126 (100%)
Job Type			
Cust. Serv. Call Center	28 (54.9%)	23 (45.1%)	51 (100%)
Cust. Serv. In-Store	83 (50.9%)	80 (49.1%)	163 (100%)
Manufacturing	25 (42.4%)	34 (57.6%)	59 (100%)
General Labor	198 (50.0%)	198 (50.0%)	396 (100%)
Restaurant Labor	75 (51.0%)	72 (49.0%)	147 (100%)
Restaurant Cust. Serv.	45 (54.9%)	37 (45.1%)	82 (100%)
Driving	16 (39.0%)	25 (61.0%)	41 (100%)
Clerical	43 (48.9%)	45 (51.1%)	88 (100%)
Sales In-Store	19 (51.3%)	16 (45.7%)	35 (100%)
Shipping/Warehouse	35 (44.9%)	43 (55.1%)	78 (100%)
Sales Call Center	22 (61.1%)	14 (38.9%)	36 (100%)
Multiple Job Types	11 (45.9%)	13 (54.2%)	24 (100%)
Posting Age			
1-4 days old	477 (50.9%)	460 (49.1%)	937 (100%)
5-14 days old	123 (46.8%)	140 (53.2%)	263 (100%)
Staffing Agency			
No	585 (50.0%)	586 (50.0%)	1171 (100%)
Yes	15 (51.7%)	14 (48.3%)	29 (100%)
Temporary			
No	563 (50.0%)	564 (50.0%)	1127 (100%)
Yes	37 (50.7%)	36 (49.3%)	73 (100%)

B. Bivariate Analyses

Table 4.39 displays the predicted probability of a callback for the treatment variables (see Table B.15 for the full logistic regression output). As shown, the predicted probabilities were as follows: no record = .220, record = .123, CQE = .098, African American = .108, White = .185. Displayed in Table 4.40, average marginal effects showed that the difference between the no record and record and CQE categories was statistically significant ($p <= .001$). This result was confirmed with a Chi square test, $\chi 2 = 22.47$; $df = 1$; $p <= .001$. The difference between the no record and record categories was statistically significant ($p <= .001$). This result was confirmed with a Chi square test, $\chi 2 =$

13.40; *df* = 1; *p* <= *.001*. The difference between the record and record with CQE categories was not statistically significant (p = .256). This result was confirmed with a Chi square test, $\chi2$ = 1.28; *df* = 1; *p* = .258. The difference between African Americans and Whites was statistically significant (p <= .001). This result was confirmed with a Chi square test, $\chi2$ = 14.09; *df* = 1; *p* <= *.001*.

Table 4.39. Probability of a callback for the pooled criminal record and racial conditions.

Variable	Margin	SE	CI Lower	CI Upper
Record Type				
No Record	0.220	0.021	0.180	0.260
Record	0.123	0.016	0.091	0.154
Record and CQE	0.098	0.015	0.069	0.126
Race				
African American	0.108	0.013	0.084	0.133
White	0.185	0.016	0.154	0.216

Table 4.40. Average marginal effects of a callback for the pooled criminal record and racial conditions.

Variable – Base Outcome	Difference	SE	z	P>z	CI Lower	CI Upper
White – African American	0.077	0.020	3.82	0.000	0.037	0.116
No Record - CQE	0.122	0.025	4.84	0.000	0.073	0.172
Record - CQE	0.025	0.022	1.14	0.256	-0.018	0.068
No Record - Record	0.098	0.026	3.72	0.000	0.046	0.149

C. Multiple Regression Analysis

Table 4.41 and Figure 4.2 display the predicted probability of a callback for each criminal record separated by race (see Table B.15 for the full logistic regression output). As shown, the predicted probabilities were as follows: African American no record = .166, African American record = .089, African American CQE = .070, White no record = .274, White record = .156, White CQE = .125. In Table 4.42, average marginal effects showed that the difference between African Americans with no criminal record and African Americans with a criminal record and a CQE was statistically significant (p <= .001). The difference between African Americans with no record and African Americans with a record was statistically significant (p <= .001). The difference between African Americans with a record and African Americans with a record and a CQE was not statistically significant (p = .258).

The difference between Whites with no criminal record and Whites with a criminal record and a CQE was statistically significant (p <= .001). The difference between Whites with no criminal record and Whites with a criminal record was statistically significant (p <= .001). The difference between Whites with a criminal record and Whites with a criminal record and a CQE was not statistically significant (p = .256).

Average marginal effects also showed that the difference between Whites with no criminal record and African Americans with no criminal record was statistically significant (p <= .001). The difference between Whites with a criminal record and African Americans with a criminal record was statistically significant (p <= .001). The difference between Whites with a criminal record and a CQE and African Americans with a criminal record and a CQE was statistically significant (p <= .001).

Table 4.41. Probability of a callback for criminal record type conditioned by race.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
African American	0.166	0.022	0.123	0.209
White	0.274	0.028	0.219	0.329
Record				
African American	0.089	0.015	0.059	0.118
White	0.156	0.022	0.113	0.199
Record and CQE				
African American	0.070	0.013	0.045	0.096
White	0.125	0.020	0.086	0.164

Table 4.42. Average marginal effects of a callback for criminal record type conditioned by race.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am No Record - African Am. CQE	0.096	0.021	4.50	0.000	0.054	0.138
African Am. Record - African Am. CQE	0.019	0.017	1.13	0.258	-0.014	0.052
African Am. No Record - African Am. Record	0.077	0.022	3.57	0.000	0.035	0.119
White No Record - White CQE	0.149	0.031	4.77	0.000	0.088	0.210
White Record - White CQE	0.031	0.027	1.14	0.256	-0.023	0.085
White No Record - White Record	0.118	0.032	3.69	0.000	0.055	0.181
White No Record - African Am. No Record	0.108	0.029	3.75	0.000	0.052	0.164
White Record - African Am. Record	0.067	0.019	3.58	0.000	0.030	0.104
White CQE - African Am. CQE	0.055	0.016	3.48	0.001	0.024	0.086

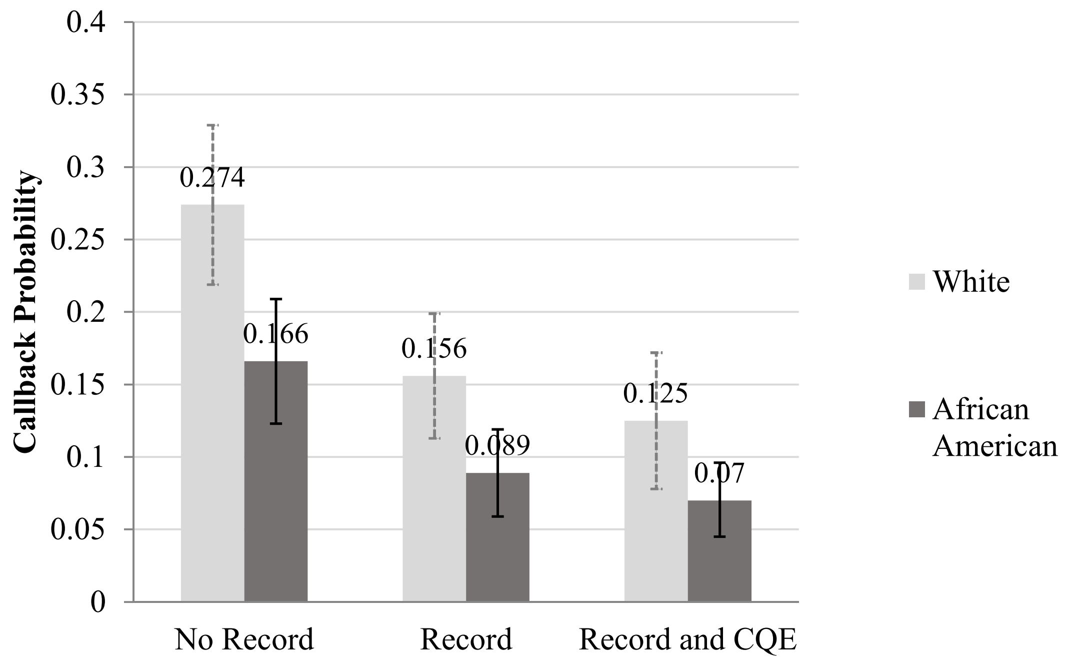

Figure 4.2. Probability of a callback for criminal record type conditioned by race with 95% confidence intervals.

D. Robustness Checks

i. Control Variables

Table 4.43 displays the predicted probability of a callback for each criminal record separated by race (see Table B.16 for the full logistic regression output). As shown, the predicted probabilities were as follows: African American no record = .165, African American record = .091, African American CQE = .069, White no record = .268, White record = .159, White CQE = .124. In Table 4.44, average marginal effects showed that the difference between African Americans with no criminal record and African Americans with a criminal record and a CQE was statistically significant ($p <= .001$). The difference between African Americans with no record and African Americans with a record was statistically significant ($p = .001$). The difference between African Americans with a record and African Americans with a record and a CQE was not statistically significant ($p = .190$).

The difference between Whites with no criminal record and Whites with a criminal record and a CQE was statistically significant (p <= .001). The difference between Whites with no criminal record and Whites with a criminal record was statistically significant (p <= .001). The difference between Whites with a criminal record and Whites with a criminal record and a CQE was not statistically significant (p = .186).

Average marginal effects also showed that the difference between Whites with no criminal record and African Americans with no criminal record was statistically significant (p <= .001). The difference between Whites with a criminal record and African Americans with a criminal record was statistically significant (p <= .001). The difference between Whites with a criminal record and a CQE and African Americans with a criminal record and a CQE was statistically significant (p <= .001). Therefore, inclusion of the control variables did not substantively or significantly alter the above results.

Table 4.43. Probability of a callback for criminal record type conditioned by race with controls.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
African American	0.165	0.021	0.123	0.206
White	0.268	0.027	0.216	0.321
Record				
African American	0.091	0.015	0.061	0.121
White	0.159	0.022	0.117	0.201
Record and CQE				
African American	0.069	0.013	0.044	0.094
White	0.124	0.019	0.086	0.161

Table 4.44. Average marginal effects of criminal record type conditioned by race with controls.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am. No Record -						

African Am. CQE	0.095	0.021	4.58	0.000	0.055	0.136
African Am. Record - African Am. CQE	0.022	0.017	1.31	0.190	-0.011	0.055
African Am. No Record - African Am. Record	0.073	0.021	3.45	0.001	0.032	0.115
White No Record - White CQE	0.145	0.030	4.83	0.000	0.086	0.204
White Record - White CQE	0.036	0.027	1.32	0.186	-0.017	0.088
White No Record - White Record	0.109	0.031	3.52	0.000	0.048	0.170
White No Record - African Am. No Record	0.104	0.028	3.76	0.000	0.050	0.158
White Record - African Am. Record	0.068	0.019	3.62	0.000	0.031	0.105
White CQE - African Am. CQE	0.054	0.016	3.49	0.000	0.024	0.085

Table C.5 in Appendix section C displays the predicted probabilities of a callback for the control variables and Table C.6 in Appendix section C displays their average marginal effects. While the control variables did not alter the above results, a few points are worthy of note regarding overall impacts on callbacks. First, resumes submitted to staffing agencies had a probability of a callback of 33.2%, while those that were not had a probability of a callback of 14.2%, $p = 0.029$. Second, resumes submitted to temporary positions had a probability of a callback of 20.2%, while all other positions had a probability of 14.3%, $p = 0.200$. Third, resumes submitted Sunday through Thursday had a probability of 15.1%, while those that were submitted Friday and Saturday had a probability of 8.3%, $p = 0.047$. Fourth, using general labor as the reference category (callback probability = 17.4%), results indicated significant differences with clerical positions (5%, $p = 0.000$) and restaurant customer service positions (9.2%, $p = 0.041$) and marginally significant differences with sales call center positions (31.5%, $p = 0.079$), customer service call center positions (9.4%, $p = 0.063$), manufacturing positions (10.3%, $p = 0.078$), and listings with multiple positions (10.3%, $p = 0.082$). Further, while there was not a statistically significant difference with general labor, driving positions had a callback probability of 27.1%, $p = 0.158$. Fifth, postings that did not list full-time or part-time employment (22.6%) had a significantly higher probability of a callback than those that listed part-time employment (11.5%), $p = 0.030$.

ii. Individual Names

Displayed below in Table 4.45, the predicted probabilities for individual names were as follows: DaQuan no record = .172, DaQuan record = .090, DaQuan CQE = .071, Jake no record = .307, Jake record = .175, Jake CQE = .141, Seth no record = .247, Seth record = .136, Seth CQE = .108, Tremayne no record = .164, Tremayne record = .086, Tremayne CQE = .068 (see Table B.17 for the full logistic regression output). In Table 4.47, average marginal effects showed that the differences between the no criminal record and criminal record and CQE categories were statistically significant for each individual name ($p <= .001$). The differences between the no criminal record and record categories were also statistically significant for each individual name ($p = .001$ for DaQuan and $p <= .001$ for all other names). The difference between the criminal record and criminal record with CQE categories was not statistically significant for DaQuan ($p = .264$). The difference between the criminal record and criminal record with CQE categories was not statistically significant for Jake (p = .260). The difference between the criminal record and criminal record with CQE categories was not statistically significant for Seth (p = .262). The difference between the criminal record and criminal record with CQE categories was not statistically significant for Tremayne (p = .266).

In Table 4.46, average marginal effects also showed that the difference between DaQuan with no record and Jake with no record was statistically significant (p = .001). The difference between DaQuan with a record and Jake with a record was statistically significant (p = .002). The difference between DaQuan with a record and CQE and Jake with a record and CQE was statistically significant (p = .002). The difference between DaQuan with no record and Seth with no record was statistically significant (p = .057*). The difference between DaQuan with a record and Seth with a record was statistically significant (p = .065*). The difference between DaQuan with a record and CQE and Seth with a record and CQE was statistically significant (p = .067*). The difference between DaQuan with no record and Tremayne with no record was not statistically significant (p = .825). The difference between DaQuan with a record and Tremayne with a record was not statistically significant (p = .825) The difference between DaQuan with a record and CQE and Tremayne with a record and CQE was not statistically significant (p = .825). The difference between Jake with no record and Seth with no record was not statistically significant (p = .167). The difference between Jake with a record and Seth with a record was not statistically significant (p = .166). The difference between Jake with a record and CQE and Seth with a record and CQE was not statistically significant (p = .168). The difference between Jake with no record and Tremayne with no record was statistically significant (p = .001). The difference between Jake with a record and Tremayne with a record was statistically significant (p = .001). The difference between Jake with a record and CQE and Tremayne with a record and CQE was not statistically significant (p = .001). The difference between Seth with no record and Tremayne with no record was statistically significant (p = .032). The difference between Seth with a record and Tremayne with a record was statistically significant (p = .036). The difference between Seth with a record and CQE and Tremayne with a record and CQE was statistically significant (p = .039).

Table 4.45. Probability of a callback for criminal record type conditioned by individual name.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
DaQuan	0.172	0.030	0.113	0.231
Jake	0.307	0.038	0.232	0.381
Seth	0.247	0.033	0.183	0.311
Tremayne	0.164	0.028	0.110	0.218
Record				
DaQuan	0.090	0.019	0.054	0.127
Jake	0.175	0.028	0.121	0.229
Seth	0.136	0.024	0.088	0.183
Tremayne	0.086	0.018	0.050	0.121
Record and CQE				
DaQuan	0.071	0.016	0.040	0.103
Jake	0.141	0.025	0.092	0.189
Seth	0.108	0.021	0.067	0.150
Tremayne	0.068	0.015	0.038	0.097

Table 4.46. Average marginal effect of criminal record type conditioned by individual name.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
Jake No Record - DaQuan No Record	0.135	0.042	3.21	0.001	0.052	0.217
Jake Record - DaQuan Record	0.084	0.027	3.09	0.002	0.031	0.138

Jake CQE - DaQuan CQE	0.069	0.023	3.03	0.002	0.024	0.114
Seth No Record - DaQuan No Record	0.075	0.04	1.90	0.057	-0.002	0.153
Seth Record - DaQuan Record	0.045	0.025	1.84	0.065	-0.003	0.094
Seth CQE - DaQuan CQE	0.037	0.020	1.83	0.067	-0.003	0.077
Tremayne No Record - DaQuan No Record	-0.008	0.037	-0.22	0.825	-0.081	0.065
Tremayne Record - DaQuan Record	-0.005	0.021	-0.22	0.825	-0.047	0.037
Tremayne CQE - DaQuan CQE	-0.004	0.017	-0.22	0.825	-0.037	0.030
Seth No Record - Jake No Record	-0.060	0.043	-1.38	0.167	-0.144	0.025
Seth Record - Jake Record	-0.039	0.028	-1.39	0.166	-0.094	0.016
Seth CQE - Jake CQE	-0.032	0.023	-1.38	0.168	-0.078	0.014
Tremayne No Record - Jake No Record	-0.143	0.042	-3.43	0.001	-0.225	-0.061
Tremayne Record - Jake Record	-0.089	0.027	-3.34	0.001	-0.141	-0.037
Tremayne CQE - Jake CQE	-0.073	0.023	-3.24	0.001	-0.117	-0.029

Tremayne No Record - Seth No Record	-0.083	0.039	-2.15	0.032	-0.160	-0.007
Tremayne Record - Seth Record	-0.050	0.024	-2.1	0.036	-0.097	-0.003
Tremayne CQE - Seth CQE	-0.041	0.020	-2.07	0.039	-0.079	-0.002

Table 4.47. Average marginal effect of criminal record type conditioned by individual name continued.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
DaQuan No record - DaQuan CQE	0.101	0.025	4.08	0.000	0.052	0.149
Jake No Record - Jake CQE	0.166	0.036	4.62	0.000	0.096	0.236
Seth No Record - Seth CQE	0.139	0.030	4.64	0.000	0.080	0.197
Tremayne No Record - Tremayne CQE	0.096	0.023	4.22	0.000	0.051	0.141
DaQuan Record - DaQuan CQE	0.019	0.017	1.12	0.264	-0.014	0.053
Jake Record - Jake CQE	0.034	0.030	1.13	0.260	-0.025	0.094
Seth Record - Seth CQE	0.028	0.025	1.12	0.262	-0.021	0.076
Tremayne Record - Tremayne CQE	0.018	0.016	1.11	0.266	-0.014	0.050
DaQuan No Record - DaQuan Record	0.081	0.024	3.38	0.001	0.034	0.129
Jake No Record - Jake Record	0.132	0.036	3.67	0.000	0.061	0.202

Seth No Record - Seth Record	0.111	0.030	3.71	0.000	0.052	0.170
Tremayne No Record - Tremayne Record	0.078	0.022	3.50	0.000	0.034	0.122

Displayed below in Table 4.48 are the predicted probabilities for the criminal record conditions separated by individual names with control variables added to the model (see Table B.18 for the full logistic regression output). The predicted probabilities were as follows: DaQuan no record = .167, DaQuan record = .092, DaQuan CQE = .070, Jake no record = .284, Jake record = .168, Jake CQE = .131, Seth no record = .254, Seth record = .148, Seth CQE = .115, Tremayne no record = .164, Tremayne record = .090, Tremayne CQE = .068. In Table 4.50, average marginal effects showed that the differences between the no criminal record and criminal record and CQE categories were statistically significant for each individual name ($p <= .001$). The differences between the no criminal record and record categories were also statistically significant for each individual name ($p <= .001$ for Seth and .001 for all other names). The difference between the criminal record and criminal record with CQE categories was not statistically significant for DaQuan ($p = .198$). The difference between the criminal record and criminal record with CQE categories was not statistically significant for Jake ($p = .191$). The difference between the criminal record and criminal record with CQE categories was not statistically significant for Seth ($p = .193$). The difference between the criminal record and criminal record with CQE categories was not statistically significant for Tremayne ($p = .198$).

In Table 4.49, average marginal effects also showed that the difference between DaQuan with no record and Jake with no record was statistically significant (p = .004). The difference between DaQuan with a record and Jake with a record was statistically significant (p = .005). The difference between DaQuan with a record and CQE and Jake with a record and CQE was statistically significant (p = .007). The difference between DaQuan with no record and Seth with no record was statistically significant (p = .023). The difference between DaQuan with a record and Seth with a record was statistically significant (p = .029). The difference between DaQuan with a record and CQE and Seth with a record and CQE was statistically significant (p = .031). The difference between DaQuan with no record and Tremayne with no record was not statistically significant (p = .930). The difference between DaQuan with a record and Tremayne with a record was not statistically significant (p = .930) The difference between DaQuan with a record and CQE and Tremayne with a record and CQE was not statistically significant (p = .930). The difference between Jake with no record and Seth with no record was not statistically significant (p = .498). The difference between Jake with a record and Seth with a record was not statistically significant (p = .497). The difference between Jake with a record and CQE and Seth with a record and CQE was not statistically significant (p = .498). The difference between Jake with no record and Tremayne with no record was statistically significant (p = .002). The difference between Jake with a record and Tremayne with a record was statistically significant (p = .003). The difference between Jake with a record and CQE and Tremayne with a record and CQE was not statistically significant (p = .003). The difference between Seth with no record and Tremayne with no record was statistically significant (p = .027). The difference between Seth with a record and

Tremayne with a record was statistically significant (p = .030). The difference between Seth with a record and CQE and Tremayne with a record and CQE was statistically significant (p = .033). Therefore, except for the DaQuan and Seth comparisons becoming statistically significant without use of a one-sided test, the inclusion of the control variables did not substantively or significantly alter the above results.

Table 4.48. Probability of a callback for criminal record type conditioned by individual name with controls.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
DaQuan	0.167	0.029	0.110	0.224
Jake	0.284	0.036	0.214	0.354
Seth	0.254	0.033	0.189	0.320
Tremayne	0.164	0.028	0.110	0.218
Record				
DaQuan	0.092	0.019	0.055	0.129
Jake	0.168	0.027	0.116	0.221
Seth	0.148	0.026	0.097	0.199
Tremayne	0.090	0.019	0.052	0.127
Record and CQE				
DaQuan	0.070	0.016	0.039	0.100
Jake	0.131	0.023	0.085	0.177
Seth	0.115	0.022	0.071	0.158
Tremayne	0.068	0.016	0.037	0.099

Table 4.49. Average marginal effects of criminal record type conditioned by individual name with controls.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
Jake No Record - DaQuan No Record	0.117	0.041	2.85	0.004	0.036	0.197
Jake Record - DaQuan Record	0.077	0.028	2.78	0.005	0.023	0.131
Jake CQE - DaQuan CQE	0.062	0.023	2.71	0.007	0.017	0.106
Seth No Record -						

DaQuan No Record	0.087	0.038	2.27	0.023	0.012	0.162
Seth Record - DaQuan Record	0.056	0.026	2.19	0.029	0.006	0.106
Seth CQE - DaQuan CQE	0.045	0.021	2.15	0.031	0.004	0.086
Tremayne No Record - DaQuan No Record	-0.003	0.037	-0.09	0.930	-0.077	0.070
Tremayne Record - DaQuan Record	-0.002	0.023	-0.09	0.930	-0.047	0.043
Tremayne CQE - DaQuan CQE	-0.002	0.018	-0.09	0.930	-0.037	0.034
Seth No Record - Jake No Record	-0.030	0.044	-0.68	0.498	-0.115	0.056
Seth Record - Jake Record	-0.020	0.030	-0.68	0.497	-0.079	0.038
Seth CQE - Jake CQE	-0.017	0.025	-0.68	0.498	-0.065	0.032
Tremayne No Record - Jake No Record	-0.120	0.039	-3.05	0.002	-0.197	-0.043
Tremayne Record - Jake Record	-0.079	0.026	-3.02	0.003	-0.130	-0.028
Tremayne CQE - Jake CQE	-0.063	0.021	-2.94	0.003	-0.105	-0.021
Tremayne No Record - Seth No Record	-0.090	0.041	-2.21	0.027	-0.171	-0.010
Tremayne Record - Seth Record	-0.058	0.027	-2.17	0.030	-0.111	-0.006

Tremayne CQE - Seth CQE	-0.046	0.022	-2.13	0.033	-0.089	-0.004

Table 4.50. Average marginal effects of criminal record type conditioned by individual name with controls continued.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
DaQuan No record - DaQuan CQE	0.097	0.024	4.07	0.000	0.050	0.144
Jake No Record - Jake CQE	0.152	0.033	4.60	0.000	0.088	0.217
Seth No Record - Seth CQE	0.140	0.030	4.64	0.000	0.081	0.199
Tremayne No Record - Tremayne CQE	0.096	0.022	4.27	0.000	0.052	0.139
DaQuan Record - DaQuan CQE	0.022	0.017	1.29	0.198	-0.011	0.056
Jake Record - Jake CQE	0.037	0.028	1.31	0.191	-0.018	0.093
Seth Record - Seth CQE	0.033	0.026	1.30	0.193	-0.017	0.084
Tremayne Record - Tremayne CQE	0.022	0.017	1.29	0.198	-0.011	0.054
DaQuan No Record - DaQuan Record	0.075	0.023	3.23	0.001	0.030	0.121
Jake No Record - Jake Record	0.115	0.033	3.45	0.001	0.050	0.181
Seth No Record - Seth Record	0.106	0.030	3.49	0.000	0.047	0.166
Tremayne No Record -	0.074	0.022	3.35	0.001	0.031	0.117

iii. Interactions

Displayed below in Table 4.51 are the predicted probabilities derived from the interaction between the race and criminal record treatment variables (see Table B.19 for the full logistic regression output). The predicted probabilities were as follows: African American no record = .170, African American record = .090, African American CQE = .065, White no record = .270, White record = .155, White CQE = .120. In Table 4.52, average marginal effects showed that the difference between African Americans with no criminal record and African Americans with a criminal record and CQE was statistically significant ($p = .001$). The difference between African Americans with a criminal record and African Americans with a criminal record and CQE was not statistically significant ($p = .349$). The difference between African Americans with no record and African Americans with a criminal record was statistically significant ($p = .017$). The difference between Whites with no criminal record and Whites with a criminal record and CQE was statistically significant ($p <= .001$). The difference between Whites with a criminal record and Whites with a criminal record and CQE was not statistically significant ($p = .474$). The difference between Whites with a criminal record and Whites with no criminal record was statistically significant ($p = .005$).

Average marginal effects also showed that the difference between Whites and African Americans with no record was statistically significant ($p = .015$). The difference between Whites and African Americans with a criminal record was statistically significant ($p = .046$). The difference between White and African Americans with a criminal record and CQE was statistically significant ($p = .027$). These results indicate minimal differences between the main effects and interaction models, and both AIC and BIC favored the main effects model. Additional interactions were also explored (race x city location; criminal record x city location; criminal record x job type; race x job type; race x criminal record x city location); however, results again indicated minimal differences and AIC and BIC still favored the main effects model.[31]

Table 4.51. Probability of a callback for the criminal record and race interaction.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
African American	0.170	0.027	0.118	0.222
White	0.270	0.031	0.208	0.332
Record				
African American	0.090	0.020	0.050	0.130
White	0.155	0.026	0.105	0.205
Record and CQE				
African American	0.065	0.017	0.031	0.099
White	0.130	0.024	0.083	0.177

Table 4.52. Average marginal effects of the criminal record and race interaction.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am. No Record - African Am. CQE	0.105	0.032	3.30	0.001	0.043	0.167
African Am. Record - African Am. CQE	0.025	0.027	0.94	0.349	-0.027	0.077
African Am. No Record - African Am. Record	0.080	0.033	2.40	0.017	0.015	0.145
White No Record - White CQE	0.140	0.039	3.55	0.000	0.063	0.217
White Record - White CQE	0.025	0.035	0.72	0.474	-0.043	0.093
White No Record -						

[31] Race x Criminal Record x Job Type was also examined. However, this interaction caused significant instability in the model.

White Record	0.115	0.041	2.84	0.005	0.036	0.194
White No Record - African Am. No Record	0.100	0.041	2.43	0.015	0.019	0.181
White Record - African Am. Record	0.065	0.033	1.99	0.046	0.001	0.129
White CQE - African Am. CQE	0.065	0.029	2.20	0.027	0.007	0.123

Displayed below in Table 4.53 are the predicted probabilities derived from the interaction between the race and criminal record treatment variables with control variables included in the model (see Table B.20 for the full logistic regression output). The predicted probabilities were as follows: African American no record = .179, African American record = .092, African American CQE = .064, White no record = .264, White record = .158, White CQE = .129. In Table 4.54, average marginal effects showed that the difference between African Americans with no criminal record and African Americans with a criminal record and CQE was statistically significant ($p = .001$). The difference between African Americans with a criminal record and African Americans with a criminal record and CQE was not statistically significant ($p = .290$). The difference between African Americans with no record and African Americans with a criminal record was statistically significant ($p = .020$). The difference between Whites with no criminal record and Whites with a criminal record and CQE was statistically significant ($p <= .001$). The difference between Whites with a criminal record and Whites with a criminal record and CQE was not statistically significant ($p = .398$). The difference between Whites with a criminal record and Whites with no criminal record was statistically significant ($p = .008$).

Average marginal effects also showed that the difference between Whites and African Americans with no record was statistically significant (p = .019). The difference between Whites and African Americans with a criminal record was statistically significant (p = .043). The difference between White and African Americans with a criminal record and CQE was statistically significant (p = .024). Therefore, inclusion of the control variables did not substantively or significantly alter the above results.

Table 4.53. Probability of a callback for the criminal record and race interaction with controls.

Variable	Margin	SE	CI Lower	CI Upper
No Record				
African American	0.169	0.026	0.118	0.220
White	0.264	0.030	0.204	0.323
Record				
African American	0.092	0.021	0.052	0.132
White	0.158	0.025	0.109	0.208
Record and CQE				
African American	0.064	0.017	0.031	0.097
White	0.129	0.023	0.083	0.175

Table 4.54. Average marginal effects of the criminal record and race interaction with controls.

Variable – Base Outcome	Difference	SE	Z	P>|z|	CI Lower	CI Upper
African Am. No Record - African Am. CQE	0.105	0.031	3.41	0.001	0.045	0.166
African Am. Record - African Am. CQE	0.028	0.027	1.06	0.290	-0.024	0.080
African Am. No Record - African Am. Record	0.077	0.033	2.33	0.020	0.012	0.142
White No Record - White CQE	0.135	0.038	3.53	0.000	0.060	0.209
White Record - White CQE	0.029	0.035	0.85	0.398	-0.038	0.097

White No Record - White Record	0.105	0.040	2.65	0.008	0.028	0.183
White No Record - African Am. No Record	0.094	0.040	2.35	0.019	0.016	0.173
White Record - African Am. Record	0.066	0.033	2.02	0.043	0.002	0.130
White CQE - African Am. CQE	0.065	0.029	2.26	0.024	0.009	0.122

CHAPTER 5

DISCUSSION

5.1 DISCUSSION OF RESULTS AND PREVIOUS CQE RESEARCH

> H1: The probability of a callback for applicants with a CQE will be less than those with no record.

The above results support hypothesis 1. Applicants with a CQE received significantly fewer callbacks than those with no record (probabilities were as follows: Whites with no record in mixed design = 25.1%; African Americans with no record mixed design = 19.4%; Whites with a CQE mixed design = 17.4%; African Americans with a CQE mixed design = 13.1%; Whites with no record between-subjects design = 27.4%; African Americans with no record between-subjects design = 16.6%; Whites with CQE between-subjects design = 12.5%; African Americans with CQE between subjects design = 7%). This finding was present in African American and White applicants, both designs, and in most robustness checks.[32] Further, the mixed design also showed that

[32] The mixed design interaction model showed that the difference between African Americans with a CQE and African Americans with no record was not statistically significant. However, removing influential pairs from this model resulted in a marginally significant difference using a one-sided test. Further, inclusion of control variables also resulted in a marginally significant difference using a one-sided test and removal of influential pairs along with controls resulted in a fully significant difference using a one-sided test.

when both applicants received a callback, those with a CQE had a probability of being called first that was approximately 22 percentage points lower (marginally significant when controls were added to the model) than those with no record. Therefore, the results indicate that a CQE does not eliminate the stigma of the particular criminal record used in the current study.

The above results support Leasure and Andersen (2019) which found that callbacks for African Americans with a CQE were significantly lower than those with no record (African Americans with no record = 25.2%; African Americans with a CQE = 11%). Further, the use of multiple control variables, multiple designs, a different research location, a formal certificate, multiple names, and a different criminal record in the current study adds robustness to the results of Leasure and Andersen (2019).

However, the above result does not support the findings of Leasure and Andersen (2016) which found that callbacks for White applicants with a CQE were statistically indistinguishable from those with no record (Whites with no record = 29%; Whites with a CQE = 25.6%). Potential explanations for this difference could be the use of a formal certificate, a different research location, or a different criminal record in the current study. Further, it is possible that one or more of the control variables used here (i.e., staffing agency or day submitted) were driving the higher White CQE callback rates in Leasure and Andersen (2016).

> H2: The probability of a callback for applicants with a CQE will be greater than those with a record and no CQE.

The above results do not support hypothesis 2. Applicants with a record and CQE fared no better in terms of callbacks than applicants with a record and no CQE (probabilities were as follows: Whites with a record and no CQE = 15.6%; African

Americans with a record and no CQE = 8.9%; Whites with a CQE = 12.5%; African Americans with a CQE = 7%). In fact, point estimates for the CQE condition were lower than those with a record and no CQE in every comparison. This finding was supported in all robustness checks. Therefore, findings were not even in the expected direction stated in the above hypothesis.

This result supports Leasure and Andersen (2019) which found that callbacks for African Americans with a record (one-year-old drug felony) and CQE were statistically indistinguishable from those with a record and no CQE (African Americans with a CQE = 11%; African Americans with a record and no CQE = 8.4%). Further, the use of multiple control variables, a different research location, a formal certificate, multiple names, and a different criminal record in the current study adds robustness to the results of Leasure and Andersen (2019).

The above result does not support the findings of Leasure and Andersen (2016) which found that callbacks for White applicants with a record and no CQE were significantly better than those with a record and no CQE (Whites with a CQE = 25.6%; Whites with a record and no CQE = 9.8%). Potential explanations for this difference could be the use of a formal certificate, a different research location, or a different criminal record in the current study. Specifically, it is possible that attaching a formal CQE certificate to the resume drew more attention to a criminal record condition that would be overlooked if only present in a cover letter (see Lareau, 2014 finding that approximately 60% of surveyed employers do not read cover letters). Interestingly, this would explain why the point estimates for the CQE condition were lower than those with a record and no CQE in every comparison. Further, it is again possible that one or more of the control variables used here (i.e., staffing agency or day submitted) were driving the higher White CQE callback rates in Leasure and Andersen (2016). However, as the between-subjects design was the only one that could include the record and CQE versus record and no CQE comparison (because of the risk of detection as noted above), it is also possible that particular unobserved resume or employer characteristics were driving this result (see Heckman, 1998).

> H3: The probability of a callback for applicants with no record will be greater than those with a record.

The above results support hypothesis 3. Applicants with no record received a significantly larger number of callbacks than those with a record (probabilities were as follows: Whites with a record and no CQE = 15.6%; African Americans with a record and no CQE = 8.9%; Whites with no record = 27.4%; African Americans with no record = 16.6%). This finding was supported in all racial comparisons and in all robustness checks.

This finding is consistent with the bulk of literature in this area finding that a criminal record has a significant negative impact on hiring outcomes. For example, Pager (2003) found the following callback probabilities for testers presenting a felony drug conviction: White no record = 34%; White record = 17%; African American no record = 14%; African American record = 5%. Further, Agan and Starr (2017a) found that those with a criminal record (drug or property crime) had a callback rate of 8.5% and those without a record had a callback rate of 13.6%. As noted above, numerous other studies using various designs have found similar results. Because the current study used a unique criminal record formulation (multiple convictions for drug and theft crimes) and was conducted in a different geographic setting, the findings of previous research in this area are reinforced.

> H4: The probability of a callback for African American applicants will be less than White applicants in all criminal record conditions.

The above results support hypothesis 4. African American applicants received significantly fewer callbacks than White Applicants in all criminal record categories. This finding was supported in both designs and in most robustness checks. A few models in the mixed design portion of the study failed to find statistically significant racial effects. For example, the African American and White grouping models (with and without controls) failed to find statistically significant racial differences. Additionally, the individual name models (with and without controls) did not find statistically significant differences between Tremayne and Seth and Tremayne and Jake in both criminal record conditions. However, the probabilities derived from those models were in the direction predicted by the null hypothesis. Further, when both White and African American resumes received a callback, African Americans had a probability of being called first that was approximately 20 percentage points lower (statistically significant in the control model using a one-sided test) than Whites.

This finding is consistent with the plethora of research showing that African Americans were consistently less likely to advance in the hiring process than equally situated Whites (see Bendick et al., 1994; Bertrand & Mullainathan, 2004). For example, Zschirnt and Ruedin (2016) utilized meta-analysis of 42 separate correspondence studies from 1990 to 2015 to examine discrimination in hiring practices and found that discrimination against minorities was present across time, jurisdiction, gender, and economic contexts (see also the meta-analysis by Quillian et al., 2017). As noted above, Pager (2003) found that Whites with a criminal record received a larger percentage of callbacks than African Americans with no criminal record (17% versus 14%). Such a finding was nearly replicated in the current study (Whites with a record = 15.6% and African Americans with no record = 16.6%). Because the current study used a unique criminal record formulation (multiple convictions for drug and theft crimes) and was conducted in a different geographic setting, the findings of previous research in this area are reinforced.

5.2 POLICY AND THEORETICAL IMPLICATIONS

There are several policy and theoretical implications that can be derived from the above findings. The first set of implications is derived from the test of CQE effectiveness. The results here indicate that a CQE was not effective in terms of creating statistically equal chances of callbacks for ex-offenders (possessing the specific criminal record formulation used here) and those with no record when used for general employment purposes. In fact, applicants with a record and CQE fared no better in terms of callbacks than applicants with a record and no CQE. Given the judicial stamp of good character, employer immunity clause, and recent amendment strengthening the CQE, it is difficult to identify further amendments which could improve this mechanism. It is possible that certificates of relief, or any other mechanism that falls short of sealing criminal history, may not be effective at overcoming the negative stigma of the specific criminal record used in this study. As far as theoretical implications, the above results indicate that CQEs may not be effective signals of productivity (signaling theory) or effective risk reduction mechanisms (prospect theory) for ex-offenders who possess criminal history with more severe or repeat offenses.

Therefore, legislators may need to consider expanding eligibility requirements for collateral consequence relief mechanisms that eliminate consideration of one's criminal record in the hiring process. Such mechanisms definitionally eliminate the negative signals and perceived risk associated a criminal record. Expungement is one promising avenue for collateral consequence relief given recent studies which cited increased employment rates and average earning for those who have had their records expunged (see Selbin et al., 2016; Prescott & Starr, 2019). However, legislators may be reluctant to enact laws which broaden the types of ex-offenders who would be eligible for expungement (Love, 2011). Further, Prescott and Starr (2019) found that a large number of expungement eligible ex-offenders fail to apply for this mechanism.

In line with this fact, another possibility is to expand ban-the-box laws to entirely exclude criminal history questions in the hiring process. Such a provision could include reasonable time-clean requirements (requirements for an ex-offender to stay crime-free for a specified period of time) and exceptions for employers in certain industries (healthcare, childcare, etc.). For example, states could pass laws which ban consideration of criminal history that is older than 3 years. Further, placing the burden on employers to remove criminal record questions altogether would eliminate the need for ex-offenders to navigate a potentially complicated and expensive expungement process (Love, 2011). However, some ban-the-box research has found that removing criminal history questions results in increased racial discrimination (Agan & Starr 2017b; see also Sugie et al., 2017). It is argued that some employers essentially try to guess which employees possess a criminal record, which disparately impacts minorities (see Aigner and Cain's (1977) statistical discrimination theory).

A final option to reduce the negative impact of a criminal record could be to increase employer incentives for hiring ex-offenders. Specifically, programs which provide employers direct financial benefits for hiring ex-offenders may reduce the impact of a criminal record. Programs such as Federal Bonding do not provide direct financial benefits and instead provide insurance policies if ex-offender employees cause damage or liability (Ohio Department of Correction, 2017). The best-known direct incentive program is the Work Opportunity Tax Credit. This mechanism does so by offering employers a tax credit for hiring ex–felons who have a conviction or release from prison that is no more than one year old (U.S. Dept of Labor, 2017). The amount of the tax credit is determined by the number of hours worked by the disadvantaged employee during the first year of employment; however, the maximum credit is $2,400 per employee. Increasing the dollar amount of the credit and expanding eligibility requirements may be an effective mechanism to reduce the risk (prospect theory) associated with hiring an ex-offender. However, jurisdictions would need to take steps to inform employers about such a program as research has found that many employers are unaware of current incentive programs (see Martin et al., 2019).

The second set of implications is derived from the tests of racial differences in callbacks. The results here showed that African American applicants received significantly fewer callbacks than White Applicants in all criminal record categories. This finding is consistent with the plethora of research showing that African Americans were less likely to advance in the hiring process than equally situated Whites (see Zschirnt & Ruedin, 2016; Quillian, Pager, Hexel, & Midtboen, 2017).

While it is possible that some of the racial differences found in the current study were driven by overt racism, it is more likely the result of implicit bias. Implicit bias "is a mental process that stimulates negative attitudes about people who are not members of one's own 'in group'" (Kirwan Institute, 2012; see also the above discussed concept of statistical discrimination). This bias then can lead to discriminatory behavior in a variety of contexts (Alexander, 2012). For example, criminal justice research examining implicit bias has found that individuals are more likely to support punitive policies for minorities (see for example, Hetey & Eberhardt, 2014). Further, research has also demonstrated that bias results in racially disproportionate representation in various stages of the criminal justice system (see for example Demuth & Steffensmeier, 2004; Geller & Fagan, 2010; see also Travis et al., 2014). In the current context, numerous studies have linked negative views of African Americans to employers' unwillingness to hire such individuals (Neckerman & Kirschenman, 1991; Pager & Karafin, 2009; see also Anderson, 2012).

Therefore, any effective collateral consequence relief mechanism must account for racial discrimination and implicit bias. Doing so may require the use of several mechanisms in concert with one another. One possibility is to expand the targeted groups in the Work Opportunity Tax Credit to include minorities.

However, some authors have been skeptical about the benefit of anti-discrimination legislation and case law, pointing to previous attempts that simply shifted discrimination from one structure to another (Bell, 1992). The validity of such an argument is highlighted in the current context by the research noted above finding that minorities with no criminal record are negatively impacted by BTB laws (Agan & Starr

2017b; see also Sugie, 2017). Authors of those studies argued that employers who no longer have access to criminal history information post BTB essentially try to guess which employees have criminal records, and that this guessing process disproportionately impacts minorities.

Guided by these points, a few additional policy recommendations can be offered. First, one possibility would be to encourage (through tax or other financial incentives) more private affirmative action policies and practices (see United Steelworkers of America v. Weber, 443 U.S. 193,1979 allowing the use of affirmative action in private employment). Relatedly, another possibility would be to mandate more robust public affirmative action policies and practices. Interestingly, one study examined whether Executive Order 11246 (an order requiring companies with federal contracts to be compliant with affirmative action/fair hiring policies) increased the amount of African American employees. That study found that the amount of African American employees in affected companies grew by an average of 0.8 percentage points after a five-year period of being bound by the executive order.

Second, Bell (1992) also noted that any legitimate approach to dealing with racial discrimination must recognize the current disadvantaged economic position of African Americans in the U.S. One direct method to address this point would be reparations. Reparation legislation would seek to compensate African Americans for various individual and community losses suffered as a result of slavery, segregation, and contemporary racism (Feagin, 2004). Such reparations could then lead to further positive socioeconomic opportunities and thus upward mobility. Interestingly, the U.S. has supported enforcement of Nazi victim reparations and has paid reparations to Japanese and Native Americans (Feagin, 2004).

Third, Bell (1992) and others argued that minority defendants are limited in their ability challenge discriminatory aspects of various laws and policies (see also Alexander, 2012). For example, current law imposes a large burden on minority defendants seeking to prove discriminatory intent (Alexander, 2012). Relaxing these and other standing and immunity rules could provide minorities with an effective mechanism to challenge policies and practices as discriminatory (see Alexander; 2012).

5.3 LIMITATIONS AND DIRECTIONS FOR FUTURE RESEARCH

The findings in the current study should be considered in light of the following limitations. First, this study did not examine the effectiveness of certificates of relief for women. While some research has found that that criminal history does not affect women as negatively as it does men, the topic needs further study (Galgano, 2009; Ortiz, 2014). Future research should continue to explore the impact of a criminal record for women. If future research should find a significant impact of a criminal record for women's employment opportunities, then researchers should explore the effectiveness of certificates of relief for reducing that impact.

Second, this study only focused on White and African American applicants. The results here may not generalize to other racial or ethnic groups. This is an important limitation as research has demonstrated that Hispanic individuals with criminal records also face barriers in securing employment (Pager, Western, & Sugie, 2009). Future research should also explore the effectiveness of certificates of relief for ex-offender Hispanics and other racial/ethnic groups seeking employment as race/ethnicity of these other groups may differentially impact callback rates for those with criminal records or CQEs.

Relatedly, only two racially distinct names were used as measures of race. This is an important limitation as some of the above results demonstrate statistically significant differences in callbacks between individual names used as a measure of one race (see also Bertrand & Mullainathan, 2004). For example, in the mixed design portion of the study, the differences in callbacks between Tremayne and Daquan were marginally significant for both criminal record categories (with and without controls).[33]

Third, this study used only one criminal record formation (drug and theft) and did not note the type of drug. While the criminal record used here was built to best address generalizability, including other crime types or specifying particular types of drugs could produce different results (see Leasure and Andersen, 2016, 2019). This is an important limitation as survey research has shown that an employer's willingness to hire ex-offenders depends largely on the seriousness (type) of the crime (see Albright & Denq, 1996; Kuhn, 2019). Future research should explore the effectiveness of certificates of relief on reducing the impact of various crime types (e.g., assault, D.U.I., fraud).

Fourth, the ages of the crimes used in this study were not manipulated. This is an important limitation giving research has found that employers are more willing to hire ex-offenders with older criminal histories (see Albright & Denq, 1996; Kuhn, 2019; Leasure & Andersen, 2017). Future research should explore the effectiveness of certificates of relief on reducing the impact of various criminal history ages.

[33] Removal of the influential pairs caused these differences to lose marginal significance.

Fifth, this study only utilized job postings that required submission of a resume. A search of entry-level positions posted within the last 30 days (as of 9-22-19) on Careerbuilder.com showed that 265 out of 728 postings required only a resume. While such an approach allowed for more control over the experimental conditions (see Agan & Starr, 2017a, 2017b utilizing formal applications and noting that research assistants were to rely on their judgement for answering unanticipated questions), the above results may not generalize to postings that require formal applications. Future research should explore the effectiveness of certificates of relief for ex-offenders using formal applications to apply for a position.

Sixth, the findings here may be specific to Cleveland, Ohio. This is an important limitation given the lessons learned from experiments on mandatory domestic violence arrests. Early studies in this area influenced policy, but were unsuccessfully replicated in other locations (e.g., Berk, Campbell, Klap, & Western, 1992). This study must be replicated in other geographic areas before drawing concrete conclusions (see Sherman & Strang, 2004).

Seventh, the current study did not vary the order of the education and work history on resumes. This is an important limitation as ordering of these factors could impact results. For example, it is possible that those with a criminal history would fare better in early employment outcomes if they could present work history and educational attainment that occurred after a conviction. Conversely, if a conviction is the most recent occurrence, offenders may face more reluctance from employers. Future research should manipulate the temporal ordering of the work and education history with criminal record treatment conditions.

Eight, several methods could be used to convey that an applicant possesses a CQE and only a few combinations were used in this study. The current study attached actual certificates to resumes, placed hypothetical applicants' names on the official CQE list, and provided a note about the CQE in a cover letter. A note was included in the cover letter to strengthen the signal of the CQE and briefly explain the purpose of a CQE. A brief explanation was important to include as previous research showed that employers might not be aware of certificates or their exact purpose (Ewald, 2016; Garretson, 2016; Sahl, 2016). The brief explanation used in this study was meant to convey the main benefits of the CQE; the determination from a court that the individual is not a safety risk, the presumption that the person's criminal record is insufficient evidence to disqualify an individual for an employment opportunity, and the negligent hiring immunity. While the brief note communicated these facts, several other versions could be conveyed which could influence employer response. Future research should construct differing versions of certificate explanations and examine differences in employer response.

Finally, the current study only explored the effectiveness of a CQE for improving general employment opportunities for ex-offenders. This study did not examine whether a CQE is beneficial for those seeking licensures or in obtaining positions that require licensures. It is possible that those seeking such positions may fare better in hiring outcomes. Future research should explore the effectiveness of these certificates for improving chances of licensure and subsequent employment in a license-required field.

5.4 TECHNICAL NOTES ON CORRESPONDENCE STUDIES

As a result of the above data collection project, there are several technical notes regarding correspondence designs that are worthy of discussion. First, each design showed statistically significant differences between CQE holders and those with no record. However, the difference in callback probabilities for CQE holders in these two designs are worthy of further discussion. The probability of a callback for CQE holders in the mixed design was .131 for African Americans and .174 for Whites. The probability of a callback for CQE holders in the between-subjects design was .070 for African Americans and .125 for Whites. The difference between the African American probabilities was statistically significant, $p = .04$ (using a 2-sample z-test). The difference between the White probabilities was not statistically significant, $p = .17$.

Regardless of statistical significance, both differences raise questions about the impact of the designs used in this study. One explanation is that the mixed design suffered from carryover/spillover effects. As noted above, previous research has detected the presence of such effects in experimental discrimination research examining callback rates (see Phillips, 2016). It is possible that an employer in the between-subjects design who received one resume noting a criminal conviction and CQE would be more inclined to wait and see if the candidate pool quality increased before deciding on callbacks. However, an employer in the mixed design who received both resumes (criminal record with CQE and no record) may have been influenced to conduct more interviews given the growing pool of lower quality resumes (i.e., criminal record on one resume and employment gaps, economically depressed neighborhoods, and GEDs rather than diplomas on both resumes). In essence, an employer in the mixed design may feel that they need to conduct several interviews to find a good candidate given the number of lower quality resumes.

Second, a limitation of correspondence designs utilizing electronic submission not readily identifiable in the literature is that one cannot guarantee that the correspondence was received. For example, email malfunctions (sender or receiver) and online job site malfunctions are certainly possible. This is a limitation that future researchers should note and potentially address. Researchers could include procedures in their IRB protocol that allow them to periodically confirm whether a correspondence was received. Relatedly, researchers should perform technology checks on the equipment used to receive employer callbacks. Periodic checks such as calling study phones and sending emails to study email addresses would help reduce concern in this area.

Third, researchers conducting online correspondence designs should consider how to address the growing use of online job skill assessments which are required to complete an application (even those requiring only a resume). For example, indeed.com, the largest online job website in the U.S., recently introduced several different assessments which employers can require for complete resume submission (indeed blog, 2018). These assessments include multiple job specific skill tests, problem solving tests, critical thinking tests, computer skill tests, and language tests (indeed assessments, 2019a). Further, these assessments can require an applicant to answer pre-recorded questions via phone (indeed assessments, 2019b). Therefore, researches will either need to exclude applications with such assessments from their population or they will need to develop a strategy to deal with each potential assessment. In the current study, applications requiring assessments were excluded because most applications did not require an assessment. However, this option may not be feasible in the future if assessments become a normal application practice.

Fourth, and related to the third, researchers need to be weary of automatic responses from job websites that could be misconstrued as a callback. Through the course of this research, it became apparent that several applications submitted via indeed.com would send an email congratulating the applicant on making it to the next step in the hiring process and inviting them to complete a further step. However, these emails were automatically sent by the job website and were not indicative of an employer's actual decision-making process (deduced from the fact that the congratulations email arrived seconds after resume submission). Therefore, researchers need to carefully examine callbacks to ensure that they in fact represent an employer's actual decision-making process.

Fifth, researchers conducting correspondence designs which use callbacks as a dependent variable need to enable several avenues for an employer to contact hypothetical applicants. This is an important point as recent correspondence studies utilized only voicemail and email accounts (see Agan & Starr, 2017a, 2017b). However, in the current study, callbacks were received via voicemail, text messages, emails, and various combinations thereof. Therefore, future researchers utilizing a correspondence study should use all three of these contact avenues to ensure that the callback variable is less susceptible to measurement error.

Finally, researchers and institutional review boards should begin to question the ethics of audit and correspondence designs conveying criminal history which do not involve contact with the employer post callback or submission. The results of these designs have been very useful in terms of detailing the level of stigma attached to a criminal record. However, it is possible that such designs add to this stigma in certain circumstances. For example, it is possible that an employer who invited one with a criminal record for an interview will be dissuaded from doing so in the future if the invitation was not addressed. In the aggregate, these individual instances could increase the level of stigma attached to a criminal record in the employment context. Such an issue could potentially be avoided by including an IRB procedure which allows the researcher to contact the employer and explain that they have recently accepted another offer.

CHAPTER 6

CONCLUSION

The number of those with some sort of a criminal record stands at approximately 85 million (Bureau of Justice Statistics, 2015). Such numbers are of crucial importance when considering the impact of so-called collateral consequences of conviction. Though there are many collateral consequences (barriers to housing, loss of public benefits, loss of civil rights), ex-offenders and other related parties such as probation and parole officers consistently cite that one of the most punitive collateral consequences is the barrier to employment created by incarceration and the application of a criminal record (Bahr et al., 2010; Garland et al., 2010; O'Brien, 2011; Ray et al., 2016; Western et al., 2015). These studies demonstrate that ex-offenders have issues seeking employment (i.e., lack of ability to travel), being hired, and being promoted to better paying jobs (Bahr et al., 2010; Garland et al., 2010; O'Brien, 2011; Pager, 2003; Ray et al., 2016; Western et al., 2015).

Such findings are of crucial importance because employment is a key factor for desistance from criminal behavior (Baron, 2008; Laub & Sampson, 2003; Tripodi et al., 2010; Verbruggen et al, 2012; Wang et al, 2010; Wright & Cullen, 2004; and see Lageson & Uggen, 2013; Uggen & Wakefield, 2008 for thorough reviews). Recognizing the barriers created from collateral consequences, all jurisdictions have created collateral consequence relief mechanisms meant to provide some sort of relief (Collateral Consequence Resource Center, 2017). One of the newest mechanisms, specifically created to combat collateral consequences related to employment, is the certificate of relief (sometimes called certificate of recovery or certificate of qualification for employment) (Love, 2011; Love & Frazier, 2006; O.R.C. 2953.25). Certificates of relief are intended to aid ex-offenders in their job search by demonstrating rehabilitation to decision-makers, lifting occupational licensing restrictions, and sometimes providing tort immunities to employers (Love, 2006; Love & Frazier, 2006; O.R.C. 2953.25). Previous research largely focused on access to and perceived effectiveness of certificates of relief. Two studies did examine the actual effectiveness of these certificates with a field experiment; however, they produced mixed results (see Leasure & Andersen, 2016, 2019).

The current study built upon previous research by testing an amended version of Ohio's certificate of relief, using a criminal record condition that contained multiple convictions of varying crime types (drug and theft), including multiple control variables and robustness checks, using official certificates, and by using a study location that comprises the largest number of Ohio ex-offenders.

The current study produced several important findings. First, results indicated that applicants with a CQE received significantly fewer callbacks than those with no record. Second, the results also showed that when both applicants received a callback, those with a CQE had a probability of being called first that was approximately 22 percentage points lower (marginally significant when controls were added to the model) than those with no record. Third, applicants with a record and CQE fared no better in terms of callbacks than applicants with a record and no CQE. Fourth, applicants with no record received a significantly larger number of callbacks than those with a record. Finally, African American applicants received significantly fewer callbacks than White Applicants in all criminal record categories and when both White and African American resumes received a callback, African Americans had a probability of being called first that was approximately 20 percentage points lower (significant using a one-sided test) than Whites.

The findings that show the negative impact of a criminal record and minority status on hiring outcomes are consistent with a long line of research. It was this long line of research that prompted the specific analyses in this study. The results of Leasure and Andersen (2016) provided promising evidence that the certificate of relief may be effective at reducing criminal record stigma. However, the results of that analysis were not confirmed in this study. Such a result emphasizes the importance of replication in experimental research. Despite the findings here, the above limitations show that further research is needed to better understand the effectiveness of certificates of relief. Given the substantial negative impacts of a criminal record in the hiring process, such research is needed in order to identify an effective collateral consequence relief mechanism.

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APPENDIX A

LOGISTIC REGRESSION DIAGNOSTICS

Several diagnostics were performed for the logistic regressions. These diagnostics were largely guided by a resource distributed by the UCLA Institute for Digital Research and Education (UCLA Institute for Digital Research and Education, 2019a). The first diagnostic performed was a check for influential observations.

A. Influential Observations

As noted in the Stata manual, “DFBETAs are perhaps the most direct influence measure of interest to model builders. DFBETAs focus on one coefficient and measure the difference between the regression coefficient when the specified observation is included and excluded, the difference being scaled by the estimated standard error of the coefficient” (StataCorp, 2019: 15; see also Williams, 2016). Guided by previous recommendations, values greater than 1 were further examined for potential influence (see Bollen & Jackman, 1985; Schutte & Violette, 1994; Williams, 2016; see also Belsley, Kuh, and Welsch, 1980 recommending further investigation of values greater than 2). This was meant to identify observations that shifted the regression coefficient estimate at least one standard error. Stata’s dbeta and lfdbeta commands were used to identify DFBETAs with values greater than 1.

i. Mixed Design

For the mixed portion of the study, this procedure identified two potential influential observation pairs. Probabilities and average marginal effects with these pairs deleted (with and without controls) were compared to the results above and there were no changes in statistical or substantive significance.[34] While overall results of other models in the mixed portion of the study were unaffected by removal of the influential pairs, a few points are worthy of further discussion. For the interaction model without controls, removal of the influential observations caused the African American CQE and African American no record comparison to become marginally statistically significant (p = .189*). For the interaction model with controls added, removal of the influential observations caused the African American CQE and African American no record comparison to become fully statistically significant (p = .098*). Further, the difference between Whites with no record and African Americans with no record was now only statistically significant (p = .052*) in the interaction control model.

For the individual name models without controls, removal of the influential pairs caused the differences (both criminal record categories) between Tremayne and DaQuan to lose marginal significance (p = .106 for no record; p = .109 for CQE). For the individual name models with controls, removal of the influential pairs again caused the differences (both criminal record categories) between Tremayne and DaQuan to lose marginal significance (p = .169 for no record; p = .164 for CQE).

[34] Models excluding each individual pair were also examined. R^2, AIC, and BIC favored the model excluding both pairs.

For the first call model without controls, removal of the influential pairs caused the racial differences in both criminal record categories to become marginally significant (p = .110* for no record; p = .108* for CQE).

ii. Between-Subjects Design

There were no influential observations in the between-subjects portion of the study with a DFBETA value over 1.

B. Multicollinearity

Multicollinearity was checked using Stata's coldiag2 and collin commands which present multiple collinearity diagnostics.

i. Mixed Design

The results of the coldiag2 command showed that the highest condition index value was 21.30 (next highest value was 11.87 and all others under 10). Gujarati (2002) notes that condition indexes with values equal to or less than 10 provide weak evidence for a collinearity issue (see also Belsley, Kuh, and Welsch, 1980). Condition index values that are greater than 10 and less than 30 provide moderate evidence of a collinearity issue. However, a high condition index value must also have two or more associated variance-decomposition proportions with a value greater than .5 (see Callaghan & Chen, 2008). Here, for the condition index value of 21.30, the constant had a value of .99; however, no other proportions had a value greater than or equal to .5. For the condition index value of 11.87, only one proportion had a value above .5 (.56). Therefore, these results indicate that multicollinearity was not an issue. Another multicollinearity diagnostic is the variance inflation factor (VIF) (see UCLA Institute for Digital Research and Education, 2019a noting that values above 10 are a potential cause for concern).

Using Stata's collin command, results indicated that no variance inflation factor had a value greater than 4.84, mean VIF 1.92).

ii. Between-Subjects Design

The results of the coldiag2 command showed that the highest condition index value was 20.09 (the next highest value was 9.23). For the variance-decomposition proportions, the constant had a value of .96 and three other variance proportions had a value greater than or equal to .5 (.59, .66, .57). However, using Stata's collin command, results indicated that no variance inflation factor had a value greater than 5.46, mean VIF 1.87). Further, Allison (2012) notes that [multicollinearity] is:

> only a problem for the variables that are collinear. It increases the standard errors of their coefficients, and it may make those coefficients unstable in several ways. But so long as the collinear variables are only used as control variables, and they are not collinear with your variables of interest, there's no problem. The coefficients of the variables of interest are not affected, and the performance of the control variables as controls is not impaired.

In both of the above designs, none of the potential issues of collinearity are present in the key independent variables (racial and criminal record conditions). Allison (2012) also notes that multicollinearity can safely be ignored when the issue emanates from "indicator (dummy) variables that represent a categorical variable with three or more categories. Such was the case with the above high value condition index scores and variance-decomposition proportions.

C. Specification Error and Model Fit

Model fit was determined with the use of two pseudo R^2 measures (McFadden and Cragg-Uhler/Nagelkerke) and both AIC and BIC statistics. These measures are noted beneath each logistic regression. Favored BIC values were identified using Raftery (1995) and favored AIC values were identified using Hilbe (2009). McFadden values were guided by McFadden (1979). Cragg-Uhler/Nagelkerke values were guided by UCLA Institute for Digital Research and Education (2019b). Stata's linktest command was used to check for specification error in the models that included control variables. The UCLA Institute for Digital Research and Education (2019a) stated as follows regarding the linktest:

> The idea behind linktest is that if the model is properly specified, one should not be able to find any additional predictors that are statistically significant except by chance. After the regression command (in our case, logit or logistic), linktest uses the predicted value (_hat) and predicted value squared (_hatsq) as the predictors to rebuild the model. The variable _hat should be a statistically significant predictor, since it is the predicted value from the model. This will be the case unless the model is completely misspecified. On the other hand, if our model is properly specified, variable _hatsq shouldn't have much predictive power except by chance. Therefore, if _hatsq is significant, then the linktest is significant. This usually means that either we have omitted relevant variable(s) or our link function is not correctly specified.

i. Mixed Design

The linktest for the main effect model with controls produced a statistically significant _hat value ($p = 0.000$) and a non-significant _hatsq value ($p = 0.629$). These results suggest that this model was correctly specified. The linktest for the individual name model (examining differences for each name used as a measure of race) with controls produced a statistically significant _hat value ($p = 0.000$) and a non-significant _hatsq value ($p = 0.564$). These results suggest that this model was correctly specified. The linktest for the interaction effect model with controls produced a statistically significant _hat value ($p = 0.000$) and a non-significant _hatsq value ($p = 0.597$). These results suggest that this model was correctly specified. The linktest for the first call model with controls produced a statistically significant _hat value ($p = 0.002$) and a non-significant _hatsq value ($p = 0.715$). These results suggest that this model was correctly specified. The linktest for the African American grouping model with controls produced a statistically significant _hat value ($p = 0.000$) and a non-significant _hatsq value ($p = 0.931$). These results suggest that this model was correctly specified. The linktest for the White grouping model with controls produced a statistically significant _hat value ($p = 0.001$) and a non-significant _hatsq value ($p = 0.200$). These results suggest that this model was correctly specified. The linktest for the African American and White grouping model with controls produced a statistically significant _hat value ($p = 0.003$) and a non-significant _hatsq value ($p = 0.326$). These results suggest that this model was correctly specified.

ii. Between-Subjects Design

The linktest for the main effect model with controls produced a statistically significant _hat value ($p = 0.015$) and a non-significant _hatsq value ($p = 0.581$). These results suggest that this model was correctly specified. The linktest for the individual name model with controls produced a statistically significant _hat value ($p = 0.011$) and a non-significant _hatsq value ($p = 0.638$). These results suggest that this model was correctly specified. The linktest for the interaction model with controls produced a statistically significant _hat value ($p = 0.017$) and a non-significant _hatsq value ($p = 0.561$). These results suggest that this model was correctly specified.

APPENDIX B

LOGISTIC REGRESSION MODELS PRESENTING ODDS RATIOS

A. Mixed Design

i. Main Model

Table B.1. Logistic regression of the criminal record and racial conditions on an applicant's likelihood of a callback.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Race						
White	1.394	0.257	1.81	0.071	0.972	2.000
Record Type						
CQE	0.628	0.070	-4.17	0.000	0.504	0.781
Constant	0.240	0.039	-8.89	0.000	0.176	0.329

Note. N = 800. McFadden R^2 = 0.013. Cragg-Uhler/Nagelkerke R^2 = 0.020. AIC = 768.343. BIC = 782.397. Log pseudolikelihood = -381.172. Wald chi2 (2) = 20.60. Prob > chi2 = 0.0000. Standard error adjusted for 400 clusters. Reference category for record and CQE is no record. Reference category for White is African American.

ii. Controls

Table B.2. Logistic regression of the criminal record and racial conditions on an applicant's likelihood of a callback with controls.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Race						
White	1.430	0.282	1.82	0.069	0.972	2.103
Record Type						
Record and CQE	0.584	0.075	-4.18	0.000	0.454	0.752
City Location						
Cleveland	1.049	0.328	0.15	0.879	0.568	1.938
Not Listed	0.83	0.338	-0.46	0.647	0.374	1.843
Base Resume						
2	0.964	0.124	-0.28	0.777	0.750	1.240

Submit First						
Yes	1.013	0.130	0.10	0.919	0.787	1.304
Full-Time						
Yes	1.148	0.415	0.38	0.702	0.566	2.330
Not Listed	1.205	0.809	0.28	0.782	0.323	4.490
Job Type						
Cust. Serv. Call Center	0.741	0.432	-0.51	0.607	0.236	2.322
Cust. Serv. In-Store	0.508	0.215	-1.6	0.109	0.222	1.163
Manufacturing	0.404	0.237	-1.55	0.122	0.128	1.273
Restaurant Labor	0.459	0.276	-1.30	0.195	0.141	1.491
Restaurant Cust. Serv.	0.509	0.366	-0.94	0.348	0.124	2.088
Driving	0.559	0.328	-0.99	0.322	0.177	1.768
Clerical	0.195	0.127	-2.51	0.012	0.054	0.698
Sales In-Store	0.706	0.418	-0.59	0.556	0.221	2.251
Warehouse	0.556	0.261	-1.25	0.211	0.222	1.395
Sales Call Center	3.907	2.584	2.06	0.039	1.069	14.281
Multiple	0.305	0.295	-1.23	0.219	0.046	2.029
Staffing Agency						
Yes	13.007	6.377	5.23	0.000	4.976	34.002
Temporary						
Yes	1.812	0.817	1.32	0.187	0.749	4.387
Month Submitted						
January	0.758	0.357	-0.59	0.556	0.301	1.907
March	0.687	0.223	-1.16	0.247	0.364	1.297
April	0.445	0.211	-1.71	0.087	0.176	1.126
May	1.353	0.572	0.72	0.474	0.591	3.099
Time Submitted						
PM	0.878	0.234	-0.49	0.625	0.521	1.480
Posting Age						
5-14 days	1.235	0.416	0.63	0.530	0.639	2.388
Day Submitted						
Friday and Saturday	0.293	0.181	-1.99	0.047	0.087	0.982
Hourly Pay						
Above Median	1.010	0.395	0.03	0.979	0.470	2.172
Not Listed	1.287	0.445	0.73	0.465	0.654	2.536
Constant	0.309	0.161	-2.25	0.025	0.111	0.860

Notes. N = 800. McFadden $R^2 = 0.135$. Cragg-Uhler/Nagelkerke $R^2 = 0.198$. AIC = 729.618. BIC = 874.841. Log pseudolikelihood = -333.809. Wald chi2 (30) = 90.92. Prob > chi2 = 0.0000. Standard error adjusted for 400 clusters. Reference Category for record type is no record. Reference category for Race is African American. Reference category for city location is outside Cleveland City. Reference category for base resume is 1. Reference category for submitted first is no. Reference category for full-time is part-time.

Reference category for job type is general labor. Reference category for staffing agency is no. Reference category for temporary is no. Reference category for month submitted is February. Reference category for time submitted is AM. Reference category for posting age is 1-4 days. Reference category for day submitted is Sunday through Thursday. Reference category for hourly pay is median and below.

iii. Individual Names

Table B.3. Logistic regression of the criminal record and individual name conditions on an applicant's likelihood of a callback.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Name						
Jake	1.646	0.372	2.20	0.028	1.057	2.564
Seth	1.788	0.476	2.18	0.029	1.061	3.011
Tremayne	1.488	0.334	1.77	0.077	0.958	2.312
Record Type						
CQE	0.627	0.070	-4.17	0.000	0.504	0.781
Constant	0.196	0.040	-7.94	0.000	0.131	0.293

Note. N = 800. McFadden $R^2 = 0.016$. Cragg-Uhler/Nagelkerke $R^2 = 0.024$. AIC = 770.112. BIC = 793.535. Log pseudolikelihood = -380.056. Wald chi2(4) = 23.34. Prob > chi2 = 0.0001. Standard error adjusted for 400 clusters. Reference Category for CQE is no record. Reference category for Name is DaQuan.

Table B.4. Logistic regression of the criminal record and individual name conditions on an applicant's likelihood of a callback with controls.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Name						
Jake	1.801	0.435	2.43	0.015	1.121	2.892
Seth	1.732	0.501	1.90	0.058	0.982	3.054
Tremayne	1.501	0.370	1.65	0.100	0.926	2.433
Record Type						
Record and CQE	0.571	0.075	-4.24	0.000	0.440	0.739
City Location						
Cleveland	1.052	0.330	0.16	0.872	0.568	1.946
Not Listed	0.830	0.337	-0.46	0.646	0.375	1.839
Base Resume						
2	0.956	0.123	-0.35	0.728	0.743	1.231
Submit First						
Yes	1.014	0.130	0.11	0.916	0.788	1.303
Full-Time						

Yes	1.157	0.422	0.40	0.690	0.566	2.363
Not Listed	1.213	0.812	0.29	0.773	0.327	4.502
Job Type						
Cust. Serv. Call Center	0.736	0.428	-0.53	0.598	0.236	2.300
Cust. Serv. In-Store	0.507	0.215	-1.60	0.110	0.221	1.166
Manufacturing	0.410	0.237	-1.54	0.123	0.132	1.273
Restaurant Labor	0.462	0.280	-1.27	0.203	0.140	1.519
Restaurant Cust. Serv.	0.496	0.358	-0.97	0.331	0.121	2.037
Driving	0.541	0.320	-1.04	0.300	0.169	1.727
Clerical	0.193	0.126	-2.53	0.012	0.054	0.692
Sales In-Store	0.698	0.415	-0.6	0.546	0.218	2.241
Warehouse	0.550	0.259	-1.27	0.204	0.218	1.385
Sales Call Center	3.788	2.502	2.02	0.044	1.038	13.823
Multiple	0.312	0.299	-1.21	0.225	0.047	2.048
Staffing Agency						
Yes	12.662	6.144	5.23	0.000	4.892	32.774
Temporary						
Yes	1.819	0.826	1.32	0.188	0.747	4.427
Month Submitted						
January	0.775	0.365	-0.54	0.589	0.308	1.950
March	0.676	0.220	-1.20	0.230	0.357	1.280
April	0.430	0.206	-1.76	0.079	0.168	1.102
May	1.356	0.573	0.72	0.471	0.592	3.102
Time Submitted						
PM	0.877	0.235	-0.49	0.625	0.519	1.482
Posting Age						
5-14 days	1.232	0.417	0.62	0.538	0.635	2.390
Day Submitted						
Friday and Saturday	0.301	0.185	-1.95	0.051	0.090	1.004
Hourly Pay						
Above Median	1.008	0.393	0.02	0.984	0.469	2.163
Not Listed	1.280	0.445	0.71	0.478	0.648	2.530
Constant	0.256	0.138	-2.53	0.011	0.089	0.736

Notes. N = 800. McFadden R^2 = 0.138. Cragg-Uhler/Nagelkerke R^2 = 0.201. AIC = 731.749. BIC = 886.341. Log pseudolikelihood = -332.875. Wald chi2 (32) = 89.76. Prob > chi2 = 0.0000. Standard error adjusted for 400 clusters. Reference Category for record type is no record. Reference category for name is DaQuan. Reference category for city location is outside Cleveland City. Reference category for base resume is 1. Reference category for submitted first is no. Reference category for full-time is part-time. Reference category for job type is general labor. Reference category for staffing agency is no. Reference category for temporary is no. Reference category for month submitted is

February. Reference category for time submitted is AM. Reference category for posting age is 1-4 days. Reference category for day submitted is Sunday through Thursday. Reference category for hourly pay is median and below.

iv. Interactions

Table B.5. Logistic regression of the criminal record and race interaction on an applicant's likelihood of a callback.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Race						
White	1.642	0.401	2.03	0.042	1.017	2.651
Record Type						
CQE	0.773	0.172	-1.16	0.248	0.499	1.197
Interaction Term						
White x CQE	0.684	0.252	-1.03	0.302	0.332	1.408
Constant	0.220	0.040	-8.23	0.000	0.153	0.315

Note. N = 800. McFadden R^2 = 0.014. Cragg-Uhler/Nagelkerke R^2 = 0.022. AIC = 769.289. BIC = 788.027. Log pseudolikelihood = -380.645. Wald chi2(3) = 23.16. Prob > chi2 = 0.0000. Standard error adjusted for 400 clusters. Reference category for record and CQE is no record. Reference category for White is African American. Reference category for White x CQE is African American with no record.

Table B.6. Logistic regression of the criminal record and race interaction on an applicant's likelihood of a callback with controls.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Race						
White	1.684	0.447	1.96	0.050	1.001	2.834
Record Type						
CQE	0.714	0.174	-1.38	0.166	0.443	1.150
Interaction Term						
White x CQE	0.692	0.283	-0.90	0.367	0.311	1.540
City Location						
Cleveland	1.045	0.327	0.14	0.888	0.566	1.931
Not Listed	0.828	0.335	-0.47	0.642	0.375	1.830
Base Resume						
2	0.976	0.125	-0.19	0.848	0.759	1.255
Submit First						
Yes	1.017	0.131	0.13	0.898	0.789	1.309
Full-Time						
Yes	1.157	0.419	0.40	0.687	0.569	2.352
Not Listed	1.226	0.827	0.30	0.762	0.327	4.597

Job Type						
Cust. Serv. Call Center	0.723	0.421	-0.56	0.577	0.231	2.264
Cust. Serv. In-Store	0.514	0.217	-1.58	0.115	0.224	1.176
Manufacturing	0.411	0.239	-1.53	0.126	0.131	1.283
Restaurant Labor	0.456	0.279	-1.28	0.200	0.138	1.514
Restaurant Cust. Serv.	0.522	0.379	-0.90	0.370	0.126	2.164
Driving	0.566	0.334	-0.96	0.335	0.178	1.799
Clerical	0.193	0.126	-2.52	0.012	0.054	0.694
Sales In-Store	0.730	0.427	-0.54	0.591	0.232	2.295
Warehouse	0.553	0.260	-1.26	0.208	0.219	1.392
Sales Call Center	3.797	2.538	2.00	0.046	1.024	14.072
Multiple	0.312	0.303	-1.20	0.230	0.046	2.093
Staffing Agency						
Yes	13.232	6.432	5.31	0.000	5.104	34.308
Temporary						
Yes	1.764	0.801	1.25	0.211	0.725	4.296
Month Submitted						
January	0.758	0.358	-0.59	0.558	0.300	1.914
March	0.676	0.221	-1.20	0.231	0.357	1.283
April	0.445	0.211	-1.71	0.088	0.176	1.128
May	1.346	0.571	0.70	0.483	0.586	3.090
Time Submitted						
PM	0.882	0.236	-0.47	0.639	0.523	1.489
Posting Age						
5-14 days	1.256	0.424	0.67	0.500	0.648	2.434
Day Submitted						
Friday and Saturday	0.301	0.187	-1.93	0.053	0.089	1.018
Hourly Pay						
Above Median	1.031	0.404	0.08	0.938	0.478	2.223
Not Listed	1.312	0.461	0.77	0.439	0.659	2.612
Constant	0.274	0.147	-2.42	0.016	0.096	0.783

Notes. N = 800. McFadden R^2 = 0.136. Cragg-Uhler/Nagelkerke R^2 = 0.199. AIC = 730.815. BIC = 880.723. Log pseudolikelihood = -333.408. Wald chi2 (31) = 92.59. Prob > chi2 = 0.0000. Standard error adjusted for 400 clusters. Reference Category for record type is no record. Reference category for race is African American. Reference category for interaction term is African American x no record. Reference category for city location is outside Cleveland City. Reference category for base resume is 1. Reference category for submitted first is no. Reference category for full-time is part-time. Reference category for job type is general labor. Reference category for staffing agency is no. Reference category for temporary is no. Reference category for month submitted is February. Reference category for time submitted is AM. Reference category for posting age is 1-4 days. Reference category for day submitted is Sunday through Thursday. Reference category for hourly pay is median and below.

v. Racial Groupings

Table B.7. Logistic regression of the criminal record and racial conditions on an applicant's likelihood of a callback for the African American grouping.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Record Type						
CQE	0.642	0.125	-2.27	0.023	0.438	0.941
Constant	0.225	0.063	-5.35	0.000	0.131	0.389

Note. N = 174. McFadden R^2= 0.007. Cragg-Uhler/Nagelkerke R^2 = 0.011. AIC = 153.087. BIC = 159.406. Log likelihood = -74.544. LR chi2 (1) = 5.16. Prob > chi2 = 0.023. Standard errors adjusted for 87 clusters. Reference Category for record type is no record.

Table B.8. Logistic regression of the criminal record and racial conditions on an applicant's likelihood of a callback for the African American grouping with controls.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Record Type						
Record and CQE	0.589	0.130	-2.40	0.017	0.382	0.908
City Location						
Cleveland	1.349	1.038	0.39	0.697	0.299	6.095
Not Listed	1.470	1.546	0.370	0.714	0.187	11.539
Base Resume						
2	1.301	0.317	1.08	0.280	0.807	2.097
Submit First						
Yes	1.411	0.362	1.34	0.179	0.854	2.332
Full-Time						
Yes	1.401	1.67	0.28	0.777	0.135	14.491
Not Listed	0.477	0.758	-0.47	0.642	0.021	10.739
Staffing Agency						
Yes	41.800	46.821	3.33	0.001	4.653	375.508
Temporary						
Yes	13.159	17.819	1.90	0.057	0.926	187.006
Month Submitted						
January	1.295	1.715	0.20	0.845	0.097	17.373
March	1.860	2.297	0.50	0.615	0.165	20.926
April	0.423	0.684	-0.53	0.595	0.018	10.039
May	2.023	2.056	0.69	0.488	0.276	14.827
Time Submitted						
PM	0.792	0.742	-0.25	0.803	0.126	4.973

Posting Age						
5-14 days	1.502	1.567	0.39	0.697	0.194	11.613
Day Submitted						
Friday and Saturday	0.079	0.100	-2.01	0.044	0.007	0.935
Hourly Pay						
Above Median	2.325	2.527	0.78	0.438	0.276	19.568
Not Listed	2.136	2.548	0.64	0.524	0.206	22.119
Constant	0.040	0.072	-1.78	0.074	0.001	1.372

Notes. N = 174. McFadden R^2= 0.251. Cragg-Uhler/Nagelkerke R^2 = 0.337. AIC = 150.486. BIC = 210.508. Log pseudolikelihood = -56.243. Wald chi2 (18) = 84.94. Prob > chi2 = 0.0000. Standard error adjusted for 87 clusters. Reference Category for record type is no record. Reference category for city location is outside Cleveland City. Reference category for base resume is 1. Reference category for submitted first is no. Reference category for full-time is part-time. Reference category for job type is general labor. Reference category for staffing agency is no. Reference category for temporary is no. Reference category for month submitted is February. Reference category for time submitted is AM. Reference category for posting age is 1-4 days. Reference category for day submitted is Sunday through Thursday. Reference category for hourly pay is median and below.

Table B.9. Logistic regression of the criminal record and racial conditions on an applicant's likelihood of a callback for the White grouping.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Record Type						
CQE	0.602	0.130	-2.34	0.019	0.394	0.920
Constant	0.403	0.096	-3.81	0.000	0.253	0.643

Note. N = 174. McFadden R^2= 0.010. Cragg-Uhler/Nagelkerke R^2 = 0.017. AIC = 194.309. BIC = 200.627. Log likelihood = -95.154. LR chi2 (1) = 5.50. Prob > chi2 = 0.019. Standard errors adjusted for 87 clusters. Reference Category for record type is no record.

Table B.10. Logistic regression of the criminal record and racial conditions on an applicant's likelihood of a callback for the White grouping with controls.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Record Type						
Record and CQE	0.525	0.139	-2.43	0.015	0.312	0.883
City Location						
Cleveland	1.196	0.934	0.23	0.819	0.259	5.522
Not Listed	0.743	0.679	-0.33	0.745	0.124	4.451
Base Resume						
2	1.075	0.274	0.28	0.778	0.652	1.772
Submit First						

Yes	1.219	0.329	0.73	0.462	0.719	2.068
Full-Time						
Yes	1.55	0.977	0.7	0.487	0.451	5.331
Not Listed	1.087	1.575	0.06	0.954	0.064	18.595
Staffing Agency						
Yes	38.68	65.539	2.16	0.031	1.397	1070.893
Temporary						
Yes	2.347	1.972	1.02	0.31	0.452	12.184
Month Submitted						
January	1.127	1.451	0.09	0.926	0.09	14.072
March	0.929	0.751	-0.09	0.928	0.191	4.532
April	1.462	1.545	0.36	0.719	0.184	11.595
May	2.866	2.654	1.14	0.255	0.467	17.596
Time Submitted						
PM	0.248	0.168	-2.06	0.04	0.065	0.937
Posting Age						
5-14 days	1.263	0.942	0.31	0.754	0.293	5.45
Day Submitted						
Friday and Saturday	0.344	0.374	-0.98	0.326	0.041	2.901
Hourly Pay						
Above Median	1.847	1.477	0.77	0.443	0.386	8.852
Not Listed	4.152	2.612	2.26	0.024	1.21	14.25
Constant	0.094	0.077	-2.86	0.004	0.018	0.474

Notes. N = 174. McFadden R^2= 0.182. Cragg-Uhler/Nagelkerke R^2 = 0.273. AIC = 195.231. BIC = 255.253. Log pseudolikelihood = -78.615. Wald chi2 (18) = 32.75. Prob > chi2 = 0.018. Standard error adjusted for 87 clusters. Reference Category for record type is no record. Reference category for city location is outside Cleveland City. Reference category for base resume is 1. Reference category for submitted first is no. Reference category for full-time is part-time. Reference category for job type is general labor. Reference category for staffing agency is no. Reference category for temporary is no. Reference category for month submitted is February. Reference category for time submitted is AM. Reference category for posting age is 1-4 days. Reference category for day submitted is Sunday through Thursday. Reference category for hourly pay is median and below.

Table B.11. Logistic regression of the criminal record and racial conditions on an applicant's likelihood of a callback for the African American and White grouping.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Race						
White	1.164	0.187	0.94	0.346	0.849	1.595
Record Type						
CQE	0.634	0.103	-2.81	0.005	0.461	0.871

Constant	0.250	0.047	-7.37	0.000	0.173	0.361

Note. N = 452. McFadden R^2= 0.009. Cragg-Uhler/Nagelkerke R^2 = 0.014. AIC = 427.266. BIC = 439.607. Log likelihood = -210.633. LR chi2 (2) = 9.08. Prob > chi2 = 0.011. Standard errors adjusted for 226 clusters. Reference category for race is African American. Reference Category for record type is no record.

Table B.12. Logistic regression of the criminal record and racial conditions on an applicant's likelihood of a callback for the African American and White grouping with controls.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Race						
White	1.170	0.198	0.93	0.351	0.841	1.629
Record Type						
Record and CQE	0.621	0.108	-2.73	0.006	0.441	0.875
City Location						
Cleveland	1.099	0.463	0.22	0.823	0.481	2.510
Not Listed	0.923	0.447	-0.16	0.869	0.357	2.387
Base Resume						
2	0.870	0.149	-0.81	0.417	0.621	1.218
Submit First						
Yes	0.860	0.148	-0.88	0.381	0.614	1.205
Full-Time						
Yes	1.098	0.443	0.23	0.817	0.498	2.422
Not Listed	0.674	0.67	-0.4	0.691	0.096	4.725
Staffing Agency						
Yes	5.881	3.238	3.22	0.001	1.998	17.304
Temporary						
Yes	1.227	0.749	0.34	0.737	0.371	4.061
Month Submitted						
January	0.917	0.539	-0.15	0.883	0.29	2.902
March	0.702	0.286	-0.87	0.385	0.316	1.561
April	0.458	0.268	-1.34	0.181	0.145	1.44
May	0.717	0.472	-0.51	0.613	0.197	2.607
Time Submitted						
PM	1.217	0.399	0.60	0.549	0.640	2.316
Posting Age						
5-14 days	1.041	0.413	0.10	0.919	0.479	2.265
Day Submitted						
Friday and Saturday	0.569	0.464	-0.69	0.489	0.115	2.811
Hourly Pay						
Above Median	0.645	0.318	-0.89	0.375	0.245	1.697
Not Listed	1.061	0.409	0.15	0.878	0.498	2.260

Constant	0.315	0.177	-2.06	0.04	0.105	0.948

Notes. N = 452. McFadden R^2= 0.062. Cragg-Uhler/Nagelkerke R^2 = 0.093. AIC = 438.715. BIC = 520.988. Log pseudolikelihood = -199.357. Wald chi2 (19) = 32.94. Prob > chi2 = 0.024. Standard error adjusted for 226 clusters. Reference category for race is African American. Reference Category for record type is no record. Reference category for city location is outside Cleveland City. Reference category for base resume is 1. Reference category for submitted first is no. Reference category for full-time is part-time. Reference category for job type is general labor. Reference category for staffing agency is no. Reference category for temporary is no. Reference category for month submitted is February. Reference category for time submitted is AM. Reference category for posting age is 1-4 days. Reference category for day submitted is Sunday through Thursday. Reference category for hourly pay is median and below.

vi. First Call

Table B.13. Logistic regression of the criminal record and racial conditions on an applicant's likelihood of being called first.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Race						
White	2.128	0.951	1.69	0.091	0.887	5.109
Record Type						
CQE	0.471	0.292	-1.21	0.225	0.139	1.588
Constant	0.982	0.385	-0.05	0.963	0.456	2.117

Note. N = 90. McFadden R^2= 0.053. Cragg-Uhler/Nagelkerke R^2 = 0.095. AIC = 124.133. BIC = 131.632. Log likelihood = -59.066. LR chi2 (2) = 4.40. Prob > chi2 = 0.111. Standard errors adjusted for 45 clusters. Reference category for race is African American. Reference Category for record type is no record.

Table B.14. Logistic regression of the criminal record and racial conditions on an applicant's likelihood of being called first with controls.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Race						
White	2.423	1.169	1.83	0.067	0.941	6.240
Record Type						
CQE	0.370	0.252	-1.46	0.144	0.097	1.405
Base Resume						
2	1.212	0.772	0.30	0.763	0.347	4.225
Submitted First						
Yes	2.684	1.879	1.41	0.159	0.680	10.589
Constant	0.571	0.309	-1.03	0.301	0.198	1.651

Note. N = 90. McFadden R^2= 0.092. Cragg-Uhler/Nagelkerke R^2 = 0.160. AIC = 123.259. BIC = 135.758. Log likelihood = -56.629. LR chi2 (4) = 5.14. Prob > chi2 =

0.274. Standard errors adjusted for 45 clusters. Reference category for race is African American. Reference Category for record type is no record. Reference category for base resume is 1. Reference category for submitted first is no.

B. Between-Subjects Design

i. Main Model

Table B.15. Logistic regression of the criminal record and racial conditions on an applicant's likelihood of a callback.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Record Type						
No Record	2.643	0.551	4.66	0.000	1.756	3.978
Record	1.295	0.296	1.13	0.257	0.828	2.027
Race						
White	1.895	0.323	3.76	0.000	1.358	2.646
Constant	0.075	0.015	-12.92	0.000	0.051	0.112

Note. N = 1200. McFadden R^2= 0.040. Cragg-Uhler/Nagelkerke R^2 = 0.059. AIC = 968.115. BIC = 988.475. Log likelihood = -480.058. LR chi2 (3) = 40.40. Prob > chi2 = 0.0000. Reference Category for record type is record and CQE. Reference category for race is African American.

ii. Controls

Table B.16. Logistic regression of the criminal record and racial conditions on an applicant's likelihood of a callback with controls.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Race						
White	1.949	0.347	3.75	0.000	1.375	2.762
Record Type						
No Record	2.779	0.604	4.70	0.000	1.815	4.255
Record	1.365	0.322	1.32	0.187	0.860	2.168
City Location						
Cleveland City	0.716	0.155	-1.54	0.124	0.468	1.096
No Address	1.269	0.343	0.88	0.377	0.748	2.155
Full-Time						
Yes	1.393	0.312	1.48	0.139	0.898	2.160
Not Listed	2.428	0.885	2.44	0.015	1.189	4.959
Job Type						
Cust. Serv. Call Center	0.466	0.235	-1.51	0.131	0.173	1.254

Cust. Serv. In-Store	0.771	0.217	-0.92	0.356	0.444	1.339
Manufacturing	0.519	0.227	-1.50	0.134	0.220	1.225
Restaurant Labor	0.736	0.227	-0.99	0.320	0.403	1.346
Restaurant Cust. Serv.	0.458	0.213	-1.68	0.093	0.184	1.139
Driving	1.854	0.730	1.57	0.117	0.857	4.010
Clerical	0.232	0.117	-2.89	0.004	0.086	0.625
Sales In-Store	0.657	0.380	-0.73	0.468	0.211	2.042
Warehouse	0.606	0.229	-1.33	0.184	0.289	1.269
Sales Call Center	2.347	1.003	2.00	0.046	1.015	5.423
Multiple	1.176	0.694	0.27	0.784	0.370	3.741
Staffing Agency						
Yes	3.419	1.553	2.71	0.007	1.403	8.328
Temporary						
Yes	1.593	0.521	1.42	0.155	0.839	3.024
Month Submitted						
January	0.975	0.328	-0.07	0.940	0.504	1.885
March	0.954	0.21	-0.21	0.830	0.619	1.469
April	1.189	0.389	0.53	0.597	0.626	2.259
May	0.717	0.206	-1.16	0.247	0.408	1.259
Time Submitted						
PM	1.108	0.204	0.56	0.575	0.773	1.589
Posting Age						
5-14 Days	0.822	0.192	-0.84	0.402	0.521	1.299
Day Submitted						
Friday - Saturday	0.480	0.224	-1.57	0.116	0.192	1.200
Hourly Pay						
Above Median	1.365	0.379	1.12	0.263	0.792	2.352
Not Listed	0.942	0.196	-0.29	0.774	0.626	1.417
Constant	0.069	0.025	-7.28	0.000	0.034	0.142

Note. N = 1200. McFadden R^2= 0.099. Cragg-Uhler/Nagelkerke R^2 = 0.140. AIC = 961.890. BIC = 1114.592. Log likelihood = -450.945. LR chi2 (29) = 98.63. Prob > chi2 = 0.0000. Reference Category for record type is record and CQE. Reference category for Race is African American. Reference category for city location is outside Cleveland City. Reference category for full-time is part-time. Reference category for job type is general labor. Reference category for staffing agency is no. Reference category for temporary is no. Reference category for month submitted is February. Reference category for time submitted is AM. Reference category for posting age is 1-4 days. Reference category for day submitted is Sunday through Thursday. Reference category for hourly pay is median and below.

iii. Individual Names

Table B.17. Logistic regression of the criminal record and individual name conditions on an applicant's likelihood of a callback.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Name						
Jake	2.131	0.510	3.16	0.002	1.333	3.406
Seth	1.582	0.388	1.87	0.061	0.978	2.557
Tremayne	0.943	0.251	-0.22	0.825	0.560	1.587
Record Type						
No Record	2.701	0.566	4.74	0.000	1.791	4.074
Record	1.294	0.296	1.13	0.260	0.827	2.026
Constant	0.077	0.018	-10.7	0.000	0.048	0.123

Note. N = 1200. McFadden R^2= 0.042. Cragg-Uhler/Nagelkerke R^2 = 0.061. AIC = 970.134. BIC = 1000.675. Log likelihood = -479.067. LR chi2(5) = 42.39. Prob > chi2 = 0.0000. Reference Category for record type is record and CQE. Reference category for Name is DaQuan.

Table B.18. Logistic regression of the criminal record and individual name conditions on an applicant's likelihood of a callback with controls.

Variable	OR	SE	z	P>z	CI Upper	CI Lower
Name						
Jake	2.078	0.541	2.81	0.005	1.247	3.462
Seth	1.766	0.449	2.23	0.026	1.072	2.908
Tremayne	0.975	0.281	-0.09	0.930	0.554	1.715
Record Type						
No Record	2.801	0.611	4.72	0.000	1.827	4.295
Record	1.363	0.322	1.31	0.191	0.857	2.165
City Location						
Cleveland City	0.719	0.156	-1.52	0.129	0.470	1.101
No Address	1.281	0.347	0.91	0.361	0.753	2.177
Full-Time						
Yes	1.385	0.311	1.45	0.146	0.892	2.150
Not Listed	2.382	0.87	2.38	0.017	1.164	4.874
Job Type						
Cust. Serv. Call Center	0.474	0.239	-1.48	0.14	0.176	1.276
Cust. Serv. In-Store	0.78	0.220	-0.88	0.377	0.449	1.355
Manufacturing	0.528	0.231	-1.46	0.144	0.223	1.245
Restaurant Labor	0.739	0.228	-0.98	0.327	0.404	1.352

Restaurant Cust. Serv.	0.457	0.212	-1.68	0.092	0.184	1.137
Driving	1.844	0.727	1.55	0.121	0.851	3.992
Clerical	0.234	0.118	-2.87	0.004	0.087	0.631
Sales In-Store	0.668	0.387	-0.70	0.487	0.215	2.08
Warehouse	0.616	0.233	-1.28	0.199	0.294	1.291
Sales Call Center	2.323	0.995	1.97	0.049	1.003	5.378
Multiple	1.192	0.705	0.30	0.767	0.374	3.798
Staffing Agency						
Yes	3.398	1.545	2.69	0.007	1.394	8.284
Temporary						
Yes	1.622	0.533	1.47	0.141	0.852	3.088
Month Submitted						
January	0.985	0.332	-0.04	0.965	0.509	1.906
March	0.928	0.215	-0.32	0.747	0.589	1.462
April	1.153	0.387	0.43	0.67	0.598	2.226
May	0.731	0.211	-1.09	0.277	0.415	1.287
Time Submitted						
PM	1.112	0.205	0.58	0.564	0.775	1.595
Posting Age						
5-14 Days	0.819	0.191	-0.85	0.393	0.518	1.295
Day Submitted						
Friday - Saturday	0.485	0.227	-1.55	0.121	0.194	1.211
Hourly Pay						
Above Median	1.359	0.378	1.10	0.270	0.788	2.343
Not Listed	0.945	0.197	-0.27	0.787	0.628	1.422
Constant	0.070	0.027	-6.88	0.000	0.033	0.150

Note. N = 1200. McFadden R^2= 0.099. Cragg-Uhler/Nagelkerke R^2 = 0.140. AIC = 965.403. BIC = 1128.286. Log likelihood = -450.702. LR chi2 (31) = 99.12. Prob > chi2 = 0.0000. Reference Category for record type is record and CQE. Reference category for name is DaQuan. Reference category for city location is outside Cleveland City. Reference category for full-time is part-time. Reference category for job type is general labor. Reference category for staffing agency is no. Reference category for temporary is no. Reference category for month submitted is February. Reference category for time submitted is AM. Reference category for posting age is 1-4 days. Reference category for day submitted is Sunday through Thursday. Reference category for hourly pay is median and below.

iv. Interactions

Table B.19. Logistic regression of the criminal record and race interaction on an applicant's likelihood of a callback.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Race						
White	2.149	0.764	2.15	0.031	1.071	4.316
Record Type						
No Record	2.946	1.011	3.15	0.002	1.504	5.772
Record	1.423	0.539	0.93	0.352	0.677	2.988
Interaction Term						
White x No Record	0.840	0.364	-0.40	0.687	0.360	1.962
White x Record	0.863	0.410	-0.31	0.756	0.340	2.189
Constant	0.070	0.020	-9.30	0.000	0.040	0.122

Note. N = 1200. McFadden R^2= 0.041. Cragg-Uhler/Nagelkerke R^2 = 0.059. AIC = 971.945. BIC = 1002.486. Log likelihood = -479.973. LR chi2 (5) = 40.57. Prob > chi2 = 0.0000. Reference category for record type is record and CQE. Reference category for race is African American. Reference category for interaction term is African American with record and CQE.

Table B.20. Logistic regression of the criminal record and race interaction on an applicant's likelihood of a callback with controls.

Variable	OR	SE	z	P>z	CI Lower	CI Upper
Race						
White	2.245	0.820	2.21	0.027	1.097	4.594
Record Type						
No Record	3.149	1.109	3.26	0.001	1.579	6.279
Record	1.509	0.587	1.06	0.291	0.703	3.235
Interaction Term						
White x No Record	0.816	0.365	-0.46	0.648	0.340	1.959
White x Record	0.853	0.420	-0.32	0.747	0.325	2.238
City Location						
Cleveland City	0.715	0.155	-1.54	0.122	0.468	1.094
No Address	1.271	0.343	0.89	0.375	0.748	2.158
Full-Time						
Yes	1.396	0.313	1.49	0.137	0.900	2.165
Not Listed	2.444	0.895	2.44	0.015	1.193	5.009
Job Type						
Cust. Serv. Call Center	0.467	0.236	-1.51	0.132	0.174	1.257
Cust. Serv. In-Store	0.771	0.217	-0.93	0.354	0.444	1.337
Manufacturing	0.520	0.227	-1.50	0.135	0.22	1.225
Restaurant Labor	0.735	0.226	-1.00	0.317	0.402	1.344
Restaurant Cust. Serv.	0.459	0.214	-1.67	0.094	0.185	1.143
Driving	1.864	0.735	1.58	0.114	0.861	4.037
Clerical	0.231	0.117	-2.90	0.004	0.086	0.623

Sales In-Store	0.657	0.380	-0.72	0.469	0.211	2.044
Warehouse	0.610	0.23	-1.31	0.190	0.291	1.277
Sales Call Center	2.314	0.993	1.95	0.051	0.998	5.366
Multiple	1.173	0.693	0.27	0.787	0.369	3.734
Staffing Agency						
Yes						
Temporary	3.421	1.559	2.70	0.007	1.401	8.355
Yes	1.591	0.521	1.42	0.156	0.838	3.023
Month Submitted						
January	0.974	0.328	-0.08	0.937	0.503	1.884
March	0.952	0.21	-0.22	0.825	0.618	1.467
April	1.187	0.391	0.52	0.602	0.622	2.266
May	0.717	0.206	-1.16	0.247	0.408	1.259
Time Submitted						
PM	1.106	0.203	0.55	0.584	0.771	1.586
Posting Age						
5-14 Days	0.822	0.192	-0.84	0.400	0.520	1.298
Day Submitted						
Friday - Saturday	0.476	0.223	-1.59	0.112	0.190	1.190
Hourly Pay						
Above Median	1.361	0.378	1.11	0.267	0.790	2.346
Not Listed	0.942	0.196	-0.29	0.773	0.626	1.416
Constant	0.063	0.027	-6.58	0.000	0.028	0.144

Note. N = 1200. McFadden R^2= 0.099. Cragg-Uhler/Nagelkerke R^2 = 0.140. AIC = 965.676. BIC = 1128.559. Log likelihood = -450.838. LR chi2 (31) = 98.84. Prob > chi2 = 0.0000. Reference Category for record type is record and CQE. Reference category for race is African American. Reference category for interaction term is African American x CQE. Reference category for city location is outside Cleveland City. Reference category for full-time is part-time. Reference category for job type is general labor. Reference category for staffing agency is no. Reference category for temporary is no. Reference category for month submitted is February. Reference category for time submitted is AM. Reference category for posting age is 1-4 days. Reference category for day submitted is Sunday through Thursday. Reference category for hourly pay is median and below.

APPENDIX C

PROBABILITIES AND AVERAGE MARGINAL EFFECTS FOR CONTROL VARIABLES

A. Mixed Design

Table C.1. Probability of a callback for controls.

Variable	Margin	SE	CI Lower	CI Upper
City Location				
Outside Cleveland City	0.189	0.021	0.149	0.229
Cleveland City	0.195	0.035	0.127	0.264
Not Listed	0.166	0.044	0.080	0.252
Base Resume				
1	0.190	0.019	0.154	0.226
2	0.185	0.018	0.151	0.220
Submitted First				
No	0.187	0.018	0.152	0.221
Yes	0.188	0.019	0.152	0.225
Full-Time				
No	0.173	0.039	0.096	0.249
Yes	0.190	0.019	0.153	0.228
Not Listed	0.197	0.078	0.044	0.349
Job Type				
Cust. Serv. Call Center	0.202	0.077	0.051	0.353
Cust. Serv. In-Store	0.153	0.042	0.072	0.235
Manufacturing	0.129	0.055	0.020	0.237
General Labor	0.249	0.036	0.179	0.318
Restaurant Labor	0.142	0.061	0.022	0.262
Restaurant Cust. Serv.	0.154	0.079	-0.002	0.309
Driving	0.165	0.068	0.032	0.297
Clerical	0.071	0.038	-0.003	0.145
Sales In-Store	0.195	0.076	0.047	0.344
Warehouse	0.164	0.051	0.064	0.264
Sales Call Center	0.528	0.137	0.261	0.796
Multiple	0.103	0.079	-0.051	0.257

Staffing Agency				
No	0.167	0.016	0.135	0.199
Yes	0.660	0.093	0.477	0.843
Temporary				
No	0.181	0.017	0.148	0.213
Yes	0.268	0.071	0.129	0.408
Month Submitted				
January	0.175	0.051	0.075	0.275
February	0.212	0.033	0.147	0.276
March	0.163	0.027	0.110	0.215
April	0.118	0.037	0.045	0.190
May	0.258	0.055	0.151	0.365
Time Submitted				
AM	0.194	0.022	0.151	0.238
PM	0.177	0.024	0.129	0.225
Posting Age				
1-4 Days	0.183	0.017	0.149	0.217
5-14 Days	0.211	0.043	0.126	0.296
Day Submitted				
Sunday - Thursday	0.201	0.018	0.166	0.237
Friday - Saturday	0.079	0.039	0.004	0.155
Hourly Pay				
Median and Below	0.170	0.034	0.103	0.237
Above Median	0.171	0.034	0.105	0.237
No Pay Listed	0.203	0.024	0.156	0.25

Table C.2. Average marginal effects for controls.

Variable	Difference	SE	z	P>z	CI Lower	CI Upper
City Location						
Cleveland City	0.006	0.041	0.15	0.880	-0.075	0.088
No Address	-0.023	0.049	-0.48	0.634	-0.118	0.072
Base Resume						
2	-0.005	0.017	-0.28	0.777	-0.037	0.028
Submitted First						
Yes	0.002	0.017	0.10	0.919	-0.031	0.034
Full-Time						
Yes	0.018	0.045	0.39	0.696	-0.070	0.106
Not Listed	0.024	0.089	0.27	0.787	-0.150	0.197
Job Type						
Cust. Serv. Call Center	-0.047	0.085	-0.54	0.586	-0.214	0.121
Cust. Serv. In-Store	-0.095	0.056	-1.72	0.086	-0.204	0.014

Manufacturing	-0.120	0.064	-1.87	0.062	-0.246	0.006
Restaurant Labor	-0.107	0.071	-1.51	0.132	-0.246	0.032
Restaurant Cust. Serv.	-0.095	0.088	-1.09	0.277	-0.267	0.077
Driving	-0.084	0.076	-1.11	0.268	-0.233	0.065
Clerical	-0.178	0.051	-3.47	0.001	-0.278	-0.077
Sales In-Store	-0.053	0.085	-0.63	0.531	-0.220	0.114
Warehouse	-0.085	0.062	-1.36	0.173	-0.206	0.037
Sales Call Center	0.280	0.144	1.94	0.052	-0.002	0.562
Multiple	-0.146	0.084	-1.74	0.082	-0.310	0.019
Staffing Agency						
Yes	0.493	0.095	5.19	0.000	0.307	0.679
Temporary						
Yes	0.087	0.074	1.19	0.236	-0.057	0.232
Month Submitted						
January	-0.037	0.061	-0.61	0.542	-0.156	0.082
March	-0.049	0.043	-1.14	0.252	-0.133	0.035
April	-0.094	0.051	-1.84	0.065	-0.194	0.006
May	0.046	0.066	0.70	0.483	-0.083	0.176
Time Submitted						
PM	-0.017	0.034	-0.49	0.622	-0.083	0.050
Posting Age						
5-14 Days	0.028	0.047	0.61	0.544	-0.063	0.120
Day Submitted						
Friday - Saturday	-0.122	0.044	-2.77	0.006	-0.209	-0.036
Hourly Pay						
Above Median	0.001	0.047	0.03	0.979	-0.092	0.094
Not Listed	0.033	0.044	0.75	0.454	-0.053	0.118

Note. Reference category for city location is outside Cleveland City. Reference category for base resume is 1. Reference category for submitted first is no. Reference category for full-time is part-time. Reference category for job type is general labor. Reference category for staffing agency is no. Reference category for temporary is no. Reference category for month submitted is February. Reference category for time submitted is AM. Reference category for posting age is 1-4 days. Reference category for day submitted is Sunday through Thursday. Reference category for hourly pay is median and below.

Table C.3. Probability of a callback for alternate coding of Full-Time and City Location.

Variable	Margin	SE	CI Lower	CI Upper
Full-Time				
No	0.173	0.039	0.096	0.250
Yes	0.189	0.021	0.148	0.230
Not Listed	0.197	0.078	0.044	0.350

Both	0.199	0.054	0.092	0.305
City Location				
Outside Cleveland City	0.189	0.021	0.149	0.230
Cleveland City	0.195	0.035	0.127	0.263
No City Listed	0.161	0.084	-0.004	0.325
Multiple Cities	0.198	0.082	0.0370	0.359
Out of State	0.158	0.057	0.046	0.270

Table C.4. Average marginal effects of alternate coding of Full-Time and City Location.

Variable	Difference	SE	z	P>z	CI Lower	CI Upper
City Location						
Cleveland City	0.006	0.041	0.14	0.887	-0.075	0.087
No Address	-0.029	0.086	-0.33	0.738	-0.198	0.140
Multiple Cities	0.008	0.085	0.10	0.922	-0.159	0.176
Out of State	-0.031	0.061	-0.52	0.605	-0.151	0.088
Full-Time						
Yes	0.016	0.047	0.34	0.732	-0.075	0.107
Not Listed	0.024	0.088	0.27	0.785	-0.149	0.198
Both	0.026	0.065	0.39	0.695	-0.103	0.154

Note. Reference category for city location is outside Cleveland City. Reference category for full-time is part-time.

B. Between-Subjects Design

Table C.5. Probability of a callback for controls.

Variable	Margin	SE	CI Lower	CI Upper
City Location				
Outside Cleveland City	0.153	0.013	0.128	0.177
Cleveland City	0.117	0.018	0.082	0.152
Not Listed	0.183	0.033	0.118	0.248
Full-Time				
No	0.115	0.019	0.078	0.151
Yes	0.150	0.012	0.126	0.173
Not Listed	0.226	0.047	0.134	0.318
Job Type				
Cust. Serv. Call Center	0.094	0.039	0.018	0.170
Cust. Serv. In-Store	0.142	0.027	0.090	0.195
Manufacturing	0.103	0.036	0.032	0.173
General Labor	0.174	0.019	0.137	0.212
Restaurant Labor	0.137	0.030	0.079	0.196

Restaurant Cust. Serv.	0.092	0.035	0.024	0.161
Driving	0.271	0.066	0.142	0.400
Clerical	0.050	0.022	0.007	0.094
Sales In-Store	0.125	0.057	0.013	0.237
Warehouse	0.117	0.034	0.051	0.183
Sales Call Center	0.315	0.077	0.164	0.466
Multiple	0.197	0.083	0.034	0.360
Staffing Agency				
No	0.142	0.010	0.123	0.161
Yes	0.332	0.086	0.163	0.501
Temporary				
No	0.143	0.010	0.123	0.162
Yes	0.202	0.045	0.114	0.290
Month Submitted				
January	0.150	0.035	0.081	0.219
February	0.153	0.018	0.118	0.188
March	0.147	0.018	0.112	0.182
April	0.174	0.037	0.103	0.246
May	0.117	0.023	0.073	0.162
Time Submitted				
AM	0.142	0.012	0.118	0.167
PM	0.154	0.017	0.121	0.187
Posting Age				
1-4 Days	0.151	0.011	0.129	0.174
5-14 Days	0.13	0.021	0.088	0.172
Day Submitted				
Sunday - Thursday	0.151	0.010	0.131	0.171
Friday - Saturday	0.083	0.033	0.019	0.146
Hourly Pay				
Median and Below	0.146	0.019	0.109	0.182
Above Median	0.184	0.031	0.123	0.245
No Pay Listed	0.139	0.013	0.114	0.164

Table C.6. Average marginal effects for controls.

Variable	Difference	SE	z	P>z	CI Lower	CI Upper
City Location						
Cleveland City	-0.036	0.022	-1.62	0.106	-0.079	0.008
No Address	0.030	0.036	0.84	0.399	-0.04	0.100
Full-Time						
Yes	0.035	0.022	1.56	0.118	-0.009	0.079

Not Listed	0.111	0.051	2.17	0.030	0.011	0.211
Job Type						
Cust. Serv. Call Center	-0.081	0.043	-1.86	0.063	-0.166	0.004
Cust. Serv. In-Store	-0.032	0.034	-0.95	0.340	-0.098	0.034
Manufacturing	-0.072	0.041	-1.76	0.078	-0.151	0.008
Restaurant Labor	-0.037	0.036	-1.04	0.298	-0.107	0.033
Restaurant Cust. Serv.	-0.082	0.040	-2.04	0.041	-0.161	-0.003
Driving	0.097	0.068	1.41	0.158	-0.038	0.231
Clerical	-0.124	0.029	-4.22	0.000	-0.182	-0.066
Sales In-Store	-0.049	0.061	-0.81	0.416	-0.168	0.069
Warehouse	-0.057	0.039	-1.48	0.139	-0.133	0.019
Sales Call Center	0.141	0.080	1.76	0.079	-0.016	0.298
Multiple	0.023	0.085	0.26	0.792	-0.145	0.190
Staffing Agency						
Yes	0.190	0.087	2.19	0.029	0.020	0.360
Temporary						
Yes	0.060	0.047	1.28	0.200	-0.032	0.151
Month Submitted						
January	-0.003	0.039	-0.08	0.940	-0.080	0.074
March	-0.005	0.026	-0.21	0.830	-0.056	0.045
April	0.021	0.042	0.52	0.606	-0.060	0.103
May	-0.035	0.030	-1.2	0.230	-0.093	0.022
Time Submitted						
PM	0.012	0.021	0.56	0.578	-0.030	0.053
Posting Age						
5-14 Days	-0.022	0.025	-0.87	0.386	-0.070	0.027
Day Submitted						
Friday - Saturday	-0.068	0.034	-1.99	0.047	-0.136	-0.001
Hourly Pay						
Above Median	0.039	0.036	1.08	0.278	-0.031	0.109
Not Listed	-0.007	0.023	-0.28	0.776	-0.052	0.039

Note. Reference category for city location is outside Cleveland City. Reference category for full-time is part-time. Reference category for job type is general labor. Reference category for staffing agency is no. Reference category for temporary is no. Reference category for month submitted is February. Reference category for time submitted is AM. Reference category for posting age is 1-4 days. Reference category for day submitted is Sunday through Thursday. Reference category for hourly pay is median and below.

Table C.7. Probability of a callback for alternate coding of Full-Time and City Location.

Variable	Margin	SE	CI Lower	CI Upper
Full-Time				

No	0.116	0.019	0.080	0.153
Yes	0.149	0.013	0.124	0.174
Not Listed	0.222	0.047	0.131	0.314
Both	0.154	0.039	0.078	0.230
City Location				
Outside Cleveland City	0.152	0.013	0.128	0.177
Cleveland City	0.117	0.018	0.083	0.152
No City Listed	0.142	0.040	0.063	0.221
Multiple Cities	0.220	0.070	0.082	0.357
Out of State	0.265	0.095	0.079	0.451

Table C.8. Average marginal effects of alternate coding of Full-Time and City Location.

Variable	Difference	SE	z	P>z	CI Lower	CI Upper
City Location						
Cleveland City	-0.035	0.022	-1.58	0.114	-0.078	0.008
No Address	-0.010	0.042	-0.24	0.808	-0.093	0.072
Multiple Cities	0.067	0.071	0.94	0.346	-0.073	0.207
Out of State	0.112	0.096	1.17	0.243	-0.076	0.301
Full-Time						
Yes	0.033	0.023	1.43	0.153	-0.012	0.078
Not Listed	0.106	0.051	2.09	0.037	0.007	0.206
Both	0.038	0.043	0.88	0.379	-0.046	0.122

Note. Reference category for city location is outside Cleveland City. Reference category for full-time is part-time.