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Nonconviction Arrests and Labor Market Trajectories

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NONCONVICTION ARRESTS AND LABOR MARKET TRAJECTORIES

Erin Meyers Matsick[1]

In this Article, I examine whether nonconviction arrests—often viewed as minor legal events—destabilize labor market trajectories. While extensive research focuses on the collateral consequences of incarceration and felony convictions, significantly less is known about the economic impact of low-level criminal justice contact. Using longitudinal data from the National Longitudinal Survey of Youth 1997 (NLSY97) and individual fixed-effects models, I isolate the relationship between nonconviction arrests and labor market outcomes among men who have never been convicted of a crime.

I find that a recent nonconviction arrest is associated with a 20 percent increase in the likelihood of employer separation. This immediate disruption is likely driven by absenteeism due to detention or court appearances. In the longer term, the penalties shift toward diminished job quality, likely due to negative credentialing. Prior nonconviction arrests are associated with hourly wages that are approximately 8.4 percent lower, as well as significant reductions in access to employer-provided health insurance and retirement benefits. Reductions in employee benefits are primarily driven by a reduced likelihood of obtaining full-time employment.

Finally, I discuss the troubling implications for racial inequality, as Black men are disproportionately likely to experience nonconviction arrests. I conclude by arguing that policymakers must treat these labor-market harms as a meaningful social cost when evaluating aggressive low-level enforcement strategies, such as broken windows policing, and should consider less disruptive alternatives like citations and pretrial reforms.

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INTRODUCTION

Often, the hardships faced by individuals accused of crimes far exceed the formal penalties prescribed by law. In his 1979 book, *The Process is the Punishment: Handling Cases in a Lower Criminal Court*, Malcolm Feeley coined the titular phrase that is well-known among criminal justice researchers. The concept that "the process is the punishment" describes the experience of those arrested and prosecuted for low-level crimes, for which the formal sanctions imposed by the law are minimal (Feeley 1979). For these individuals, the greatest degree of harm is caused by the experience of criminal justice processing itself, having sacrificed money, time, and opportunities to the process, and often emerging with a criminal record.

When Feeley published his book, the era of mass incarceration was still in its early stages. As the number of incarcerated individuals skyrocketed in the 1980s, research on incarceration and related felony convictions took center stage, while the low-level criminal contact described by Feeley received far less attention (Kohler-Hausmann 2022). As a result, barriers to employment faced by individuals released from jail or prison have been studied extensively. But significantly less is known about how low-level contact impacts labor market opportunities (Stagoff-Belfort et al. 2022; Engel et al. 2019).

More recent research on low-level contact has highlighted a system of mass misdemeanors resulting from broken windows policing (Jain 2015; Kohler-Hausmann 2014; Natapoff 2011). One product of the system of mass misdemeanors is an increase in the number of arrests that do not result in conviction (Mayson & Stevenson 2020; Kohler-Hausmann 2013). These nonconviction arrests end without any formal punishment, incarceration or otherwise. In many cases, prosecutors decline to bring charges while, in others, charges are dropped. In a smaller subset of cases, a not guilty verdict is returned. From a legal perspective, these individuals are not guilty of any crime. But arrests themselves can lead to informal consequences, mirroring those of conviction and incarceration.

This Article expands on a line of literature that uses the National Longitudinal Survey of Youth - 1997 (NLSY97) to determine how varying levels of criminal justice contact impact respondents' labor market trajectories (Fernandes 2020; Sheely 2020; Smith & Broege 2020; Apel & Powell 2019). Building on these foundations, this Article makes three primary contributions to the literature. First, I limit my analysis to nonconviction arrests—one of the lowest possible forms of formalized criminal justice contact. For reasons described below, the existence of labor market penalties stemming solely from arrest raises unique policy concerns.

Second, I bifurcate my analysis to distinguish between labor market outcomes immediately following an arrest and longer-term outcomes. Arrest likely affects different dimensions of employment through distinct mechanisms and timelines. I hypothesize that arrest will have its strongest immediate effects on individuals who were employed prior to arrest. In the short term, arrest may disrupt existing employment relationships through absenteeism and negative credentialing, increasing the likelihood of employer separation. By contrast, wage penalties and reductions in job quality are less likely to emerge immediately. These effects are more plausibly driven by negative credentialing during subsequent job searches, when employers evaluate

applicants with arrest records. Because these mechanisms primarily operate when individuals seek new employment, the effects on wages and job quality are expected to materialize over a longer time horizon.

Third, I provide a more granular analysis of employment disruption and job quality than previous studies. To examine the relationship between arrest and short-term employment disruption, I analyze employer separation among men who were employed in the prior survey period. Employment disruption is defined as no longer being employed by the same employer in the subsequent survey wave. I find that experiencing a recent nonconviction arrest is associated with an approximately 8 percentage point increase in the likelihood of employment disruption, which represents approximately a 20 percent increase relative to the baseline employment disruption rate between surveys.[2]

I next examine the longer-term relationship between nonconviction arrests and job quality, focusing on wages and common employee benefits. On average, employee benefits account for approximately thirty percent of civilian workers' total compensation,[3] but the prevalence of access to benefits has not been studied in the literature. Nonconviction arrests are associated with hourly wages approximately 8.4 percent lower in the years following arrest. Nonconviction arrests are also associated with reduced access to employment benefits: following a nonconviction arrest, arrestees are approximately 5.9 percentage points less likely to hold jobs providing health insurance and 7.6 percentage points less likely to hold jobs providing retirement benefits. This reduction appears to be driven by arrestees being limited to part-time employment opportunities, as prior arrestees are 6.2 percentage points less likely to have full-time employment.

The finding that nonconviction arrests are associated with subsequent labor-market disadvantages has important policy implications for several reasons. First, collateral labor-market consequences may arise at an early stage of criminal justice processing, before any formal adjudication of guilt. The criminal legal system contains multiple institutional actors—including prosecutors, judges, and juries—that operate as checks before formal punishment is imposed. By contrast, labor-market penalties associated with arrest may be triggered solely by the initial arrest

[2] Across the sample, the average employment separation per survey period for previously employed respondents is 40 percent.

[3] U.S. BUREAU OF LABOR STAT., EMPLOYER COSTS FOR EMPLOYEE COMPENSATION — JUNE 2023 (2023), https://www.bls.gov/news.release/pdf/ecec.pdf.

decision itself. In many cases, a single officer's discretionary decision to arrest may generate lasting economic consequences, even where charges are never filed or a conviction is never obtained.

Second, growing evidence suggests that Black men are disproportionately likely to experience arrests that do not ultimately result in conviction (Harrington & Shaffer 2025; Meyers 2022; Tomic & Hakes 2008). As a result, any labor-market penalties associated with nonconviction arrests are likely to disproportionately burden Black men and may contribute to cumulative racial inequality in employment and earnings.

Third, economic theory has long suggested that legitimate labor-market opportunities may influence incentives for criminal behavior (Becker 1968). To the extent criminal justice contact weakens employment stability and long-term job quality, the criminal legal system may indirectly contribute to future criminal behavior by reducing attachment to the formal labor market.

I. THEORY

While the most visible sanctions imposed by the criminal legal system are formal punishments such as incarceration, fines, or supervision, individuals may also experience collateral consequences. In the employment context, informal collateral consequences arise when employers respond negatively to criminal justice contact, including arrests that do not ultimately result in conviction (Shimizu 2018; Geffen 2017).

This Article posits that arrests may affect labor-market outcomes through distinct mechanisms operating over different time horizons. In the short term, arrest may destabilize existing employment relationships through absenteeism, detention, court appearances, or negative credentialing. In the longer term, arrests may diminish future labor-market opportunities through negative credentialing, to the extent employers evaluate applicants with arrest records less favorably.

A. Short Term Employment Disruption

Arrest may immediately disrupt existing employment relationships (Grau et al. 2023; Smith 2022; Shimizu 2018; Geffen 2017). Approximately 58% percent of arrested men in the NLSY97 are employed in the formal labor market the survey period prior to a nonconviction

arrest.[4] If an employer learns of the arrest, the employer may decide to discharge or discipline an arrested employee for one of two reasons: absenteeism or negative credentialing.

When arrestees are forced to miss work either due to detention or to attend legal proceedings, they may be fired for absenteeism. An individual may be detained upon the initial arrest, which can last up to a few days, even if no charges are ultimately brought.[5] If the arrestee is charged, the amount of time away from work can increase exponentially. At the most restrictive end of the spectrum, an arrestee may be subject to pretrial detention, meaning that they must await the resolution of their charge in jail. This can last months or even years if the individual cannot afford or is not granted bail (Einesman 1992). Even those who are not detained pretrial will likely need to attend various time-consuming legal proceedings, which may cause the arrestee to miss work.

Even if absenteeism does not lead to dismissal, stigma from the arrest could result in discharge. This is known as "negative credentialing" (Pager 2003). In other words, an employer may draw a negative inference about the employee's character and fitness for the job due to arrest. The employer also may worry that retaining the employee will expose them to liability in tort, through the doctrines of vicarious liability or negligent retention (Lam & Harcourt 2003).

This, of course, hinges on whether the employer learns of the arrest in the first place. Most background checks of employees are run at the hiring stage and not continued throughout the course of employment. Instead, the most common way an employer would find out about an arrest is if their employee needed to miss work. As such, firing due to negative credentialing versus absenteeism may be difficult, if not impossible, to disentangle. Regardless, the risk of firing from either absenteeism or negative credentialing increases with the length of time an employee is away from work, due to either detainment or attending legal proceedings.

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[4] This is number is plausible, given the finding from the most recent Survey of Prison Inmates in 2016, which found that slightly over 60 percent of men in state and federal prisoners were employed in the 30 days prior to arrest. *Just the Stats*, BUREAU JUSTICE STATISTICS, https://bjs.ojp.gov/employment-state-and-federal-prisoners-prior-incarceration-2016 (last visited May 20, 2026).

[5] States vary in the length of time an arrestee may be held before arraignment. Often, the required time period is defined as "without undue delay." Some states have defined time limits, which range from 24 to 96 hours. *When Does a "First Appearance' take Place in Your State*, NAT'L CONF. STATE LEGISLATURES (Feb. 20, 2023), https://www.ncsl.org/civil-and-criminal-justice/when-does-a-first-appearance-take-place-in-your-state.

B. Negative Credentialing and Job Quality

After any criminal legal proceedings have concluded, arrests may continue to affect labor-market outcomes through diminished future employment prospects. Employers frequently use criminal background checks during hiring, and arrest records are often widely accessible through both commercial background-check providers and internet searches. In some jurisdictions, arrests may also affect eligibility for occupational licenses required in certain professions.

In addition, if an employee is fired due to arrest, he will lose the benefits that come with tenure at an employer, being forced to start again with a new employer. And if job discharge leads to a job-gap in his resume, it may function as negative credentialling, independent of any background checks run by potential employers.

II. PRIOR EMPIRICAL RESEARCH

Extensive data confirms a strong correlation between criminal justice contact and adverse labor market outcomes. For example, a recent study reports that by age 35, approximately 64 percent of unemployed men had an arrest history as an adult (Bushway et al. 2022). However, a central question in this literature is whether criminal justice contact independently affects labor-market outcomes or merely reflects preexisting differences between individuals who experience criminal justice contact and those who do not. Criminal-justice-involved individuals differ from the general population along many dimensions associated with labor-market performance, including educational attainment, substance abuse, and mental illness. Differential selection theory suggests that observed employment disparities may reflect underlying characteristics rather than a causal effect of criminal justice contact itself (Apel & Powell 2019).

Because random assignment to criminal justice contact is impossible, researchers rely on quasi-experimental methods to approximate causal inference. Studies in this literature commonly employ individual fixed effects, sibling fixed effects, instrumental variables, difference-in-differences, and related approaches (Kim & Steiner 2016). While no observational design can perfectly isolate causality, these methods help disentangle the relationship between criminal justice contact and labor-market outcomes from underlying selection effects.

Grogger (1995) is one of the seminal and most widely cited papers to study the impact of arrests on labor market outcomes. Grogger uses administrative data, combining data from the

California Department of Justice with the California Employment Development Department. He uses a fixed-effects model to examine the impact of pre-1984 arrest, conviction, and incarceration on earnings during the period of 1980-84. Grogger finds that experiencing an arrest between 1980 and 1984 resulted in worse labor market outcomes, but that the impact was moderate and short-lived.

Critically, the period Grogger analyzed represents a labor market and carceral landscape that is fundamentally distinct from the contemporary era. Since the early 1980s, two structural shifts have likely amplified the labor market consequences of arrest. First, the use of pretrial detention has expanded drastically since the early 1980s, with the number of people held in jail pretrial nearly quadrupling since that time.[6] Historically, pretrial detention was used solely to address flight risk and ensure appearance for trial.[7] A shift to allow public safety to factor into detention decisions has driven this expansion, which was still very much ongoing during Grogger's study. The expanded use of pretrial detention has likely caused a far higher proportion of arrestees to miss work, potentially leading to the more employment instability in the short term.

Second, Grogger's study predates the advent of the internet and digitization of criminal records. Contemporary employers have expansive access to arrest data through commercial background checks and online searches. Consequently, a single arrest can now function as a perpetual negative credential that marks an individual regardless of whether their case resulted in a conviction.[8]

More recent research has examined arrest in the context of the modern labor market. Existing contemporary research identifies a short-term penalty for employment status. For example, both Fernandes (2020) and Sheely (2020) utilize the NLSY97 and individual fixed-effects models to find that arrest corresponds to reductions in job stability. Fernandes (2020) finds that an arrest corresponds to two fewer weeks worked in the following year, independent of

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[6] *Pretrial Detention*, PRISON POL'Y INITIATIVE, https://www.prisonpolicy.org/research/pretrial_detention/ (last visited May 13, 2026).
[7] Samuel R. Wiseman, *Pretrial Detention and the Right to Be Monitored*, 123 YALE L.J. 1344, 1351 (2014).
[8] Finlay (2009) provides empirical evidence for this likely change over time, using the NLSY97. Finlay applies a difference-in-differences model to assess the impact of state-level laws that facilitated access to criminal history data on the internet. He finds that expanded access results in those with criminal records being worse off in terms of both income and employment. Keith Finlay, *Effect of Employer Access to Criminal History Data on the Labor Market Outcomes of Ex-Offenders and Non-Offenders*, NBER Working Paper w13935 (2008).

conviction. Similarly, Sheely (2020) demonstrates that for women, arrest appears to be the primary driver of subsequent unemployment and labor market detachment, with conviction and incarceration providing no significant added impact. These longitudinal survey findings are corroborated by related studies on pretrial detention using administrative data (Grau et al. 2023; Smith 2022; Dobbie et al. 2018). For example, Smith (2022) studies the experience of 191 low-level misdemeanor arrestees from the San Francisco Bay area. Smith finds post-arrest job loss is prevalent among the sample and seemingly driven by pretrial detention. Rates of job loss increased substantially as time held in jail increased: 17 percent held for under four days lost jobs, 46 percent held for four-to-seven days lost jobs, and 77 percent held for eight or more days lost jobs.

There is little existing evidence of an arrest penalty on job quality among those who remain employed. Fernandes (2020), for example, finds no statistically significant relationship between arrest and wages in the year immediately following arrest. Similarly, Apel and Powell (2019), using sibling fixed effects and NLSY97 data measured approximately thirteen years after respondents' first arrests, find no statistically significant relationship between arrest and wages.

There are several reasons, however, why existing studies may understate the job quality consequences of arrest. First, wage analyses are typically conditioned on current employment. Individuals who lose their jobs following an arrest and have not secured new employment are excluded from wage analyses altogether rather than treated as having zero earnings. As a result, the workers who remain employed in the immediate aftermath of arrest may be disproportionately higher earning or more economically stable than those who were terminated. This selection effect can make post-arrest wages appear relatively stable even when arrest has substantially disrupted employment opportunities. Put differently, the job quality consequences of arrest may not fully emerge until individuals reenter the labor market and seek new employment following job separation (Grau et al. 2023).

In addition, existing studies may combine forms of criminal justice contact that operate differently in labor markets. For example, Apel and Powell appear to include juvenile arrests alongside adult arrests in their analysis. Juvenile records are often subject to greater confidentiality protections and may therefore generate weaker labor-market signaling effects than adult arrests. To the extent juvenile and adult arrests operate differently in employment markets, combining them may attenuate estimated effects. Further, Apel and Powell combine men and women in their

analysis, who might experience different forms of labor market penalties.[9] Finally, wage penalties may diminish over time as arrests become more temporally distant from hiring decisions.

This Article contributes to the existing literature in three primary ways. First, it is designed to isolate the relationship between nonconviction arrests and labor market outcomes, given the unique concerns surrounding penalties associated with nonconviction. Second, it provides separate analyses to distinguish between potential short-term employment disruption and longer-term job-quality effects. Finally, it provides a more granular assessment of labor-market consequences beyond wages and employment status, including employment disruption and common employee benefits.

III. DATA AND METHODOLOGY

I study the relationship between nonconviction arrest and labor market outcomes using the NLSY97. The NLSY97 follows 8,984 individuals' life trajectories beginning in 1997. The initial 1997 survey was conducted when respondents were between ages 12 and 18. Follow-up surveys were conducted annually until 2011, and every other year since. While data is available through more recent rounds of the survey, I use data from 1997-2019 to avoid any confounding impact of the COVID-19 pandemic. The NLSY97 is particularly well-suited to examine the relationship between criminal justice contact and labor market outcomes because of its longitudinal, nationally representative nature, extensive information on respondents' individual characteristics, and detailed data on both criminal justice contact and labor market outcomes.

This Article focuses specifically on arrests that do not result in a conviction. Following the approach used by Sheely (2020) and Apel and Sweeten (2012), I therefore restrict the sample to respondents whose highest level of involvement with the criminal justice system throughout the period is arrest, to ensure I am not capturing any impact of arrests that result in conviction. Consistent with prior research in this literature, I limit the analysis to men due to men's disproportionate representation in the criminal legal system and the distinctive nature of men and women's experiences with the criminal justice system and in the labor market. This leaves me with a sample of 3,489 men.

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[9] For example, if women are likely to detach from the labor market following an arrest—a result found by Sheely (2019)—selection effects could skew any impact on their wages.

A. Arrest History

At each survey, respondents were asked whether they were arrested since the date of their previous survey.[10] I create two separate variables that capture respondents' arrest histories—one focused on measuring the impact of recent arrest and the other focused on the impact of any prior arrest. The "recent arrest" variable indicates whether the respondent was arrested between the previous survey and the current survey. This variable is intended to capture the short-term consequences of arrest, including potential job disruption.

The "prior arrest" measure identifies whether a respondent reported an adult, nonconviction arrest in any previous survey.[11] This variable is intended to capture potential extended labor-market consequences associated with negative credentialing and diminished future employment opportunities. Of the 3,489 men in the sample who have never been convicted, 707 experienced a nonconviction adult arrest at some point in the survey.

B. Labor Market Outcomes

I focus my analysis on "employee-type" jobs, which are defined in the NLSY97 as "a situation in which the respondent has an ongoing relationship with a specific employer." I limit the outcomes in this way because these are the types of jobs in which negative credentialing and dismissal due to arrest are most likely to play a role (Apel & Powell 2019). Anecdotal and some empirical evidence suggests that individuals with criminal records who cannot obtain employment in the labor market often turn to self-employed ventures (Finlay et al. 2023).

Respondents can report multiple jobs over the course of the survey period. For respondents who report more than one job, the NLSY creates an indicator variable for whether an individual's job is their "main job" at the time of the survey:[12] My analyses are limited to respondents' "main job," as it is intended to capture the respondent's primary attachment to the labor market and

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[10] For sensitive topics, including criminal justice activity, the NLSY uses a detailed self-administered questionnaire to reduce concerns of underreporting in an interview setting.

[11] For example, if a respondent reports a single arrest in the 2005 wave of the survey, the indicator variable for recent arrest will be equal to 1 in 2005 and 0 in all other years. For this respondent, the indicator variable for prior arrest will be equal to 1 in all survey periods after 2005, and equal to 0 otherwise.

[12] If an individual holds more than one job simultaneously, the main job is that where he usually works the most hours. If the respondent works the same amount of hours at more than one job, the main job is the one at which he has been working the longest. *Appendix 2: Employment Variable Creation,* NAT'L LONGITUDINAL SURVEYS, https://www.nlsinfo.org/content/cohorts/nlsy97/other-documentation/codebook-supplement/appendix-2-employment-variable (last visited Feb. 11, 2025).

principal source of earnings. Consistent with the theoretical framework described above, I distinguish between short-term measures of employment disruption and longer-term measures of job quality.

1. Employment Disruption

In the survey period immediately following arrest, the analysis focuses on employment disruption. Accordingly, I limit my short-term analysis to those who were formally employed as of the prior survey date.

Each job a respondent reports correlates to a unique employer ID. My outcome of interest is whether the individual experiences employment disruption, which I define as no longer working for the same employer as in the previous survey, matched through the employer ID. This indicator variable takes on a value of 1 if no longer employed by the prior employer and 0 otherwise. While this category includes a variety of statuses—unemployed, not in the labor force, working for another employer, or self-employed—it is designed to pinpoint individuals who were formally employed before arrest and experienced separation from their prior employer.

2. Job Quality

The longer-term analysis examines whether nonconviction arrests are associated with diminished job quality over time. Unlike the short-term employment-disruption analysis, these outcomes are not likely to be limited to individuals employed at the time of arrest. Negative credentialing may affect a broad range of workers, including individuals who lose employment following arrest, individuals who remain employed but face reduced mobility in the labor market, and individuals who were not employed at the time of arrest but later seek work.

The primary measure of job quality is the log of hourly wages associated with the respondent's main job. Following common practice in the empirical labor literature, I trim extreme wage observations to reduce the influence of outliers, excluding reported hourly wages below $3 per hour and above $100 per hour. In addition to wages, I examine access to employer-provided benefits, which are an important component of overall compensation and job stability. On average, employee benefits account for approximately thirty percent of civilian workers' total

compensation.[13] Accordingly, I analyze two common forms of employer-provided benefits: medical insurance and employer-funded retirement plans. For each benefit, I create an indicator variable reflecting whether the respondent's employer makes the benefit available to employees. Unemployed individuals are not included in wage and benefit regressions.

C. Control Variables

The regression models include a variety of time-varying characteristics associated with labor-market outcomes. These controls include educational attainment, school enrollment, household composition, and geographic characteristics. Educational controls include indicators for high school completion, college completion, and current school enrollment. Household controls include whether the respondent lives with a spouse or partner and whether the respondent lives with a child under age eighteen. Geographic controls include census region and residence in a rural area.

Control variables are measured in the prior survey period to reduce concerns regarding simultaneity bias and reverse causality, with the exception of geographic controls. I do not include age as a variable because it is closely correlated with year fixed effects, given the low variability in ages among NLSY97 respondents.

D. Empirical Specification

I estimate separate models examining short-term employment disruption and longer-term job-quality outcomes. All models include individual fixed effects and year fixed effects. Standard errors are clustered at the respondent level.

The short-term employment model examines whether nonconviction arrests are associated with employment disruption in the survey period immediately following arrest. The regression is limited to respondents employed in the prior survey period and is estimated as follows:

$$Y_{it} = \alpha_0 + \beta_1(arrested_{[t-1,t]}) + \beta_2 X_{it-1} + \gamma_t + u_i + \varepsilon_{it}$$

[13] U.S. BUREAU OF LABOR STAT., EMPLOYER COSTS FOR EMPLOYEE COMPENSATION — JUNE 2023 (2023), https://www.bls.gov/news.release/pdf/ecec.pdf.

In these models, Y_{it} represents the measure of employment separation, as described above. X_{it-1}represents the vector of lagged control variables described above.

The longer-term job-quality models examine whether nonconviction arrests are associated with diminished wages and employee benefits over time. These models include both a measure of arrest occurring in the current survey period and a measure indicating whether the respondent experienced a prior adult nonconviction arrest. The models are estimated as follows:

$$Y_{it} = \alpha_0 + \beta_1(arrested_{[0,t-1]}) + \beta_2(arrested_{[t-1,t]}) + \beta_3 X_{it-1} + \gamma_t + u_i + \varepsilon_{it}$$

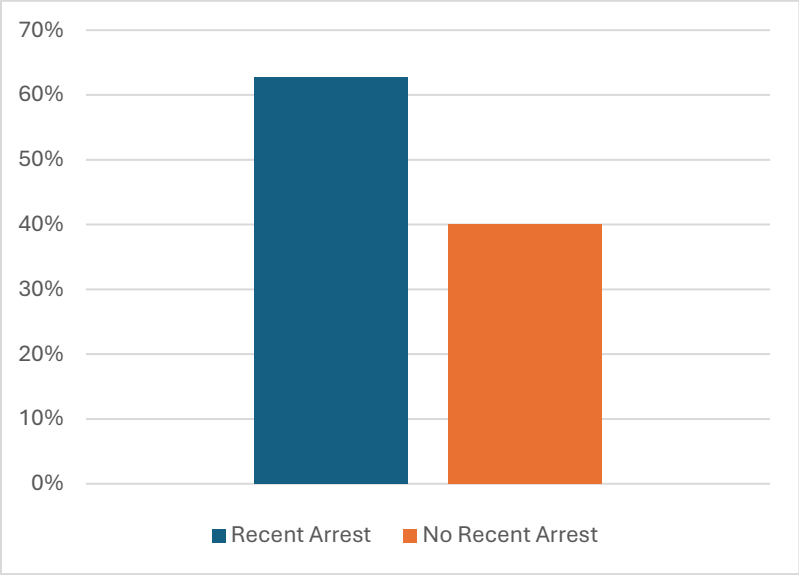
While standard wage equations include control variables for tenure and tenure squared, I do not include measures of tenure as control variables in my main analyses. This is because arrest, through job disruption, is likely to impact tenure as well. Notably, robustness checks that include these variables do not significantly alter the results.

IV. RESULTS

A. Descriptive Statistics

Figure 1 provides employment disruption statistics, broken down by whether respondent reported an arrest in the most recent survey period. In the survey immediately following a nonconviction arrest, 62% of previously employed respondents report employment disruption. In contrast, in survey periods when no arrest occurs, respondents experience employment disruption at a rate of 40%.

FIGURE 1. SHORT-TERM EMPLOYMENT DISRUPTION



Sample includes men who have never experienced conviction who reported formal employment in the prior survey period. All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97.

Table 1 compares averages, as of the last included survey from 2019, across respondents who have never been arrested and those who have experienced an arrest, but never been convicted. Those who have been arrested are less likely to be employed in the formal labor market, full-time or otherwise. Of those who are employed in the formal labor market, prior arrestees earn significantly less than those who have not, in the form of both wages and common employee benefits.

TABLE 1. 2019 DESCRIPTIVE STATISTICS BY ARREST STATUS

Characteristic[14]	Never Arrested	Previously Arrested
Employment Status[15]		
Employed	81.9	68.6
Employed Full Time	76.2	60.3
Job Quality		
Wage	$29.86	$24.38
Retirement Account	82.0	73.3
Medical Insurance	87.1	78.0
Demographics		
White, Non-Hispanic	68.8	58.0
Black, Non-Hispanic	13.1	22.5
Hispanic	12.8	14.6
Other Race	5.3	5.0
Living with Partner	70.5	57.3
Living with Child	55.9	44.3
Education		
Graduated HS (%)	94.9	83.0
Graduated College (%)	46.0	25.5
Sample Makeup		
Unweighted Sample Size	2,037	544
Non-Respondents in 2019	745	163
% Non-Response	26.8	23.1

Sample includes men who have never experienced conviction. All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97.

However, other average characteristics of arrestees vary in important ways from those who have never been arrested. This illustrates why comparing average wages between arrestees and non-arrestees cannot determine whether arrest causes poor labor market outcomes. For instance,

[14] All values reported are percentages, with the exception of wages.
[15] This is limited to employment in the formal labor market. 9.9 percent of the never arrested sample is self-employed, and 13.6 percent of the previously arrested sample is self-employed.

arrestees are significantly less likely to have a high school or college diploma, as well as less likely to be cohabitating with a partner or a child, than their never-arrested counterparts. It is plausible, then, that the employment, wage, and benefit differentials are based on differences underlying characteristics.

B. Regression Results

By controlling for individual fixed effects and many time-varying characteristics that may be associated with the labor market outcomes tested, I can at least partially isolate the relationship between arrest and subsequent labor market outcomes. Table 2 presents regression results, where the outcome of interest is employment disruption.

TABLE 2. SHORT-TERM EMPLOYMENT DISRUPTION

	(1) Employment Disruption
Recent Nonconviction Arrest	0.079***
	(0.024)
High School Graduate	-0.029*
	(0.017)
College Graduate	-0.027*
	(0.014)
Enrolled in School	0.302**
	(0.150)
Child in Household	-0.021*
	(0.012)
Partner in Household	-0.059***
	(0.010)
Lives in Rural Area	0.000
	(0.013)
Observations	26,134
Number of Respondents	2,955

Sample includes men who have never experienced conviction and who reported formal employment in the prior survey period. All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97. ***p<0.01, **p<0.05, *p<0.1. Year fixed effects and controls for region are included. Standard errors clustered at the respondent level in parentheses.

For respondents who were employed in the previous survey period, those who experience nonconviction arrest in the intervening time are approximately 20 percent[16] more likely to experience separation from their previous employer, compared to those who do not experience an arrest during that time.

TABLE 3. LONG-TERM JOB QUALITY

	(1) Ln(Hourly Wage)	(2) Employer-Funded Retirement Account	(3) Employer-Provided Medical Insurance
Prior Nonconviction Arrest	-0.088***	-0.076***	-0.059**
	(0.024)	(0.029)	(0.028)
Recent Nonconviction Arrest	-0.051**	-0.051*	-0.039
	(0.020)	(0.027)	(0.029)
High School Graduate	0.023*	0.059***	0.090***
	(0.013)	(0.018)	(0.019)
College Graduate	0.291***	0.268***	0.306***
	(0.016)	(0.016)	(0.017)
Enrolled in School	-0.057	-0.179	-0.444**
	(0.076)	(0.111)	(0.204)
Child in Household	0.039***	-0.018	-0.020
	(0.011)	(0.013)	(0.013)
Partner in Household	0.073***	0.055***	0.030***
	(0.008)	(0.011)	(0.010)
Lives in Rural Area	-0.013	-0.037**	-0.034**
	(0.011)	(0.015)	(0.014)
Observations	24,885	22,236	22,248
Number of Respondents	2,956	2,883	2,883

Sample includes men who have never experienced conviction and who are formally employed in the current survey period. All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97. ***p<0.01, **p<0.05, *p<0.1. Year fixed effects and controls for region are included. Standard errors clustered at the respondent level in parentheses.

In the period immediately following arrest, there is some indication of reduction in job quality. Hourly wages decrease by approximately 5 percent and the likelihood of employer-funded retirement similarly decreases by approximately 5 percent, with no statistically significant change to medical insurance. Consistent with the theory that job quality declines will take longer to

[16] The average likelihood of a respondent leaving an employer from period-to-period across the entire survey is approximately 40 percent. That means that the 7.9 percentage point coefficient represents approximately a 20 percent increase in employer turnover.

materialize, the decreases in job quality are much stronger in subsequent periods. In the periods *after* the first survey post-arrest, wages decrease 8.4 percent[17], and the likelihood of employer-funded retirement and medical insurance decrease by 7.6 percent and 5.9 percent, respectively.

The decrease in employee benefits appears to largely be driven by a decline in full-time employment, defined as working more than 35 hours per week at a main job. In the period immediately following arrest, respondents are less likely to be employed full time and less likely to be employed at all. While the disparity in likelihood of employment disappears in later periods, there remains a 6.2 percent decrease in the likelihood of being employed full time. And, when limiting the job quality regressions to those who are employed full time, the wage disparity remains but the disparity in benefits disappears. These results are reported in the appendix.

TABLE 4. LONG-TERM EMPLOYMENT STATUS

	(1) Employed	(2) Employed Full Time
Prior Nonconviction Arrest	-0.022	-0.062***
	(0.020)	(0.020)
Recent Nonconviction Arrest	-0.063***	-0.051***
	(0.021)	(0.019)
High School Graduate	0.093***	0.125***
	(0.012)	(0.012)
College Graduate	0.165***	0.355***
	(0.013)	(0.014)
Enrolled in School	0.062	-0.061
	(0.087)	(0.109)
Child in Household	-0.015	-0.024**
	(0.010)	(0.011)
Partner in Household	0.014*	0.054***
	(0.008)	(0.010)
Lives in Rural Area	-0.021**	-0.023**
	(0.010)	(0.011)
Observations	36,621	36,621
Number of Respondents	3,169	3,169

Sample includes men who have never experienced conviction. All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97. ***p<0.01, **p<0.05, *p<0.1. Year fixed effects and controls for region are included. Standard errors clustered at the respondent level in parentheses.

[17] $100\times(e^{-0.088}-1) = -8.42\%$

In sum, nonconviction arrest is associated with job disruption in the short term, and lower wages and likelihood of full-time employment in the longer term, which translates to reduced employee benefits as well.

V. DISCUSSION

The results of this Article suggest that a nonconviction arrest—often treated as a legally minor event—may nevertheless function as a form of punishment through its impact on employment. Utilizing individual fixed-effects models, this study isolates the relationship between nonconviction arrest and labor-market outcomes from stable individual characteristics associated with both arrest and employment disadvantage. The findings indicate that even absent conviction, arrest is associated with substantial short-term employment disruption and longer-term reductions in job quality.

These findings are particularly important because the labor market disadvantages identified arise prior to any formal adjudication of guilt. The criminal legal system ordinarily contains multiple institutional checks before punishment is imposed, including prosecutorial screening, judicial oversight, and trial procedures. Labor-market penalties associated with arrest, by contrast, may be triggered solely by the initial arrest decision itself. In many cases, a single officer's discretionary decision to arrest may generate lasting economic consequences even where charges are never filed or no conviction is obtained.

This dynamic reflects Feeley's classic observation that "the process is the punishment." For individuals arrested for low-level offenses, the burdens imposed by criminal justice processing itself and the creation of an arrest record may exceed any formal sanction ultimately imposed by law.

Several limitations should be noted. Although individual fixed-effects models substantially reduce concerns regarding stable unobserved heterogeneity, observational data cannot fully eliminate selection concerns. In addition, the NLSY97 does not allow precise measurement of the mechanisms underlying employment disruption, including the relative roles of pretrial detention, absenteeism, and employer stigma. Future research using linked administrative employment and criminal justice records may help further isolate these mechanisms.

A. Mechanisms: Employment Disruption and Negative Credentialing

The findings further suggest that arrests affect labor-market outcomes through distinct mechanisms operating across different time horizons. In the short term, the primary consequence of arrest appears to be employment disruption. Men experiencing a recent nonconviction arrest are substantially more likely to separate from their prior employer in the following survey period. This immediate instability is likely driven by absenteeism associated with detention, court appearances, attorney meetings, and other demands of criminal justice processing.

The modern expansion of pretrial detention may amplify these effects. Since the 1980s, the number of individuals held pretrial has increased dramatically, meaning that even legally minor arrests may result in prolonged periods away from work. Existing evidence suggests that even relatively short jail stays substantially increase the likelihood of job loss (Smith 2022). To the extent absenteeism drives arrest-related firings, attempting to alter employer behavior may not fully resolve the problem, as prolonged absences impose genuine operational costs on employers.

In later survey periods, prior nonconviction arrests are associated with lower wages and reduced access to employer-provided benefits, including health insurance and retirement plans. These longer-term disadvantages are likely driven by negative credentialing during future hiring processes. Contemporary employers have broad access to arrest information through commercial background checks and internet searches, allowing even nonconviction arrests to function as enduring negative signals in the labor market.

B. Racial Inequality and Nonconviction Arrest

The labor-market consequences identified in this Article are especially concerning given the racial disparities associated with nonconviction arrests. Existing research suggests that Black men are disproportionately likely to experience arrests that do not ultimately result in conviction (Harrington & Shaffer 2025; Meyers 2022; Tomic & Hakes 2008). Importantly, these disparities appear concentrated in offense categories characterized by substantial police discretion, including drug offenses, public-order offenses, and traffic-related offenses.

High rates of nonconviction among Black arrestees likely reflect over-arrest in the first instance, followed by prosecutorial screening of weak or low-priority cases. As a result, the

economic penalties associated with arrest are likely to fall disproportionately on Black men, even where no formal punishment is ultimately imposed. In this sense, discretionary arrest practices may contribute to cumulative racial inequality in employment and earnings (Jain 2015).

These findings also complicate traditional understandings of criminal punishment. Police officers do not merely initiate criminal proceedings; discretionary arrest decisions may independently trigger significant private-sector penalties through the labor market. In effect, arrest decisions may function as a mechanism through which officers indirectly impose economic punishment outside the formal adjudicative process.

C. Policy Implications

To the extent nonconviction arrest creates labor market disadvantages, those disadvantages should be treated as a meaningful social cost when designing policing and pretrial policies. Arrests impose direct governmental costs through policing, detention, and court administration, as well as risks associated with police-citizen encounters and deprivation of liberty. This Article suggests that policy makers should consider the potential for downstream labor-market harms in that calculus.

These concerns are particularly relevant in the context of aggressive low-level enforcement strategies such as broken windows policing. The underlying premise of broken windows theory is that increased enforcement of minor offenses deters more serious criminal activity (Kelling & Wilson 1982). Yet if arrest-induced job loss weakens attachment to the formal labor market and contributes to future criminal behavior, aggressive low-level arrest strategies may ultimately prove criminogenic rather than crime reducing.

To the extent deterrence can be achieved through less disruptive means, policymakers may wish to rely more heavily on alternatives such as citations, summonses, or diversion programs. Such approaches may preserve many of the administrative and public-safety functions of arrest while reducing the employment disruption associated with custodial processing (Harmon 2016).

In addition, jurisdictions could adopt policies designed to minimize time away from work following arrest. Potential reforms include reducing reliance on cash bail, expanding remote court appearances, and increasing the use of night court proceedings. Because employment stability is strongly associated with reduced criminal activity, policies that preserve labor-market attachment may generate both economic and public-safety benefits.

VI. Conclusion

Substantial labor-market disadvantages appear to follow arrest, absent any criminal conviction. And those penalties are likely disproportionately borne by Black men because of racial disparities in discretionary policing and arrest practices. These findings suggest that arrest itself—not just conviction—may function as a significant form of punishment, one that may undermine public safety by destabilizing employment. If arrest-related job loss contributes to future criminal activity, then aggressive low-level arrest strategies such as broken windows policing may be counterproductive in the long run. Policymakers should therefore consider labor-market consequences of arrest as an important social cost when designing policing, bail, and pretrial procedures, and should consider less disruptive alternatives such as citations, summonses, and reforms that reduce unnecessary pretrial detention.

Declarations

A. Funding

This research received no specific funding.

B. Ethics Approval

Not applicable. This study relies exclusively on secondary analysis of de-identified data from the National Longitudinal Survey of Youth 1997 (NLSY97).

C. Clinical Trial Registration

Not applicable.

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APPENDIX

TABLE A1. LONG-TERM JOB QUALITY: FULL TIME EMPLOYMENT ONLY

	(1) Ln(Hourly Wage)	(2) Employer-Funded Retirement Account	(3) Employer-Provided Medical Insurance
Prior Nonconviction Arrest	-0.077***	-0.026	-0.024
	(0.029)	(0.038)	(0.030)
Recent Nonconviction Arrest	-0.056**	-0.016	-0.015
	(0.023)	(0.035)	(0.033)
High School Graduate	0.015	0.088***	0.063**
	(0.017)	(0.028)	(0.028)
College Graduate	0.203***	0.161***	0.137***
	(0.018)	(0.023)	(0.021)
Enrolled in School	-0.010	-0.045	-0.352
	(0.095)	(0.181)	(0.220)
Child in Household	0.031***	-0.021	-0.016
	(0.011)	(0.014)	(0.012)
Partner in Household	0.058***	0.033***	0.005
	(0.008)	(0.012)	(0.010)
Lives in Rural Area	-0.010	-0.031*	-0.017
	(0.012)	(0.016)	(0.013)
Observations	18,736	16,944	16,944
Number of Respondents	2,681	2,602	2,602

Sample includes men who have never experienced conviction and who are formally employed full time in the current survey period. All values are calculated using the NLSY sample weights. https://www.nlsinfo.org/weights/nlsy97. ***p<0.01, **p<0.05, *p<0.1. Year fixed effects and controls for region are included. Standard errors clustered at the respondent level in parentheses.