PART III

Consequences of Criminal Justice Contact

Level of Criminal Justice Contact and Early Adult Wage Inequality

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This study explores heterogeneity in the relationship between criminal justice contact and early adult wages using unconditional quantile regression models with sibling fixed effects, estimated separately by race-ethnicity. The findings support the contention that the relationship between criminal justice contact and wages is heterogeneous in three respects: level of contact, race, and location in the wage distribution. First, entry-level contacts in the form of arrest are largely uncorrelated with wages, whereas wage gaps are evident following late-stage contacts in the form of jail or prison incarceration. Second, the wage gap from incarceration is observable among black respondents, but not whites or Latinos. Third, the size of the wage gap from incarceration is approximately U-shaped with respect to the black wage distribution.

In the span of a generation, the criminal justice system metamorphosed into an unprecedented form of state intervention in American life, reaching a scale unmatched by any other society or any other time (Garland 2001a). Imprisonment growth is the most frequently noted and studied symptom of this phenomenon (Garland 2001b; Kirk and Wakefield 2018; Wakefield and Wildeman 2014). The trend is also obvious in noncarceral forms of criminal justice contact, such as those that entail supervision without secure custodial confinement (Phelps 2017).

The contemporary criminal justice system is also notable for its concentration of young men of color (Patterson and Wildeman 2015; Pettit 2012; Pettit and Western 2004; Shannon et al. 2017). This persistent disparity, apparent across all levels of the system, is only partly explained by differences in the frequency or level of criminal offending across racial-ethnic groups (see, for example, Gelman, Fagan, and Kiss 2007; Sampson and Lauritsen 1997). Paired with the system's unparalleled scale, this racial disparity seems to leave little doubt that it is today more of a major vehicle of contemporary social stratification than it historically has been (Wakefield and Uggen 2010).

In this study, we examine the relationship of criminal justice contact with the early adult wages of a large representative sample using a method new to the study of punishment and

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inequality. We measure different stages of criminal justice contact to determine the degree to which the level of contact is correlated with wages.[1] We also estimate race- and ethnicity-specific models to study whether criminal justice contacts have uniform or distinct effects across sociodemographic groups, which is particularly important in light of the social patterning of criminal justice contact. Finally, we estimate (unconditional) quantile regression models to document heterogeneity in the relationship between criminal justice contact and wages. Taken together, our results point to level-specific effects and distinct racial patterning of early adult wage inequality following criminal justice contact.

RESEARCH ON CRIMINAL JUSTICE CONTACT AND WAGE INEQUALITY

Spanning several decades and countries, an extensive literature documents the impact of criminal justice contact on the labor market. This rich research tradition is mixed with respect to legal jurisdictions, types of contact, age and representativeness of the samples, measurement sources, research designs, and methodological rigor. We refer readers to more comprehensive summaries of employment-related consequences available elsewhere, and focus attention here on the outcomes most germane to our study—wages and earnings (see Apel and Ramakers, forthcoming; Kirk and Wakefield 2018; Travis, Western, and Redburn 2014; Raphael 2014; Wakefield and Uggen 2010).

Research on the relationship between criminal justice contact and wages is not as uniform as one might first suspect. For example, Ross Matsueda and his colleagues do not find any difference in earnings between individuals in the Supported Work evaluation who were formerly incarcerated versus formerly addicted to drugs (1992). They find instead that formerly incarcerated individuals are more likely to earn income illegally. The authors also estimate an inverse relationship between the number of prior arrests and earnings, but an unexpectedly

positive relationship for the number of prior weeks in jail. Karen Needels also does not find any relationship between incarceration and earnings among men in the Transitional Aid Research Project evaluation, although she does find that the number of arrests is inversely correlated with long-term earnings (1996).

In Jeffrey Grogger's panel study of individuals arrested in California, arrest is correlated with a modest 4 percent earnings penalty that declines and then disappears after the fourth quarter, whereas conviction is uncorrelated with earnings, and the coefficients are even positive (1995). By comparison, jail incarceration corresponds with a 16 percent earnings decline and prison incarceration a 22 percent decline, both of which persist for at least six quarters. Joel Waldfogel's sample of men convicted for the first time in the federal criminal justice system experience earnings erosion when they are convicted, as well as when they are incarcerated (1994a, 1994b). The effects are particularly large for higher-status workers who are better educated and whose occupations require more trust, indicating that the effects of criminal justice contact is status dependent, to some degree. Daniel Nagin and Waldfogel find that conviction is actually correlated with 10 percent higher earnings among London-area men, controlling for self-report delinquency and crime, and explain this unexpected finding by arguing that criminal conviction relegates individuals to spot market jobs which are high-paying but unstable in the long run (1995, 1998).

Research using state administrative data sources reveals an unexpectedly positive correlation between time served in prison and postrelease earnings. For example, Jeffrey Kling observes that formerly incarcerated men in Florida who serve longer prison terms have initially higher earnings than their counterparts who serve shorter terms (2006). However, two years after incarceration, the differences disappear. Similar findings are reported in Illinois (Jung 2011). Additional evidence from Washington indicates that the earnings of formerly

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1. To speak of stages of the criminal justice process can be misleading, because it implies a degree of coordination between justice personnel and justice institutions that simply does not exist. While it possesses a distinctly progressive structure, whereby downstream decision-making is influenced by upstream decision-making, the criminal justice process more closely resembles a series of administrative filters than stages.

incarcerated black men grow 21 percent more slowly following release than their white counterparts, contributing to "compound disadvantage" (Lyons and Pettit 2011).

Until recently, the National Longitudinal Survey of Youth 1979 (NLSY79) has been the only large-scale, self-report survey permitting study of the long-term relationship between criminal justice contact and wages in a representative sample.[2] Jeffrey Fagan and Richard Freeman show a consistent inverse correlation between incarceration and earnings (1999; on null effects for earlier interviews, see Bound and Freeman 1992). In a panel study, Bruce Western reports that prior incarceration corresponds with a reduction in wages of about 16 percent relative to non-incarcerated individuals (2002). He also finds that incarceration deflects individuals onto a flatter wage profile, slowing wage growth by 31 percent relative to high-risk men who are never incarcerated. Models estimated separately for white, black, and Hispanic respondents indicate more or less uniform deceleration in wage growth following incarceration. Steven Raphael reports a wage gap of about 15 percent following incarceration, although in his most stringent test (restriction of the sample only to individuals who have been or will be incarcerated), the incarceration-wage correlation disappears (2007). Haeil Jung finds that both youth and adult incarceration are correlated with reductions in adult wages (2015).

Amanda Geller and her colleagues report from the Fragile Families and Child Wellbeing study that men who have ever been incarcerated possess a wage rate 9 to 22 percent lower than their non-incarcerated peers, depending on the model specification (2006). However, sensitivity analysis also indicates the results are not robust to unobserved confounding. Robert Apel and Gary Sweeten, using the 1997 cohort of the National Longitudinal Survey of Youth, report a nonsignificant wage gap of about 9 percent among those who are incarcerated following their first criminal conviction, compared to their similarly first-time convicted peers who are not incarcerated (2010).

To summarize, the nature of the relationship between criminal justice contact and wages remains in doubt. Evidence exists that arrest is inversely correlated with wages, but for either a short period of time or a long period of time. Evidence also exists that conviction has no relationship with wages, that it is inversely correlated with wages, and that it is positively correlated with wages. Additional evidence indicates that incarceration has no relationship with wages, that it is inversely correlated with wages, and that incarceration (length) is positively correlated with wages. It would be seriously mistaken to conclude from this body of research that the correlation between criminal justice contact and wages has been firmly established.

MECHANISMS UNDERLYING CRIMINAL JUSTICE CONTACT AND WAGE INEQUALITY

Three prominent mechanisms are usually invoked to explain why criminal justice contact might be correlated with wages (see also Western, Kling, and Weiman 2001). We consider differential selection, labor demand, and labor supply in turn. However, these mechanisms are more relevant to understanding the correlation between conviction or incarceration and wages, involving decision-making in the court system, but less obviously applicable to understanding entry-level contacts such as arrests.

Differential Selection

Selection mechanisms are implicated if individuals who experience criminal justice contact would have had lower wages or experienced slowed wage growth even in the absence of contact. Criminal justice contact is more heavily concentrated among individuals occupying the lowest rungs of the social ladder. Incarceration, in particular, resembles a sorting mechanism that absorbs socially marginal populations (Wakefield and Uggen 2010).[3] For example, just

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2. This advantage is offset by the fact that, aside from a self-report crime and criminal justice module administered in the 1980 interview, the only form of criminal justice contact that is possible to measure is whether the interview was conducted in a correctional institution.

3. "Prisons . . . house the jobless, the poor, the racial minority, and the uneducated, not the merely criminal" (393).

32 percent of state prison inmates and 39 percent of local jail inmates have a high school diploma, compared with about 82 percent of the general population and even 58 percent of probationers (Harlow 2003, table 1). Furthermore, in the month prior to their arrest, 53 percent of state prison inmates take home less than $1,000 ($1,525 in 2017 dollars, an annualized equivalent of $18,300), and 25 percent live with someone who receives welfare (Harlow 2003, table 14).

An additional source of selection into the criminal justice system is undoubtedly criminal offending. Simply put, individuals who are more criminally active are more exposed to criminal justice contact, other things equal. Furthermore, a higher volume of police contacts and arrests is correlated with subsequent criminal justice processing. Additionally, a defendant's current offense and criminal history account for the lion's share of variation in judicial sentencing. That said, criminologists have long been aware that, although legal variables tend to be the most salient determinants of criminal justice processing, extralegal variables frequently impinge on criminal justice decision-making, especially at times when officials are entitled to more discretion. For example, Robert Sampson finds that black youth and youth from low-status neighborhoods accumulate significantly more police contacts, net of several forms of delinquent behavior, and a higher volume of police contacts is then highly correlated with court referral (1986; see also Sampson and Laub 1993). The influence of neighborhoods is partly "ecological contamination," as police departments adopt more legalistic practices in low-status and minority neighborhoods (Smith 1986). Yet even in the court system, young black males and individuals from low-status families tend to be subjected to more punishment than can be explained by legally relevant variables alone (Sampson 1986; Steffensmeier, Ulmer, and Kramer 1998).

Labor Demand

Demand-side mechanisms focus on the willingness of employers to knowingly hire individuals with a history of criminal justice contact. The analytical focus is on employers as decision-makers and gatekeepers: criminal justice contact is a stigma that, in the eyes of employers, makes job applicants unemployable or at least undesirable. This mechanism is corroborated by experimental audits and correspondence tests.[4] Devah Pager's studies of entry-level job openings document a callback rate of formerly incarcerated individuals that is just one-half the size, from a 25 to 28 percent baseline, of the rate among their peers with no incarceration (Pager 2003, 2005, 2007; Pager, Western, and Bonikowski 2009; Pager, Western, and Sugie 2009; see also Decker et al. 2015).[5] Formerly incarcerated black applicants experience even larger disparities. Specifically, black applicants without a prison record have a similar callback rate to white applicants with a prison record—being black and formerly incarcerated thus constitutes "double jeopardy" in low-wage labor markets (Pager 2005, 2007). Aside from their categorical exclusion at the point of the decision to hire, further evidence indicates post-hiring, race-coded job channeling whereby blacks are placed into lower-prestige occupations (Pager, Western, and Bonikowski 2009).

Employers also appear to make hiring decisions on the basis of noncarceral contacts

4. In a typical study, a pair of applicants, known as auditors or testers, applies for the same job. Relevant background characteristics of the pair (such as gender, race, education, and work history) are matched as best as possible while the key characteristic under study—possession of some kind of criminal history—is randomly varied between the testers. In an audit study, the auditors apply in person for posted job openings, whereas in a correspondence study, résumés or applications with fictitious credentials are submitted. The outcome in either kind of study is the callback, or any form of favorable follow-up from an employer (such as offer of hire, invitation for an interview, or solicitation of more information).

5. Sarah Galgano reports on a correspondence study using female testers in Chicago (2009). She does not observe any difference in callback rates, suggesting that "a criminal history is not as universally stigmatizing for women" (33). Scott Decker and his colleagues also do not find any difference in callback rates in the correspondence portion of their study but do find differences in the audit portion (2015).

when that information is available. Richard Schwartz and Jerome Skolnick report a lower callback rate for conviction, relative to employment files with no criminal record (1962). Even employment files with a trial and acquittal—an applicant who is criminally accused but proclaimed to be without guilt—show lower callback. Christopher Uggen and his colleagues further report that employers incorporate arrests into their hiring decisions, in that applications indicating an arrest receive a callback 29 percent of the time relative to a baseline of 33 percent (2014; see also Vuolo, Lageson, and Uggen 2017). Thus, even individuals who have a minor brush with the law can be stigmatized if potential employers find out about it.[6]

Labor Supply

Supply-side mechanisms emphasize the training and credentials possessed by job seekers that make them more or less attractive hires to potential employers. Education and work experience are crucial components of supply-side explanations. A number of studies indicate that youthful criminal justice contact is correlated with schooling deficits (Bernburg and Krohn 2003; Hirschfield 2009; Hjalmarsson 2008; Kirk and Sampson 2013; Sweeten 2006; Widdowson, Siennick, and Hay 2016). These studies are mixed as to whether the correlation between arrest and schooling withstands rigorous selection controls, but intermediate and especially later stages of criminal justice contact—namely, court involvement and incarceration—are strongly correlated with high school non-completion.

Criminal justice contact is also correlated with nonwage facets of an individual's work experience that can translate into later wage gaps. Apel and Sweeten report that, among individuals convicted for the first time, those who are incarcerated are subsequently less likely to be employed and work fewer weeks when they are employed (2010). This work experience gap is accounted for largely by labor force nonparticipation, which is a form of work detachment that can worsen long-term employment prospects.[7] A similar phenomenon has recently been reported among formerly incarcerated individuals in Boston, who experience idleness in the weeks following their return to the community—they are neither working nor looking for work (Western et al. 2015). Criminal justice contact can therefore contribute to a spotty work record because of detachment from work, beyond any time out of the labor market due to confinement (Holzer, Raphael, and Stoll 2006).[8]

CRIMINAL JUSTICE CONTACT AND DISTRIBUTIONAL HETEROGENEITY

Research on the consequences of criminal justice contact has focused largely on estimation of differences in (regression-adjusted) mean outcomes, but recognition is growing of the need to unpack average effects to better understand the consequences of criminal justice contact for social inequality (Kirk and Wakefield 2018; Sampson 2011; Wakefield and Wildeman 2014). Only a handful of studies address heterogeneity in outcomes among justice-involved individuals, finding that some precontact characteristics moderate postcontact outcomes in

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6. On employer use of criminal background checks, see Holzer 1996; Holzer et al. 1996; Stoll and Bushway 2008; Stoll 2009. On policies that prevent employers from inquiring about criminal histories on job applications (such as Ban the Box), see Agan and Starr 2018.

7. Labor force nonparticipants include stay-at-home parents, school-going youth, retirees, and disabled persons. They also include discouraged workers, or those individuals who have given up looking for work. Labor force nonparticipation is different from unemployment, which presumes that an individual is actively seeking work but has not yet been hired (such as having recently filled out a job application or gone on a job interview).

8. Harry Holzer and his colleagues report that 96 percent of employers will hire applicants with only a GED, applicants who are former welfare recipients (92 percent), applicants who have been unemployed for a year or more (83 percent), applicants with a "spotty work record" (59 percent), and applicants with a criminal record (38 percent) (2006). Even if employers lack access to criminal history information, therefore, formerly incarcerated job applicants are quite likely to experience hiring difficulty simply because of the spotty record caused by incarceration-induced work history gaps.

interesting ways. For example, a pair of recent child well-being studies demonstrate that parental incarceration is most harmful to well-being among children from comparatively advantaged family environments—those for whom the confinement of a parent is likely to result in a more substantial, and unexpected, change to the family milieu (Turney 2017; Turney and Wildeman 2015). Sara Wakefield and Kathleen Powell report that children of incarcerated fathers with severe substance abuse problems prior to their confinement exhibit less aggression compared to children of substance-abusing fathers who are not incarcerated (2016). Christopher Dennison and Stephen Demuth find that individuals from high-status backgrounds experience downward mobility following deeper criminal justice contact relative to their low-status counterparts (2018).

Although researchers typically focus on differences in means, differences in other distributional quantities (such as percentiles) are frequently insinuated in inequality scholarship. Inattention to this distributional heterogeneity is especially problematic for the study of an institution—and the criminal justice system is one prominent institution—that more or less routinely interfaces with highly disadvantaged populations. However, predictions diverge about the nature of the relationship between criminal justice contact and wage inequality. For comparatively low-wage workers, criminal justice contact might not have any effect discernible from other facets of their lives that already situate them in a highly disadvantaged milieu. Wages might be similarly inelastic with respect to criminal justice contact among comparatively high-wage workers, who benefit from more privileged social contexts (but see Waldfogel 1994a, 1994b). Alternatively, criminal justice contact (carceral contact, in particular) might further entrench wage inequality among low-wage workers, and at the higher end of the continuum, create wage inequality where it might not have otherwise existed.

We believe a study of distributional heterogeneity more closely aligns with scholarly interest in criminal justice contact as a possible mainspring of wage inequality. We thus propose the unconditional quantile regression model to probe the relationship between criminal justice contact and hourly wage. We explore heterogeneity through estimation of the model at all wage percentiles between the 5th and the 95th. Because of the salience of race-ethnicity in the criminal justice system as well as in the population wage distribution, we estimate the models separately for white, Latino, and black respondents. We also explore different levels of criminal justice contact, and though we focus our attention on only arrest and incarceration for collinearity reasons, we comment on intermediate forms of contact at relevant points.

DATA

The data used for this study come from the National Longitudinal Survey of Youth 1997 (NLSY97), a nationally representative sample of about nine thousand American youth born between 1980 and 1984 (Bureau of Labor Statistics 2015). Funded by the Bureau of Labor Statistics and fielded by the National Opinion Research Center, the first round of the NLSY97 was administered in 1997 and 1998, when respondents were between ages twelve and eighteen. To date, seventeen rounds of data are available, which effectively span ages twelve to thirty-six inclusive. The first fifteen rounds were conducted annually; as of the sixteenth, the survey is biennial. One distinct advantage of the NLSY97 is its goal to document the transition from school to work in a contemporary sample of young people, which means that it provides a broad array of measures related to employment and attainment outcomes. A second distinct advantage, essential for this study, is that the survey regularly inquires about forms of criminal justice contact that transpire between interviews.

The objective of this study is to compare the early adult wage distribution of individuals with a history of criminal justice contact to their counterparts—specifically, their sibling or siblings—with no reported contacts. For each respondent, the last available round is selected for analysis. In most cases, this is the seventeenth round, but for 30 percent of respondents, the last available interview is from an earlier round on account of attrition. The median respondent is 32 years of age at the last available interview.

Hourly Wage

Descriptive statistics for all measures are provided in table A1. The dependent variable is the hourly wage reported in a formal job at the last available interview. In the NLSY97, a formal job is an "employee-type job" defined as "a situation in which the respondent has an ongoing relationship with a specific employer" (Center for Human Resource Research 2002, 96). The hourly wage in 2016 dollars is calculated by pooling wage information across all jobs worked within the reference window, thus assigning heavier weight to longer-duration jobs.[9] At the last available interview, 87.7 percent of all respondents are employed, and the mean wage is higher than $19 per hour (median = $15.68). Respondents who are not employed are excluded from the regression models.

Criminal Justice Contact

The independent variables of interest are forms of criminal justice contact. In each round, NLSY97 survey staff ask respondents a series of questions to ascertain whether, since the last interview, they had been arrested, charged (if arrested), court involved (if charged), convicted (if court involved), and sentenced (if convicted). Respondents who report having been sentenced are then asked whether they served a sentence in a juvenile correctional institution, reform or training school, jail, or adult correctional institution (prison). Those who report institutional confinement are further asked to provide the month and year of entry and exit. The type of incarceration of most interest in this study is that which takes place in either jail or prison, although for the handful of respondents for whom it applies, incarceration in a juvenile correctional institution or a reform or training school is treated as a control variable.[10]

These questions are used to create measures of two distinct types of criminal justice contact: arrests and incarceration spells, the latter being jail or prison.[11] Each is measured using a dummy variable to indicate whether, as of the last available interview, respondents had ever reported that type of contact. These represent accumulative indicators of noncarceral and carceral contacts and are the key measures in the empirical models. More than one-third (35 per-

9. To account for the fact that respondents may report more than one job within a reference window, we incorporate job weights. The weight is constructed as the number of weeks worked in job j, divided by the sum of the total number of weeks worked across all K jobs:

$$Weight_j = \frac{Weeks_j}{\sum_{j=1}^{K} Weeks_j}.$$

The denominator is not the total number of calendar weeks worked, but the sum of the number of calendar weeks worked in all jobs, that is, the sum of K job durations. By construction, the job weights sum to unity at each interview. Each reported wage is multiplied by its corresponding job weight; these are then summed across all jobs reported within a reference window.

10. Supplementary information on incarceration can be obtained from at least three additional sources: the location where the respondent's interview was conducted (dormitory, prison, or hospital), the type of dwelling in which the respondent resides as of the interview (jail, prison, detention, or work release), or the type of interview conducted or else the reason that a respondent was not interviewed (interview completed in person or by phone when respondent was incarcerated; not interviewed because of inaccessibility due to imprisonment). However, none of these supplementary sources of information accommodates a clear distinction between jail and prison, so this information is not used.

11. Because the intermediate measures of criminal justice contact (such as charges, convictions) are highly correlated with arrest, they cannot be included simultaneously in a cross-sectional analysis. However, we comment at relevant points on sensitivity analyses which include these measures in place of arrest. Jail and prison spells refer to postconviction sentences (that is, sanctions), rather than post-arrest or pretrial detentions. In other words, in this study, carceral contact should be taken to mean that an individual was punished for a criminal offense with a sentence to a correctional facility.

cent) of the sample has ever been arrested, a figure that aligns with what has been reported elsewhere from the NLSY97 (Brame et al. 2012, 2014). At the "deep end" of the criminal justice system, almost one in ten (9.8 percent) has been sentenced to jail or prison. Among those who have been incarcerated in prison, exactly half have also been incarcerated in jail on a different (usually prior) occasion.

In addition to dummy indicators for criminal justice contact prevalence, two additional measures are created. First, the accumulated frequency of arrests and incarceration spells, as well as the accumulated duration of jail and prison incarceration are obtained by summing the relevant information across all interviews from the first to the last available. These serve as alternative measures of criminal justice contact, used to ascertain whether discrete *stage effects* stem from single contacts or instead *accumulation effects* flow from repeated contacts. Second, the number of years elapsed since the first reported contact is measured based on the respondent's age as of the interviews in which the first arrest and incarceration spell are reported. These are included to determine whether the effects of criminal justice contact fade or grow over time.

Control Variables

The regression models control for a number of other variables, including gender, age at interview (dummy coded), marital status (never married, currently married, currently divorced or separated), cohabitation, dwelling type (house, apartment, other dwelling type), urbanicity (central city, suburb, outside MSA), census region, and number of years in the interview reference window. As mentioned, having ever been incarcerated in a juvenile correctional institution, reform school, or training school is also included as a control variable. We do not include employment- and education-related controls to avoid so-called collider variables that are likely to mediate the relationship between criminal justice contact and early adult wages (Elwert and Winship 2014).

METHODS

We focus here on the intuition underlying the approach (for technical methodological details, see the appendix). In light of our interest in the study of inequality as a distributional phenomenon, the method of choice is the unconditional quantile regression model (Firpo 2007; Firpo, Fortin, and Lemieux 2009). Because the criminal justice contact measures are binary, the model lends itself to estimation of the impact of increasing the proportion of arrested or incarcerated individuals on the marginal or unconditional wage distribution. Rather than yielding a single overall (and possibly unrepresentative) estimate of the impact of criminal justice contact, this approach considers whether criminal justice contact has heterogeneous effects, that is, different effects at different points in the wage distribution. We estimate all models from the 5th to the 95th percentiles of the wage distribution.

To incorporate a quasi-experimental design element that deals with certain forms of selection bias, we estimate the unconditional quantile regression models with fixed effects for siblings. The design of the NLSY97 involved interviewing all age-eligible residents in each sampled household, where age eligibility was defined by birth year (1980–1984).[12] The analytic sample is thus limited to the roughly four thousand respondents (about 45 percent of the sample) that have one or more coresident siblings. Identification of the model derives from a comparison of respondents who have ever experienced criminal justice contact to one or more similar-age siblings who have never experienced contact, which provides a strong control for early neighborhood and family environment as well as social class background, all of which are important correlates of criminal justice contact and early adult wages.

Because wages differ considerably by race and ethnicity, separate models are estimated for white, Latino, and black respondents. Additionally, as a supplement to tests of statistical significance, we use effect-size calculations to judge substantive significance using Cohen's *d* (Cohen 1988). The effect size is routinely judged

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12. Multiple-respondent households comprise as many as five respondents. Although coresident interviewees are most likely to be siblings, they are not universally so.

Figure 1. Density of Hourly Wage
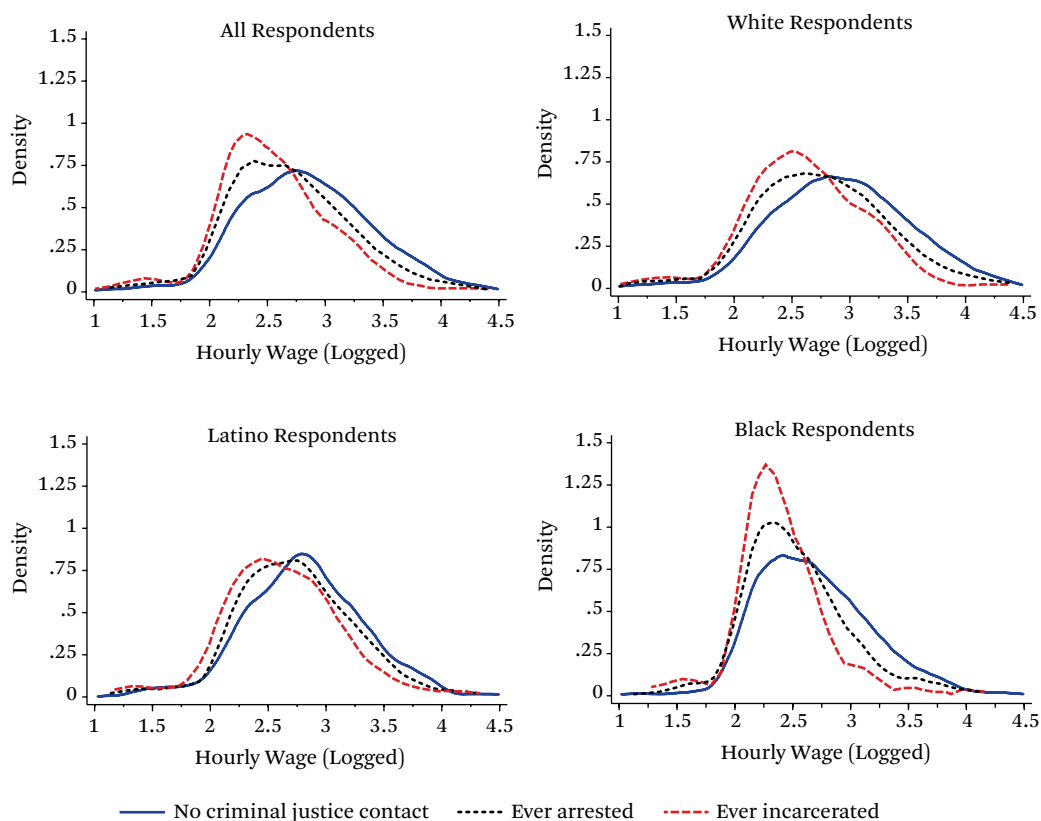
Source: Authors' estimates from respondents' last available round of the National Longitudinal Survey of Youth 1997, rounds 1–17 (Bureau of Labor Statistics 2015).
Note: Estimates are unweighted. The distributions derive from the full sample, not the sibling subsample. The criminal justice contact groups are not mutually exclusive.

against a minimum threshold of 0.20, below which an effect size is generally regarded as uninteresting in terms of practical significance, even if the coefficient is statistically significant.

RESULTS

Figure 1 provides provisional evidence about the nature of the correlation between criminal justice contact and early adult wages, for both the full sample and the race-ethnicity–specific subsamples. The density of log hourly wages is graphed for respondents who have never experienced criminal justice contact, those who have ever been arrested, and those who have ever been incarcerated (for the cumulative distribution of log hourly wages, see figure A1). Individuals with a history of criminal justice

contact exhibit wage distributions that are shifted leftward relative to the normative, no-contact distribution. Level of criminal justice contact and wages are also correlated, as indicated by the fact that the wages of individuals who have been incarcerated are consistently lower relative to those who have been arrested.

Figure 1 also documents racial-ethnic differences in the wage distributions as well as in the magnitude of the wage gap for respondents with a history of criminal justice contact. Black respondents with a history of incarceration, in particular, exhibit a visually striking deviation from the no-contact wage distribution relative to their incarcerated white and Latino peers. This is compounded by the fact that the wage distribution of black respondents is noticeably

lower than their white and Latino peers to begin with. On its face, this harmonizes with Pager's observations concerning the double jeopardy African Americans with a criminal history experience in low-wage labor markets (2005).

In the empirical analysis, we probe the information conveyed by the wage distributions just shown, to examine whether the noted patterns persist when we account for additional variables. Table 1 provides select estimates of the relationship between criminal justice contact and early adult wages from unconditional quantile regression models with sibling fixed effects. The inclusion of sibling fixed effects means that individuals who have experienced criminal justice contact are compared with their similar-age siblings who have never experienced criminal justice contact. Although it is not necessary that wages are logged for this analysis, doing so means the coefficients are approximate proportional differences in the hourly wage at a given marginal quantile.[13] The reference group for the arrest and incarceration coefficients is respondents who have never experienced criminal justice contact.

The first finding of interest is that, with few exceptions, individuals who have been arrested do not differ in their early adult wages from similar-age siblings who have never been arrested. Some coefficients are even positive in sign, though never close to statistical significance. The only indications of a relationship between arrest and hourly wages are a pair of coefficients which are marginally significant ($p < .10$), one at the 50th percentile among all respondents and another at the 90th percentile among black respondents.

The second finding of interest is that incarceration is correlated with early adult wages among black but not white or Latino respondents. Among black respondents, three of five quantile regression coefficients are significant using a .05 criterion (and one more is significant using a .10 criterion), and it is

notable that these three coefficients also differ from their white counterparts ($p < .10$). For example, evaluating at the 25th percentile, blacks who have ever been incarcerated earn a 28 percent lower wage ($e^{-0.33} - 1 = -0.28$) than their similar-age siblings who have never experienced criminal justice contact. Evaluating at the 90th percentile, the wage penalty is 42 percent ($e^{-0.54} - 1 = -0.42$).[14]

Figures 2 and 3 provide the full suite of quantile regression estimates and confidence intervals for arrest (figure 2) and incarceration (figure 3), spanning the wage distribution from the 5th to the 95th percentiles. With respect to arrest, and confirming the impression from the results reported in table 1, no obvious wage disparity is evident between individuals with an arrest record and their counterparts without an arrest record. Above the 85th percentile, the quantile regression estimates exceed minimum effect-size thresholds, but interestingly, the coefficients for whites are positive but negative for Latinos and blacks. However, fewer coefficients are statistically significant than what would be expected merely by chance.

Concerning incarceration, the estimates for whites and Latinos also confirm the prior impression of null findings from table 1. Indeed, not a single estimate is statistically significant at any conventional level, and virtually all are within the bounds of a substantively null effect size. For black respondents, on the other hand, 71 percent of the quantile regression estimates are statistically significant using a .05 criterion (and 87 percent are significant using a .10 criterion). Below the 60th percentile, the effect sizes are small but substantively meaningful (that is, $|d| \geq 0.20$), whereas above the 60th percentile, the effect sizes are well within the medium range (that is, $|d| \geq 0.50$). Although it is not shown, the incarceration coefficients frequently differ at the 10 percent significance level from the arrest coefficients, indicating that carceral contacts have additive effects on

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13. The interpretation of coefficients as proportional differences is a convenient approximation, but the approximation is overestimated when coefficients are larger than ±0.10. Instead, the transformation $e^b - 1$ yields the technically correct proportional difference in the hourly wage at a given marginal quantile.

14. At the urging of an anonymous reviewer, in a sensitivity analysis we restricted the analytic sample to respondents from households with at least one same-sex sibling. Despite the fact that this reduced the sample from 4,035 to 2,296, all results were replicated, and in fact, the coefficients tended to be larger in magnitude.

Table 1. Select Quantile Regression Estimates of the Difference in Hourly Wage

Percentile	Quantile Wage	Ever Arrested Coeff. (SE)	Ever Incarcerated Coeff. (SE)	Quantile Wage	Ever Arrested Coeff. (SE)	Ever Incarcerated Coeff. (SE)
	(A) All Respondents (N = 3,249)			(B) White Respondents (N = 1,696)		
10th	$8.25	-0.04 (0.05)	-0.15 (0.09)+	$8.41	-0.08 (0.09)	0.01 (0.17)
25th	$10.81	-0.02 (0.05)	-0.10 (0.08)	$11.73	-0.12 (0.09)	0.00 (0.13)
50th	$15.75	-0.08 (0.04)+	-0.20 (0.07)**	$17.98	0.01 (0.07)	-0.08 (0.12)
75th	$23.61	-0.07 (0.05)	-0.19 (0.08)*	$26.74	0.10 (0.08)	-0.02 (0.10)
90th	$35.44	0.05 (0.08)	-0.08 (0.10)	$40.25	0.13 (0.10)	-0.08 (0.12)
Linear (fixed)	—	-0.02 (0.04)	-0.18 (0.08)*	—	0.02 (0.06)	-0.06 (0.09)
	(C) Latino Respondents (N = 768)			(D) Black Respondents (N = 785)		
10th	$9.18	0.11 (0.11)	0.03 (0.17)	$7.74	-0.08 (0.09)	-0.23 (0.15)
25th	$11.60	0.01 (0.10)	-0.02 (0.15)	$9.43	-0.01 (0.09)	-0.33 (0.13)**
50th	$16.00	-0.05 (0.10)	-0.08 (0.13)	$12.50	0.04 (0.08)	-0.22 (0.12)+
75th	$22.64	-0.19 (0.14)	-0.14 (0.18)	$17.66	-0.10 (0.10)	-0.34 (0.15)*
90th	$31.65	-0.35 (0.21)	-0.11 (0.24)	$26.17	-0.24 (0.14)+	-0.54 (0.23)*
Linear (fixed)	—	-0.00 (0.12)	0.00 (0.22)	—	-0.15 (0.08)+	-0.52 (0.13)***

Source: Authors' estimates from respondents' last available round of the National Longitudinal Survey of Youth 1997, rounds 1-17 (Bureau of Labor Statistics 2015).

Note: Estimates are unweighted. The coefficients derive from (unconditional) quantile regression models of log hourly wage with sibling fixed effects. The coefficients for control variables are not shown. Cluster-robust standard errors are reported, and are obtained from the bootstrap with 250 replications. For comparative purposes, coefficients from linear regression models (of the mean) with sibling fixed effects are also shown.

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

Figure 2. Full Quantile Regression Estimates of the Relationship Between Arrest and Hourly Wage
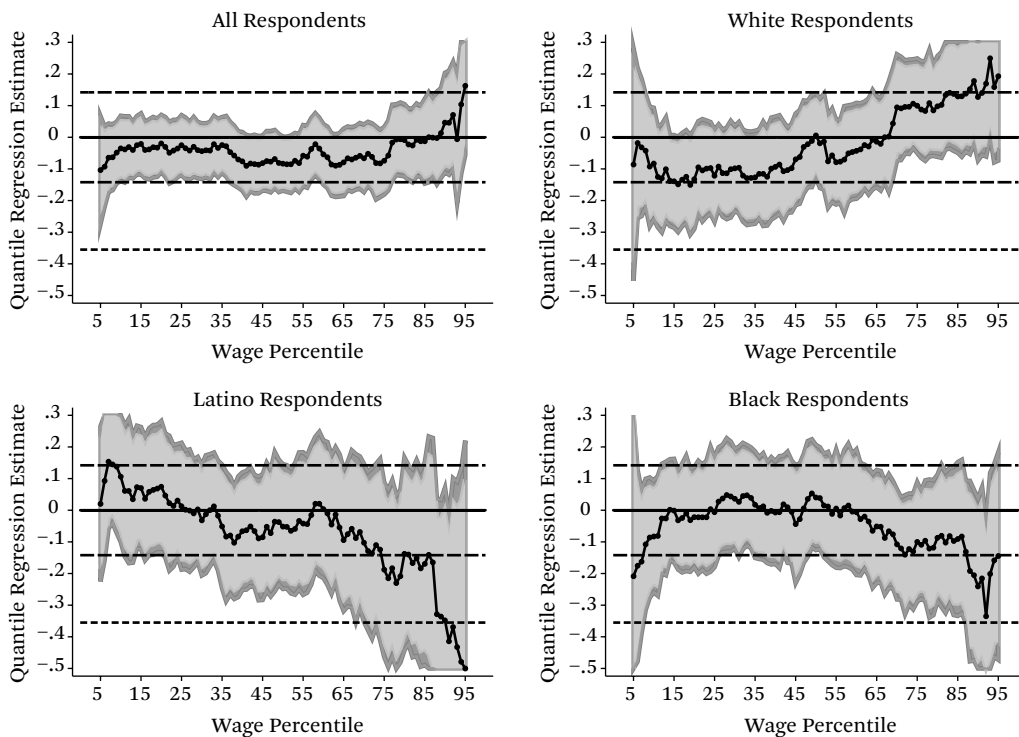
Source: Authors' estimates from respondents' last available round of the National Longitudinal Survey of Youth 1997, rounds 1–17 (Bureau of Labor Statistics 2015).
Note: Estimates are unweighted. The coefficients derive from unconditional quantile regression models of log hourly wage with sibling fixed effects, with cluster-robust standard errors obtained from the bootstrap with 250 replications. The confidence intervals are 90 percent (light gray) and 95 percent (dark gray). For graphing purposes, the coefficients and confidence intervals are censored at +0.3 and −0.5. The solid horizontal line is drawn at zero to judge statistical significance, whereas the dashed horizontal lines are drawn to judge substantive significance. Specifically, the long-dashed lines mark a small effect size ($|d| = 0.20$), and the short-dashed line marks a medium effect size ($|d| = 0.50$).

black wages, over and above noncarceral contacts.

Interestingly, the relationship between incarceration and wages is strong and consistent enough among the black respondents that it is observable in the pooled sample. As can be seen in figure 3 (top left panel), above the 40th percentile, many of the quantile regression coefficients are both significant and sizable, as indicated by an abundance of small effect sizes (that is, $|d| \geq 0.20$). Indeed, 41 percent of the incarceration coefficients are statistically significant using a .05 criterion (and 58 percent are significant using a .10 criterion). These are apparently driven by the incarcera-

tion experiences of the one-quarter of the sample who is black, because when black respondents are removed from the pooled sample, there is just a single significant incarceration coefficient.

Figure 4 graphs the wage gap in dollar metric rather than proportional metric for justice-involved black respondents and their similar-age siblings with no criminal justice contact. At each wage percentile, the estimate averages over the difference in exponentiated marginal predictions derived from the quantile regression models. Given the nonsignificance of arrest, we focus our attention on incarceration. Below the 40th percentile—which is $11.85 per

Figure 3. Full Quantile Regression Estimates of the Relationship Between Incarceration and Hourly Wage
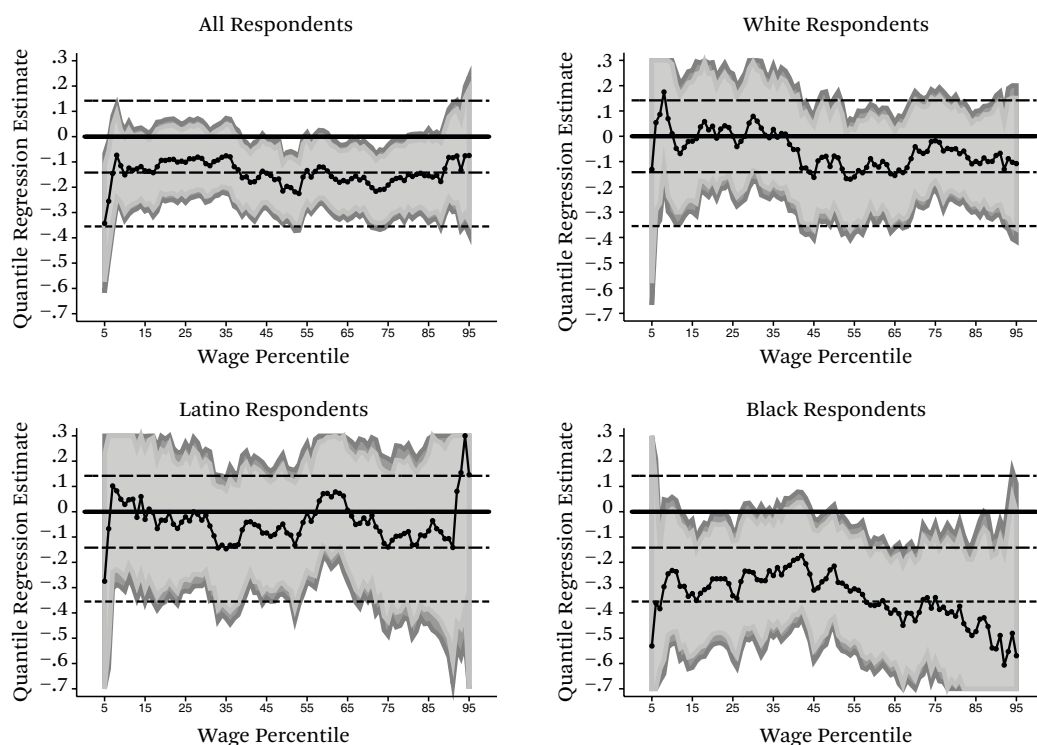
Source: Authors' estimates from respondents' last available round of the National Longitudinal Survey of Youth 1997, rounds 1–17 (Bureau of Labor Statistics 2015).
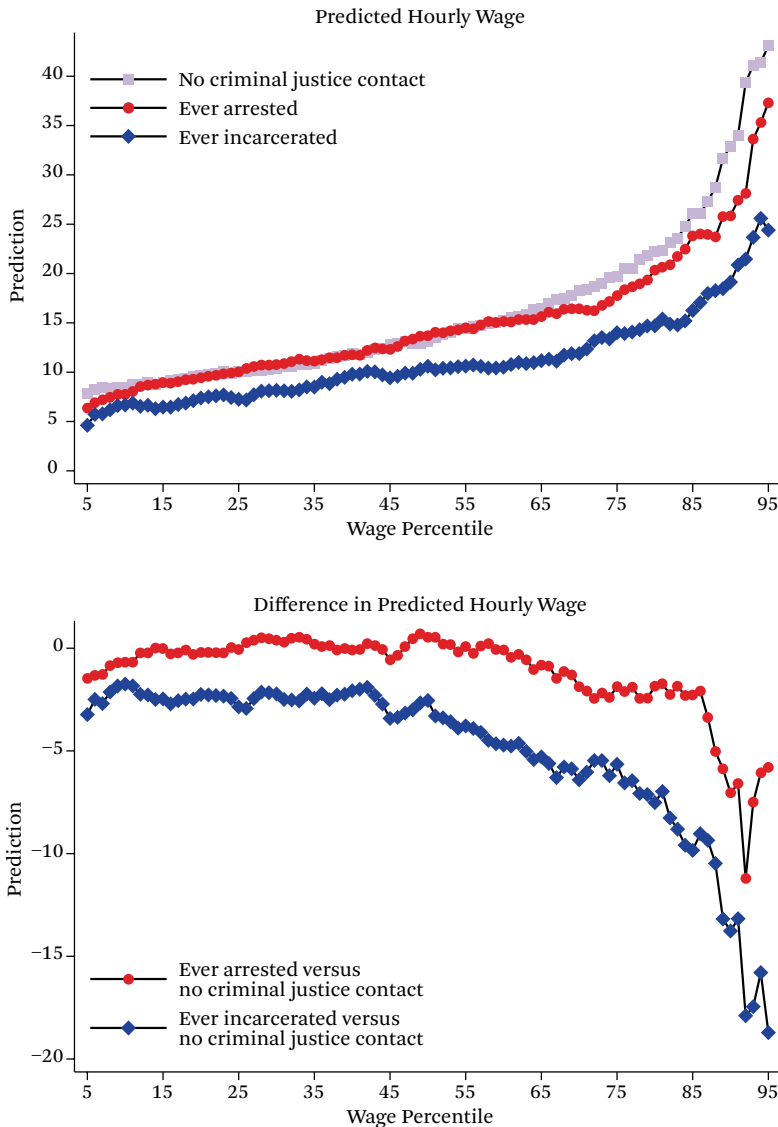
Note: Estimates are unweighted. The coefficients derive from unconditional quantile regression models of log hourly wage with sibling fixed effects, with cluster-robust standard errors obtained from the bootstrap with 250 replications. The confidence intervals are 90 percent (light gray) and 95 percent (dark gray). For graphing purposes, the coefficients and confidence intervals are censored at +0.3 and −0.7. The solid horizontal line is drawn at zero to judge statistical significance, whereas the dashed horizontal lines are drawn to judge substantive significance. Specifically, the long-dashed lines mark a small effect size ($|d| = 0.20$), and the short-dashed line marks a medium effect size ($|d| = 0.50$).

hour among the baseline, no-contact black respondents—the wage penalty for formerly incarcerated blacks is a roughly constant $2.40. When considered in percentage terms, this implies that the wage gap narrows as the baseline hourly wage grows. Above the 40th percentile, on the other hand, the wage penalty grows in both an absolute and relative sense as the baseline wage grows. In percentage terms, then, the size of the wage penalty is roughly U-shaped when evaluated across the full wage distribution of black respondents.

Supplemental Measures of Criminal Justice Contact

In the appendix, we substitute the binary indicators of criminal justice contact with continuous measures. Table A2 provides quantile regression estimates including arrest frequency and the total time spent incarcerated in jail or prison. (The coefficients and standard errors are multiplied by ten to eliminate zeros.) There is some indication that arrest accumulation culminates in a wage penalty among white respondents at the 50th percentile and lower. For ex-

Figure 4. Implied Relationship Between Criminal Justice Contact and Hourly Wage in 2016 Dollars, Black Respondents

Source: Authors' estimates from respondents' last available round of the National Longitudinal Survey of Youth 1997, rounds 1–17 (Bureau of Labor Statistics 2015).
Note: Estimates are unweighted. The coefficients derive from unconditional quantile regression models of log hourly wage with sibling fixed effects, and average over the difference in exponentiated marginal predictions.

ample, at the 25th percentile, the hourly wage of whites with ten arrests is 21 percent lower $(e^{-0.23} − 1 = −0.21)$ than their similar-age siblings with no arrest record. There is also evidence that accumulation of time spent behind bars

further corrodes the wages of black respondents beyond the stage effects observed in table 1.

Table A3 provides quantile regression estimates including the number of years elapsed since the first arrest and first incarceration

spell. For black respondents, the results indicate that the size of the wage gap grows over time, but the same is not true for other racial/ethnic groups or other forms of criminal justice contact. For example, at the 50th percentile, the hourly wage is estimated to be 12 percent lower ($e^{-0.13} - 1 = -0.12$) if a black respondent was first incarcerated five years earlier, but 24 percent lower ($e^{-0.27} - 1 = -0.24$) if the first incarceration spell was ten years earlier, and 33 percent lower ($e^{-0.40} - 1 = -0.33$) if it was fifteen years earlier.

In a final set of models, we substitute binary indicators for having been charged or convicted with a crime for having been arrested for a crime.[15] In the quantile regression models with charging and incarceration, the only difference from what is reported above concerns white respondents, for whom wages are significantly lower at the 10th percentile ($p < .05$) and 25th percentile ($p < .10$) among those who have been charged compared to their similar-age siblings who have never been charged.[16] The results for Latinos and blacks are otherwise unchanged. In the quantile regression models with conviction and incarceration, the only difference is Latino wages are significantly lower at the 50th percentile ($p < .10$) and 90th percentile ($p < .05$) following conviction. The results for whites and blacks are otherwise unchanged.

DISCUSSION

Whether criminal justice contact is correlated with early adult wages depends to a great extent on a respondent's race-ethnicity and the level of contact. Interestingly, arrest is largely uncorrelated with wages in our analysis. For example, of 91 quantile regression coefficients for arrest, none is statistically significant for whites, and just one is significant for Latinos and blacks (using a 0.05 criterion). There are thus far fewer significant results than what we would expect by chance, even conditional on there being no true relationship between arrest and wages. One possible exception is arrest frequency

among white respondents at the 50th percentile and below, for whom there is a weak indication that arrest has incremental effects stemming from the accumulation of repeated contacts. Another exception relates to having been charged (in place of arrest), which is correlated with wage erosion among whites at the 25th percentile and below. In any case, the impact of any single arrest is so small as to be negligible, but repeated arrests and post-arrest criminal justice processing do correspond with a wage gap among low- to middle-wage whites.

Highly consistent evidence of a relationship between criminal justice contact and early adult wages stems from incarceration among black respondents. For whites and Latinos, no empirical evidence supports a wage penalty following carceral contact; the coefficients are both statistically and substantively null. On the contrary, formerly incarcerated blacks earn significantly lower wages than their similar-age siblings with no history of criminal justice contact (and even their similar-age siblings who have an arrest record), and the coefficients are noteworthy in that they are not trivial in magnitude. Across the black wage distribution, the estimates indicate a U-shaped wage penalty, with an inflection point at about the 40th percentile where the wage penalty is smallest in percentage terms. All evaluation points, however, show a corrosive correlation between incarceration and black wages.

The evidence therefore supports the conclusion that the relationship between criminal justice contact and early adult wages is heterogeneous. Namely, the wage gap is more or less limited to incarceration among black respondents, and, with the noted exceptions, there is no wage gap following arrest for blacks, nor a discernible wage gap following any form of criminal justice contact for whites and Latinos. Furthermore, the size of the black wage gap varies along the wage distribution; in percentage terms, it averages roughly 26 percent in the middle half of the wage distribution and 38 per-

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15. Note that 80 percent of NLSY97 respondents who have ever been arrested reporting having been charged, and 60 percent have been convicted.

16. David Kirk's study of self-report versus official arrest indicates a tendency of some youth (whites in particular) to overreport arrest, suggesting that self-report charges might be more valid as a measure of arrest for this group (2006).

cent in the lower and upper quartiles. The strength and salience of the relationship between incarceration and black wages is further evident from the fact that an incarceration-wage relationship is detectable in a model that pools together whites, Latinos, and blacks—the wage gap in this model is driven by the roughly 3.5 percent of the NLSY97 sample who are formerly incarcerated black respondents.

Our results both harmonize and conflict with prior studies. For example, our conclusions differ from those of Matsueda and his colleagues and Needels, both of whom observe no relationship between incarceration and earnings among mostly black reentry program participants (Matsueda et al. 1992; Needels 1996). They also differ from those of Anke Ramakers and colleagues and Rasmus Landersø, who find no relationship between incarceration (length) and wages or earnings among recently incarcerated individuals in the Netherlands and Denmark (Ramakers et al. 2014; Landersø 2015). On the other hand, our conclusions are in line with the findings of Waldfogel and Grogger, who identify effects of incarceration on administrative earnings (Waldfogel 1994a, 1994b; Grogger 1995); with Grogger's finding that arrest is uncorrelated with long-term (beyond one year) earnings (1995); and with Signe Andersen's finding of long-term income erosion following incarceration (versus community service) among punished individuals in Denmark (2015). The findings align also with those of numerous other studies that estimate self-report wage gaps of varying size in panel data from the NLSY79, Fragile Families, and the NLSY97 (Fagan and Freeman 1999; Western 2002; Geller, Garfinkel, and Western 2006; Raphael 2007; Apel and Sweeten 2010; Jung 2015). However, that incarceration is correlated only with wages for black respondents in our study but not for whites or Latinos conflicts with Western's finding of uniform wage erosion across demographic groups (2002).

Provisional follow-up analyses, which are not shown, indicate that the wage penalty experienced by formerly incarcerated blacks likely stems from a combination of supply-side and demand-side mechanisms. First, we find that formerly incarcerated blacks work fewer weeks than their similar-age siblings with no criminal justice contact, and this is due to their longer duration of labor force nonparticipation rather than to unemployment (see also Apel and Sweeten 2010). Second, we find that formerly incarcerated black workers are employed in less prestigious occupations than their no-contact siblings, consistent with the race-coded job channeling noted by Pager, Western, and Bart Bonikowski (2009a). It thus seems likely that black wage inequality due to incarceration is attributable to a combination of work detachment and low work quality.

By way of limitations, the estimates reported in this study are the long-term effects of criminal justice contact. The typical first arrest occurred almost thirteen years prior to the last available interview, and the typical first incarceration spell was experienced almost nine years prior. Short-term effects that cannot be observed by virtue of our study design are therefore possible. Additionally, because our data are cross-sectional (the last available interview), high collinearity makes it impossible to simultaneously include all available measures of criminal justice contact. It will thus be important to build on this study using panel methods that are capable of exploiting the timing of first contact with different stages of the criminal justice system. Facets of work experience other than hourly wages might also undergo corrosion following criminal justice contact, especially for the whites and Latinos in our study for whom no correlations are consistent. Finally, undoubtedly other sources of confounding than strictly household-based confounding (which can be eliminated using sibling fixed effects) are possible. Criminal offending is an obvious candidate, but it is regrettably measured inconsistently and from poorly defined subsamples over time in the NLSY97. However, when we control for a measure of the total frequency of self-report crime from the first interview (a measure available for the full sample and temporally prior to criminal justice contact), the findings are unchanged.

To bring this study to a close, our findings provide confirmation of "double jeopardy" and "compound disadvantage" of being both black and formerly incarcerated in the labor market (Pager 2005, 2007; Lyons and Pettit 2011). In our data, this is evident from the fact that, at any

given percentile, the wages of black respondents are lower than their white counterparts, and black wages carry a penalty from incarceration that white wages do not. There is thus substantial between-race inequality in wages, worsened by within-race inequality that follows incarceration among black respondents but not their non-black counterparts.

Where we believe our results make a new empirical contribution to punishment and inequality scholarship is our finding that black wages following incarceration are lower no matter the point in the wage distribution. Research tends to focus on the marginalization of blacks already in a socially precarious position, for example, those applying for entry-level and low-wage jobs (Pager 2003), those whose school dropout and persistent joblessness are concealed by mass incarceration and thus from national indicators of economic health and racial well-being (Pettit 2012; Western and Beckett 1999), or those who subsist in a racialized caste system reminiscent of the ghetto (Wacquant 2000). Although our findings do not repudiate these concerns, they do suggest that incarceration is a salient barrier to wage mobility for a much larger swath of the black population than is apparent in punishment and inequality discourse.

APPENDIX

In a standard regression model with sibling fixed effects, the basic parameterization would be as follows:

$$Y_{il} = \alpha + \sum_{j=1}^{K} \beta_j X_{ilj} + \delta_1 Arrest_{il} + \delta_2 Incarceration_{il} + u_l + e_{il}, \tag{1}$$

where $i = 1, \ldots, N$ indexes individuals, $l = 1, \ldots, M$ indexes households, and $j = 1, \ldots, K$ indexes control variables. The model assumes

that u_l is fixed rather than random, giving rise to the sibling fixed-effects model. The way criminal justice contact is measured, a respondent who has ever been incarcerated has also, by definition, been arrested. The coefficient for incarceration in this regression model thus represents the additive influence of jail or prison confinement on hourly wages, over and above arrest. Summing the arrest and incarceration coefficients yields the relationship between hourly wages and total criminal justice contacts from arrest to incarceration. Specifically, the two quantities of interest are formed as follows:

$$\vec{\delta}_r = \begin{cases} \delta_1 & \textit{if Arrested} \\ \delta_1 + \delta_2 & \textit{if Incarcerated} \end{cases}. \tag{2}$$

For the resulting coefficients and standard errors, the reference group comprises respondents who have never experienced criminal justice contact.

Quantile regression expands on this approach by probing the distributional effects of criminal justice contact on early adult wages. The unconditional quantile regression model with sibling fixed effects is estimated using the method of Firpo and his colleagues (Firpo, Fortin, and Lemieux 2009; Firpo 2007; for software details, see Borgen 2016). The appeal of this model is the ability to estimate the impact of regressors on the unconditional or marginal quantile of the outcome, as opposed to the conditional quantile as is typical in quantile regression models (Koenker 2005; Koenker and Bassett 1978).[17] This is accomplished via calculation of a recentered influence function (RIF) as a first step:

$$RIF(Y_i; q_\tau, F_Y) = q_\tau + \frac{\tau - 1\{Y_i \le q_\tau\}}{f_Y(q_\tau)}, \tag{3}$$

17. In a standard (conditional) quantile regression model with some form of criminal justice contact as the key regressor, for example, the model estimates would reflect whether respondents with criminal justice contact have a higher or lower wage than what would be expected given their characteristics on the control variables. The control variables influence where in the wage distribution respondents fall, however, so the estimate of criminal justice contact is identified within groups of individuals sharing the same profile on all of the covariates except criminal justice contact. In the unconditional quantile regression model, on the other hand, it is possible to examine how the relationship between criminal justice contact and wages (net of their joint association with the control variables) varies across the outcome distribution. This is because the quantiles are defined with respect to the unconditional distribution rather than the conditional distribution and thus the control variables (for a good description of the distinction, see Killewald and Bearak 2014).

where q_τ is the value of the outcome at quantile τ, $f_Y(q_\tau)$ is the density of the outcome at quantile τ, and $1\{Y_i \leq q_\tau\}$ is a dummy indicator for whether the outcome for individual i is at or below q_τ. Note that the density can be estimated using any suitable kernel weighting function, and in this analysis we choose the Epanechnikov kernel with a bandwidth or half-smoothing window equal to one-quarter of a standard deviation of the outcome trimmed at the 1st and 99th percentiles. For each respondent, at each quantile, the RIF takes on one of two values, resembling a regime switch:

$$RIF(Y_i; q_\tau, F_Y) = \begin{cases} q_\tau - \dfrac{1-\tau}{f_Y(q_\tau)} & \text{if } Y_i \leq q_\tau \\[2ex] q_\tau + \dfrac{\tau}{f_Y(q_\tau)} & \text{if } Y_i > q_\tau \end{cases}.$$

At the second step, the estimated RIF is treated as the dependent variable in a cross-sectional, linear regression model that includes sibling fixed effects along with the regressors:

$$\widehat{RIF}(Y_{il}; q_\tau, F_Y) = \alpha + \sum_{j=1}^{K} \beta_j X_{ilj} + \delta_1 Arrest_{il} \\ + \delta_2 Incarceration_{il} + u_l + e_{il}, \quad (4)$$

where the terms are all defined as in (1). We also perform the same summing procedure defined in (2) to obtain the two quantities of interest.

As Firpo and his colleagues define it, a coefficient in this model "corresponds to the marginal effect on the unconditional quantile of a small location shift in the distribution of covariates, holding everything else constant" (2009, 954). In the case where the regressor of interest is a dummy variable, as is true for criminal justice contact, the coefficient represents the impact of a change in the probability of experiencing a particular stage of criminal justice contact. Note that we estimate the RIF regression for all percentiles ranging from the 5th to the 95th and obtain cluster-bootstrapped standard errors with 250 replications.

Effect-Size Calculation

As a supplement to standard tests of statistical significance, we devote attention to the substantive significance of results with the use of effect-size calculations. The effect size of choice, Cohen's d, is the standardized difference between means estimated from two independent groups (Cohen 1988). It takes the following elementary form:

$$d = \frac{\left| \bar{Y}_T - \bar{Y}_C \right|}{\sqrt{\left[s_{Y_T}^2 (N_T - 1) + s_{Y_C}^2 (N_C - 1) \right] / (N_T + N_C - 2)}} .$$

Here, T references a treatment or intervention group and C references a comparison or non-intervention group, and the denominator is the pooled variance. This formula is adapted in three ways for the current study. First, the treatment and comparison groups referenced in the formula are defined, respectively, by whether a given stage of criminal justice contact has ever been reached or not. Second, the numerator is replaced by a regression coefficient from the quantile regression model, which yields an adjusted difference in the hourly wage at a given quantile. Third, the denominator is replaced by the pooled variance of the hourly wage at the last interview, from groups defined by the cumulative stage of criminal justice contact.

These modifications give rise to the following effect-size formula:

$$d_r = \frac{\left| \bar{\delta_r} \right|}{\sqrt{\left[s_{Y_T}^2 (N_T - 1) + s_{Y_C}^2 (N_C - 1) \right] / (N_T + N_C - 2)}} , \quad (5)$$

where the numerator is the coefficient for either arrest or incarceration, as defined in (2). Cohen's d is bound by $[0, \infty)$ and is routinely judged against thresholds of 0.20 (small), 0.50 (medium), and 0.80 (large) with respect to substantive significance (Cohen 1988). An effect size smaller than 0.20 is generally regarded as not worth mentioning, even if the coefficient is statistically significant.

Figure A1. Cumulative Distribution of Hourly Wage, by Criminal Justice Contact and Race/Ethnicity
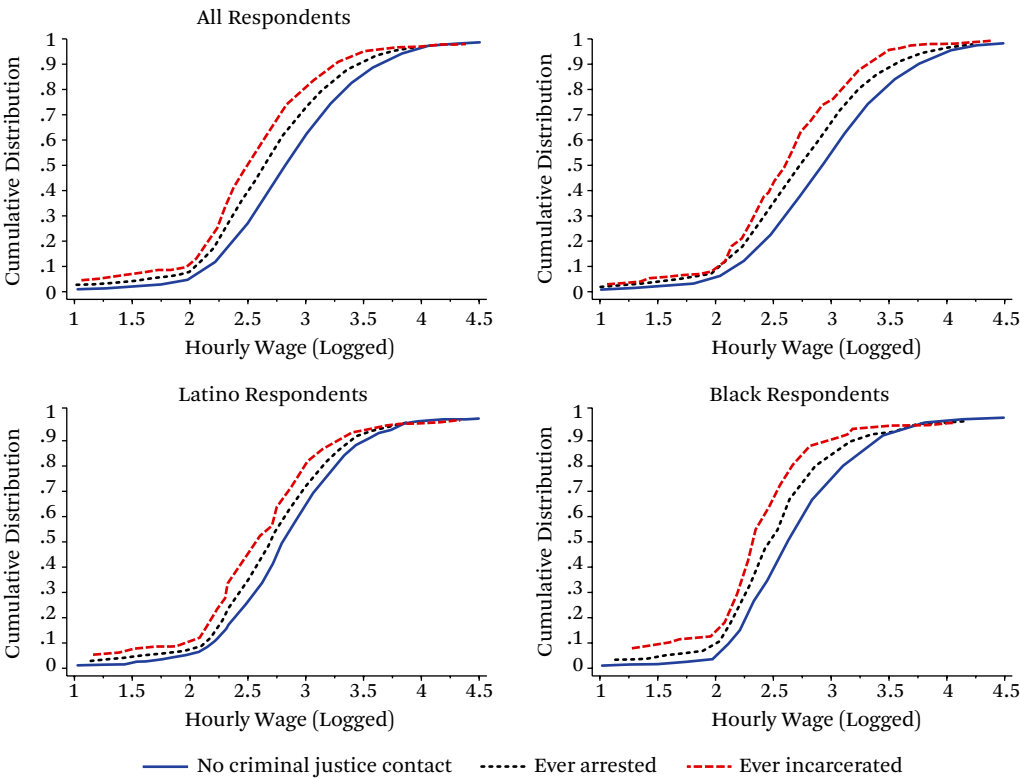
Source: Authors' estimates from respondents' last available round of the National Longitudinal Survey of Youth 1997, rounds 1–17 (Bureau of Labor Statistics 2015).
Note: Estimates are unweighted. The distributions derive from the full sample, not the sibling subsample. The criminal justice contact groups are not mutually exclusive.

Table A1. Descriptive Statistics

Variables	All Respondents Mean (SD)	White Respondents Mean (SD)	Latino Respondents Mean (SD)	Black Respondents Mean (SD)
Nonzero wage	87.7%	89.5%	88.0%	83.8%
Hourly wage[a][b]	19.2 (12.9)	21.1 (14.2)	18.4 (11.1)	15.7 (10.3)
Criminal justice contact				
Ever arrested	35.0%	31.8%	35.3%	41.3%
Ever incarcerated	9.8%	7.9%	10.0%	13.3%
Ever jailed	7.5%	6.6%	8.3%	8.7%
Ever imprisoned	4.6%	3.0%	4.3%	8.0%
Total number of arrests[a]	3.8 (5.2)	3.5 (4.5)	3.9 (5.2)	4.2 (6.1)
Total months incarcerated[a]	12.9 (21.4)	10.2 (18.6)	11.4 (23.4)	17.2 (22.7)
Total months jailed[a]	5.9 (11.5)	5.1 (12.8)	5.2 (7.7)	7.8 (11.6)
Total months imprisoned[a]	17.9 (24.8)	15.7 (20.3)	16.5 (32.3)	20.2 (24.2)
Years since first arrest	12.8 (5.0)	12.9 (5.0)	12.6 (5.2)	12.8 (5.0)
Years since first incarceration	8.8 (4.9)	8.6 (5.0)	8.8 (4.8)	9.0 (4.7)
Control variables				
Male	51.2%	51.7%	51.4%	50.1%
Age	31.9 (4.0)	31.6 (4.3)	32.1 (3.7)	32.4 (3.5)
Marital status				
Never married	51.0%	43.2%	49.7%	68.0%
Currently married	39.7%	47.1%	40.0%	24.3%
Separated or divorced	9.3%	9.7%	10.3%	7.7%
Currently cohabiting	17.4%	17.0%	20.0%	15.9%
Biological children	1.3 (1.4)	1.1 (1.3)	1.5 (1.4)	1.6 (1.6)
Dwelling type				
House or farm	68.8%	74.1%	69.5%	57.6%
Apartment or condo	23.2%	17.9%	25.0%	32.3%
Other dwelling	8.0%	8.0%	5.5%	10.1%
Urbanicity				
MSA central city	40.6%	33.4%	45.2%	51.5%
MSA suburb	53.4%	59.1%	50.3%	44.0%
Outside of MSA	6.1%	7.5%	4.5%	4.5%
Census region				
Northeast	16.2%	18.4%	13.4%	14.0%
Midwest	20.8%	27.6%	9.0%	16.5%
South	40.1%	32.5%	31.6%	62.0%
West	23.0%	21.5%	46.0%	7.5%
Ever incarcerated as juvenile	2.5%	1.7%	3.3%	3.4%
N (full sample)	8,984	4,748	1,901	2,335
N (sibling subsample)	4,035	2,043	947	1,045

Source: Authors' estimates from respondents' last available round of the National Longitudinal Survey of Youth 1997, rounds 1–17 (Bureau of Labor Statistics 2015).

Note: Descriptive statistics are unweighted and are based on the full sample, not the sibling subsample. The means of dummy variables are shown as percentages. Hourly wages are in 2016 dollars and, for descriptive purposes, are trimmed at the 99th percentile.

[a] Descriptive estimate is limited to respondents with nonzero values.

[b] Variable is shown here untransformed, but is logged in the regression models.

Table A2. Sensitivity of Select Quantile Regression Estimates to Alternative Measures of Criminal Justice Contact

Percentile	Total Arrest Frequency (÷ 10) Coeff. (SE)	Total Incarceration Months (÷ 10) Coeff. (SE)	Total Arrest Frequency (÷ 10) Coeff. (SE)	Total Incarceration Months (÷ 10) Coeff. (SE)
	(A) All Respondents		(A) White Respondents	
10th	−0.13 (0.08)	−0.06 (0.04)[+]	−0.29 (0.15)[+]	0.01 (0.07)
25th	−0.07 (0.08)	−0.04 (0.04)	−0.23 (0.11)*	0.06 (0.06)
50th	−0.16 (0.08)*	−0.02 (0.04)	−0.20 (0.10)[+]	0.07 (0.08)
75th	−0.15 (0.08)[+]	−0.02 (0.02)	−0.02 (0.11)	0.01 (0.04)
90th	0.03 (0.14)	−0.03 (0.03)	−0.05 (0.18)	0.01 (0.06)
Linear (fixed)	−0.06 (0.11)	−0.07 (0.04)[+]	−0.25 (0.09)**	0.04 (0.02)*
	(C) Latino Respondents		(D) Black Respondents	
10th	0.27 (0.20)	−0.14 (0.09)	−0.01 (0.11)	−0.01 (0.01)
25th	0.00 (0.16)	−0.06 (0.07)	−0.03 (0.13)	−0.10 (0.06)[+]
50th	−0.03 (0.15)	0.02 (0.06)	0.01 (0.17)	−0.10 (0.06)[+]
75th	−0.03 (0.21)	0.00 (0.06)	−0.03 (0.14)	−0.10 (0.05)*
90th	0.14 (0.31)	0.02 (0.10)	−0.27 (0.19)	−0.07 (0.08)
Linear (fixed)	0.31 (0.34)	−0.07 (0.05)	−0.08 (0.11)	−0.17 (0.06)**

Source: Authors' estimates from respondents' last available round of the National Longitudinal Survey of Youth 1997, rounds 1–17 (Bureau of Labor Statistics 2015).
Note: Estimates are unweighted. The coefficients derive from (unconditional) quantile regression models of log hourly wage with sibling fixed effects. The coefficients for control variables are not shown. Cluster-robust standard errors are reported, and are obtained from the bootstrap with 250 replications. For comparative purposes, coefficients from linear regression models (of the mean) with sibling fixed effects are also shown.
[+]$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests)

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Table A3. Sensitivity of Select Quantile Regression Estimates to Alternative Measures of Criminal Justice Contact

Percentile	Years Since First Arrest Coeff. (SE)	Years Since First Incarceration Coeff. (SE)	Years Since First Arrest Coeff. (SE)	Years Since First Incarceration Coeff. (SE)
	(A) All Respondents		(B) White Respondents	
10th	−0.00 (0.00)	−0.01 (0.01)	−0.01 (0.01)	0.01 (0.02)
25th	−0.00 (0.00)	−0.00 (0.01)	−0.01 (0.00)[+]	0.01 (0.01)
50th	−0.00 (0.00)	−0.01 (0.01)	0.00 (0.01)	−0.00 (0.01)
75th	−0.00 (0.00)	−0.01 (0.01)	0.00 (0.01)	−0.00 (0.01)
90th	−0.00 (0.01)	−0.00 (0.01)	0.00 (0.01)	−0.01 (0.01)
Linear (fixed)	−0.00 (0.00)	−0.01 (0.01)[+]	−0.00 (0.00)	0.00 (0.01)
	(C) Latino Respondents		(D) Black Respondents	
10th	0.01 (0.01)	−0.01 (0.02)	−0.01 (0.01)	−0.02 (0.01)
25th	−0.00 (0.01)	−0.00 (0.01)	−0.00 (0.01)	−0.03 (0.01)*
50th	0.00 (0.01)	−0.01 (0.01)	0.01 (0.01)	−0.03 (0.01)**
75th	−0.01 (0.01)	0.01 (0.02)	−0.00 (0.01)	−0.02 (0.01)*
90th	−0.01 (0.01)	0.02 (0.02)	−0.01 (0.01)	−0.03 (0.02)
Linear (fixed)	0.01 (0.01)	−0.01 (0.02)	−0.01 (0.01)	−0.04 (0.01)***

Source: Authors' estimates from respondents' last available round of the National Longitudinal Survey of Youth 1997, rounds 1–17 (Bureau of Labor Statistics 2015).

Note: Estimates are unweighted. The coefficients derive from (unconditional) quantile regression models of log hourly wage with sibling fixed effects. The coefficients for control variables are not shown. Cluster-robust standard errors are reported, and are obtained from the bootstrap with 250 replications. For comparative purposes, coefficients from linear regression models (of the mean) with sibling fixed effects are also shown.

[+]$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests)

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