



The Great Decoupling: The Disconnection Between Criminal Offending and Experience of Arrest Across Two Cohorts

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Our study explores the arrest experiences of two generational cohorts—those entering adulthood on either side of a large shift in American policing. Using the National Longitudinal Survey of Youth (1979 and 1997), we find a stark increase in arrest odds among the later generation at every level of offending, suggesting a decoupling between contact with the justice system and criminal conduct. Furthermore, this decoupling became racially inflected. Blacks had a much higher probability of arrest at the start of the twenty-first century than both blacks of the generation prior and whites of the same generation. The criminal justice system, we argue, slipped from one in which arrest was low and strongly linked to offending to one where a substantial share of Americans experienced arrest without committing a crime.

Keywords: criminal justice contact, carceral state, criminal offending, generational change

“Black teens who commit a few crimes go to jail as often as white teens who commit dozens.” So read a recent *Washington Post* headline (Ehrenfreund 2015). This finding emerged alongside other news that nearly three of every four young black men had been stopped and frisked by police in New York City but were much less likely than whites who were stopped to have contraband or be engaged in unlawful

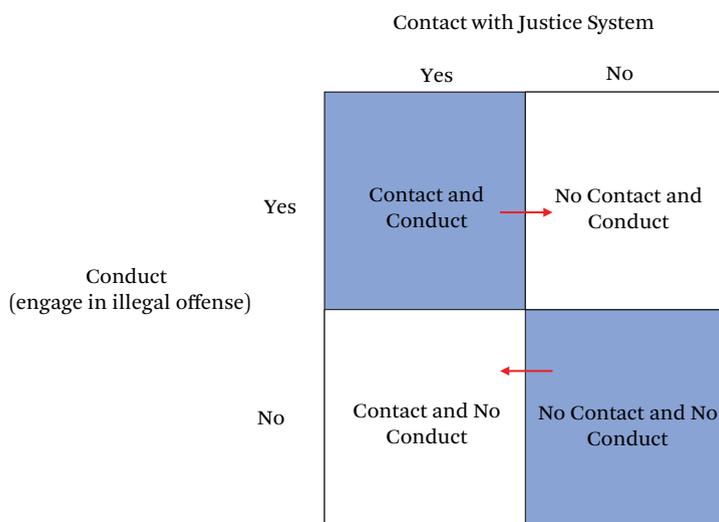
activity (Fagan et al. 2009). Although scholars have penned volumes on the rise of the carceral state, expansion of surveillance, several “wars on” policy developments, and their racially disparate consequences, the possibility that contact with criminal justice was increasingly disconnected from criminal offending (and that this disconnect was racially inflected) was barely taken up.¹

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1. There are of course several exceptions, particularly work by legal scholar Jeffrey Fagan. In general, however, scholars interested in crime rarely engage scholars motivated to understand punishment.

Figure 1. Basic Depiction of the Relationship Between Contact and Conduct

Source: Authors' compilation.

We argue that existing bodies of knowledge mischaracterize one of the most fundamental relationships in the modern era between Americans and the carceral state—namely, that between criminal offending and criminal justice contact. A sizable body of research tacitly assumes the relationship to be unidirectional, with more offending leading to more contact and, subsequently, more incarceration. Yet research on mass incarceration documents that shifts in policies are a primary mechanism to increasing contacts with the justice system (and subsequent punishment) net of individual criminality (Tonry and Melewski 2008). One crude measure of this change is the shift in confinements per crime: in 1970, the ratio was twenty inmates per thousand crimes; by 2000, it had increased to 112 for every thousand. Given this dynamic, how might exposure to criminal justice connote something other than offending patterns? How might criminality and justice involvement be intertwined in ways that get lost in academic debates? In addition, how might larger policy changes—such as the war on drugs and broken windows policing—which increased contact, affect the risk of justice involvement for different generations of individuals and different generations of racial groups?

This study would be the first (to our knowledge) to examine how the relationship between

reported criminality and justice involvement has changed across two generations, slipping from a system under which involvement was a good proxy for having run afoul of the law to one defined by increasingly separate constituencies of criminal offender and custodial citizen (Lerman and Weaver 2014).

We start from a fairly uncontroversial assumption that contact should follow, not lead, criminal offending. The idea is so simple and straightforward as to be obvious: that criminal justice exposure should exhibit a strong relationship to being engaged in crime. Otherwise said, individual criminal justice contact should be strongly predicted by criminal behavior and offending patterns. Imagine a two-by-two table with four quadrants (figure 1). We take it as uncontroversial that under a criminal justice system that promotes public safety while confining abuses of power, most Americans should fall in the shaded diagonal and a very small share should fall in the opposite diagonal—those people who did not engage in crime but were arrested or jailed or, conversely, those individuals who were not law abiding but were also not arrested (or, therefore, convicted). In other words, for a non-arbitrary system of justice, contact should closely follow conduct. The share of Americans having involuntary encounters with criminal justice institutions (police,

courts, probation or parole agencies, jails, and prisons) should be tightly coupled and have considerable overlap with those who engage in unlawful behavior. There will be mistakes in interpreting law abiding and assessing guilt, of course, but the exceptions should not mock the rule.

This article descriptively examines the distributions within these quadrants for two distinct generational cohorts, those coming of age as the prison boom was beginning in earnest and those moving through early adulthood two decades later when incarceration would soon reach its peak, based on a representative and over-time data source, the National Longitudinal Survey of Youth (NLSY 2014). This survey is particularly suited to our aims because it queries respondents about their criminal behavior as well as about being arrested by police (which we use as a proxy for criminal justice contact).

Our preliminary findings are striking and carry troubling implications. First, the members of the late-1990s cohort were much more likely to have contact with legal authorities than the 1979 cohort, even though the earlier cohort reported engaging in substantially more offending. Given that the latter cohort reached adulthood under the policy and practices of broken windows policing, zero tolerance in schools, the drug war, and mandatory minimums, the greater odds of being arrested makes sense. Concretely, in the 1979 cohort of eighteen- to twenty-three-year-olds, only 10 percent had been arrested by police, versus 25 percent of their counterparts in the 1997 cohort.

Second, we find that the predictive value of criminal offending for estimating justice involvement waned for Generation X. Self-reported criminal conduct, thus, is a less good predictor among the more recent generation of having contact with criminal justice. In the 1979 cohort, if one did not report unlawful behaviors, one was somewhat unlikely to report experiencing arrest (18 percent of those who reported being arrested reported no offending). By the 1997 cohort, it was the opposite: fully 70 percent of the people who reported that they had been arrested did not report engaging in a property or violent crime. So distinct are these trends in contact that we find that reported criminal involvement in the earlier co-

hort triggers arrest by police at the same rate as no reported offending in the later cohort. By the later generation, the underlying relationship between crime and contact with criminal justice had transformed.

Third, and more troubling still, we find the growth of a cavernous disparity by racial membership across the generations. In the 1979 cohort, quadrant membership does not differ significantly by racial group. In the 1997 cohort, it does—and dramatically so. By 1997, the share who reported no criminal offending but being arrested grows and differs by racial group. Crime self-report distributions by race do not shift by more than a few percentage points across the two cohorts; contact with the law does. In the 1997 cohort, black men were more likely than white men to be arrested and report no illegal activity. In addition, the group that is the least visible in scholarly or popular discourse, namely, those who report engaging in property damage, theft, or violence but are not arrested or convicted, is also racially inflected; white men were more likely than black men to indicate engaging in criminal offenses but not being arrested.

Our conclusion is stark: security from state discipline and oversight is increasingly decoupled from law-abidingness, conditioned less on patterns of behavior than in prior generations. This decoupling is what characterizes criminal justice in the twenty-first century, which has dire consequences for the lives of those who are at high risk of oversight despite not having engaged in crime.

THE CRIME-CONTACT CONUNDRUM

The idea that criminality is tightly linked to contact with the criminal justice system is so foundational to most criminological theory, political rhetoric, and public policy that we take it for granted. Quite simply, we by and large assume that many if not most of those involved in the criminal justice system are engaged in some criminal activity or other wrongdoing (or that if they were not at the time of arrest, they were at some point doing something unlawful). Innocence (or mistaken criminality) is assumed to counter the modal experience and as something that should be sorted out by the system itself—such as false charges dropped, mis-

taken identities clarified, or innocent citizens freed. From this perspective, the severe inequities by race and place observed in arrest and incarceration rates are driven by how, where, and when the criminal justice system directs its gaze on particular parts of the population and not, so much, the underlying criminality of that population. The unequal effects of the war on drugs, for example, were driven by policies that applied more severe enforcement and sentences on those drug-related offenses found in minority communities (Alexander 2012). But scholarly analyses suggested that it was the enforcement, adjudication, and sentencing that was racially disparate and excessive (Tonry 1995), not necessarily that a significant share of those who were arrested were innocent of criminal activity or drug-related behaviors.

Alarming recent evidence from the fields of law, political science, sociology, and criminology suggest a decoupling of criminality from criminal justice contact, leading one of us to argue for the importance of distinguishing the criminal offender, who is characterized by his or her behavior and the “custodial citizen,” who “is defined by his or her relationship to the state,” a relationship predicated more on who one is than what one has done (Lerman and Weaver 2014, 32). Examples abound. Many studies began to document that racial disparities in arrest outcomes were poorly explained by individual-level differences in delinquency, and that black arrest odds remained substantially higher and racial arrest disparities appeared to strengthen even after taking into account differential crime involvement and criminal history (Unnever, Cullen, and Barnes 2017; Huizinga et al. 2007; Tapia 2012; Andersen 2015; Kirk 2008; Gase et al. 2016; Mitchell and Caudy 2017; but see Beaver et al. 2013).

One set of studies found stark disparities between actual drug possession and drug distribution arrests—namely, that higher drug arrest rates is not explained solely by greater involvement in drug distribution. To be sure, although a link between drug possession and being arrested for drug possession does exist, other factors such as where and under what condition the drug was purchased, what type of drug was purchased, and citizen complaints about crime all play a prominent role in who

is arrested for drug possession (Beckett et al. 2005; Ramchand, Pacula, and Iguchi 2006; Engel, Smith, and Cohen 2012). Those who purchased crack cocaine or who purchased drugs in public places from strangers were more likely to be arrested (Beckett et al. 2005; Ramchand, Pacula, and Iguchi 2006). These inequities are related to the fact that “open air” drug markets in disadvantaged urban neighborhoods are more likely to attract the attention of police than indoor drug operations often found in white suburbs (Hagedorn 1994). In a Seattle study, for instance, Katherine Beckett and her colleagues find that police practices targeting crack offenders and outdoor markets were directly related to the significant overrepresentation of blacks in drug arrest rates (2006, 105, 129). In a national sample, blacks and Latinos were much less likely to be engaged in drug offending but more likely to report experiencing a drug distribution arrest (Mitchell and Caudy 2017). Thus, enforcement decisions can create a situation in which blacks are just under 15 percent of all drug users but become 33 percent of all drug-related arrests, 46 percent of drug convictions, and 45 percent of those serving time in state prison for drug offenses (Bobo and Thompson 2010).

The pattern observed by Beckett and her colleagues for drug arrests also applies more generally to low-level misdemeanor arrests. The movement toward “broken windows” and “order maintenance” policing, first by New York and quickly followed by jurisdictions across the United States, provides a case in which exposure to the justice system becomes increasingly associated not with crime in the sense of *malum en se* criminality but, instead, for minor transgressions or perceived transgressions such as loitering, attempting to clean car windows at a stoplight, and so on (Sampson and Raudenbush 2004; Harcourt 2009; Fagan and Davies 2000). Often conducted without probable cause, misdemeanor arrests are often deployed against people who are legally innocent. Selection for arrest, not evidence of guilt, often drives outcomes in this low-level domain: “the petty offense process is permitted to distribute criminal liability based on race and social vulnerability rather than individual fault” (Natapoff 2012, 119–22). The result, as Issa Kohler-Hausmann

describes it, is a system of “misdemeanor justice” that places a massive burden upon tens-of-thousands of New York residents each year that leads to no findings of guilt, fine, or legal assessment that a crime was committed (2013). As the law professor and former prosecutor Paul Butler recounts of his own experience, “When I got arrested, I thought it would matter that I was innocent. It turns out, however, for misdemeanor arrests, whether you are innocent or guilty is not the most important thing” (2017, 64).

New York’s Stop, Frisk, and Question (SFQ) exemplifies perhaps the most dramatic decoupling of criminality from contact. Emerging from the “broken windows” approach to policing, New York’s SFQ’s explicit purpose was to maximize contacts with citizens purely for the purpose of questioning them in non-arrest situations “in the hope that some yield fruit” (Epp, Maynard-Moody, and Haider-Markel 2014, 8). In effect, this not only increased the number of contacts with the justice system but also introduced a new mechanism by which the system contacted Americans outside of the context of criminal offending. The result, in short, was the large-scale surveillance of minority communities. Through careful empirical studies, SFQ was shown to target blacks and Latinos far more frequently than whites, even when controlling for crime participation, and to target residents in minority neighborhoods regardless of their levels of neighborhood disorder and crime (Gelman, Fagan, and Kiss 2007, 821; Grunwald and Fagan, forthcoming). Not only did blacks and Latinos represent 51 and 33 percent of the stops, while only comprising 26 and 24 percent of the total population, respectively, these stops were characterized by a significant disconnection from actual criminality: they were much less likely to lead to arrests than stops of whites, a pattern that strongly suggests blacks were being stopped despite little evidence of criminal wrongdoing (Gelman, Fagan, and Kiss 2007, 816, 822). Although various legal and investigatory justifications were often given in support of SFQ, the racial disparities in police stops have often been linked to little more than race, where the only “crime” in play at the time of the stop was “racial incongruity” with the location in which one was

stopped (being black in a majority white area or vice versa) or racial belonging (Capers 2009; Meehan and Ponder 2002). Two-thirds of all police stops failed to meet the “reasonable suspicion” standard, particularly when blacks were stopped. “Racial composition,” Jeffrey Fagan and his colleagues argue, was “as important as local crime conditions in predicting police stop activity” (Fagan et al. 2009, 330). Indeed, if one looks at the reasons police gave for making the stops, the connection to crime was dubious; many were stopped for making “furtive movements” or being in a high-crime area, a designation that itself was “virtually uncorrelated with actual crime rates,” for instance (Lerman and Weaver 2014; Grunwald and Fagan, forthcoming).

Strikingly similar patterns were found in police stops of motorists, though with a notable twist. In *Pulled Over*, Charles Epp and his colleagues document the emergence, acceptance, and eventual institutionalization of a new kind of stop tactic in police departments—the investigatory stop—that which is not intended to stop crime but to use any pretext to “merely check people out.” As police training manuals described, stopping a surplus of people was an explicit goal: “you have to stop a lot of vehicles to get the law of averages working in your favor” (quoted in Epp, Maynard-Moody, and Haider-Markel 2014, 39). Using this kind of stop to observe as many people as possible became accepted, even celebrated. Unlike traffic safety stops, which show some parity, the racial disparities in these investigatory stops were large, leading the authors to conclude that drivers were stopped less by “how they drive” than by “who you are” (25).

Studies in a variety of locations and contexts find that minority drivers are stopped at a disproportionate amount relative to their total composition of the driving population (Browning et al. 1994; Lamberth 1996; Smith and Perocelli 2001; Baumgartner, Epp, and Shoub 2018). Although the levels of such disparities vary by study—as does who is doing the stopping (local versus state or traffic police)—the consensus is that the disparities in stops by race are so persistent to warrant the act of “driving while black” itself to be viewed as something that is considered criminal, in that

it leads to unequal contact with police. Similarly, Amada Armenta finds that Latino residents in Nashville, Tennessee, are often fearful of driving as it might lead to (now legal) immigration checks (2017). In both instances, enforcement strategies and policies have blurred the distinction between criminality and other ordinary acts such as commuting to work or driving down the expressway.

A final example merits attention. A recent study of adolescent boys calls into question the strength of the connection—and even the directionality of the relationship—between arrests and offending in recent years. It demonstrates not only that police contact was likely to trigger subsequent offending in a sample of adolescent boys, but that self-reported delinquency bore no relationship to subsequent police contact and prior law-abidingness did not “protect” them from police stops: “at each wave of the survey, boys who reported little or no involvement in delinquency at the prior wave were just as likely to have been stopped by police six months later as boys who had reported higher levels of delinquent behavior at the previous wave” (Del Toro et al., n.d.).

A second dimension to our decoupling argument often evades academic discussion: the lack of contact with criminal justice among some who report criminality. In contrast to the experiences of black and Latino youth who are treated as suspicious for “offenses” as mundane as walking down the street or driving in a car, the relationship between criminal offending and criminal justice contact is problematic in the opposite direction in more affluent and whiter suburbs and college campuses, the so-called antitargets of the war on drugs (Richards, Berk, and Forster 1979; Singer 2014; Jacques and Wright 2015; Wooden and Blazak 1995; Mohamed and Fritsvold 2010). A study of Amherst, New York, a wealthy, largely white suburb outside of Buffalo, finds that police practice a “maximum tolerance” rather “zero tolerance” approach to youth offending (Singer 2014, 239). Generally, Amherst police had a high level of tolerance for low-level offending often treated along the lines of the old adage “boys will be boys.” Simon Singer concludes that though as many as 64 percent of the youth in the study could have been arrested based on

their delinquent behavior, few were (238). Although 22 percent had been picked up by police, only 10 percent of them had been adjudicated in some way for their behavior (238). Additionally, young people living in affluent white suburbs such as Amherst are unlikely to be arrested for possession of small amounts of pot (Singer 2014, 21; Hagedorn 1994). Such maximum tolerance approaches in suburbs thus is also implicated in a decoupled relationship between offending and contact in these communities but in the direction of a false negative, or underenforcement and diminished probability of contact among offenders.

The central objective of our study is to determine whether and how the basic relationship between criminality and criminal justice contact has shifted across the past several decades. To this end, we use nationally representative data from two cohorts, young adults in the 1970s and young adults in the 1990s, to compare their reported experiences with both criminal offending and criminal justice contact. The timing of these two cohorts provides a unique lens into the experiences of those living under very different criminal justice regimes. The 1970s cohort was surveyed prior to the war on drugs and just as the institutional changes of the late 1960s war on crime were being reflected on the ground. The 1990s cohort entered young adulthood in a policy era characterized by broken windows policing, increased prosecutorial activism (Pfaff 2017), and a sweeping set of legislative changes that together bent the criminal justice system toward a focus on low-level or non-offenders (National Research Council 2014).

Figure 1 provides a conceptualization of our theoretical and analytic approach by depicting a binary distinction between a respondent's self-reported criminal activity (whether they engaged in an illegal offense) against a respondent's experience of criminal justice contact. The top left yes–yes quadrant of figure 1 is the area assumed by most: the vast majority of those involved with the criminal justice system have been so engaged because of their involvement in criminal behavior. The no–no quadrant provides the logical opposite: lack of criminal justice contact is generally associated with lack of criminal involvement. The yes offending–no

contact quadrant is, essentially, a system failure—the inability of the criminal justice system to contact those actually committing the crimes. The no offending–yes contact quadrant is a different type of system failure, and the one of interest in this study: individuals who are contacted by the criminal justice system without engaging in an illegal activity. The arrows depict movement from a criminal justice system tightly connected to offending. Our argument is that, over time and particularly among the generation coming of age before and after one of the largest transformations in criminal justice to date, the share of Americans shifted between these quadrants in ways that are troubling for a system predicated on few “errors” (few yes–no combinations)—and that this maladaptation is heavily skewed by race.

DATA AND METHODS

To explore our argument, we rely on the National Longitudinal Survey of Youth. The NLSY asks a nationally representative survey sample of more than eight thousand young adults questions about their criminal offending and direct experiences with the criminal justice system in two generational cohorts, one that turned eighteen around 1980 and one that did so in the late 1990s.² The NLSY includes a detailed battery of self-reported offenses as well as several measures of reported contact with police, courts, and correctional institutions. Despite some differences in question wording between the two years of the survey and its reliance on offense and arrest self-report, which we discuss in detail in the following section, and a slightly smaller sample in the later co-

hort, the NLSY is an ideal data source for making comparisons across cohorts, given that it kept its sampling procedures identical across the cohorts, includes large oversamples of nonwhites, and has high response rates in the 82 percent to 93 percent range (Stevens and Morash 2014).³ Using a self-weighting representation of households with youth between ages twelve and sixteen in 1996 and between fourteen and twenty-two in 1978 yields cohort samples that are representative of the American young adult population.

Following existing approaches to ensuring the cohort samples are as similar as possible, we remove the military and low-SES oversamples from the 1979 cohort and exclude respondents under eighteen in both cohorts. After that adjustment, 5,837 respondents remain in the 1979 cohort analysis and 8,683 in the 1997 analysis. We examine the 1980 wave, the only year of the 1979 NLSY that includes queries about criminal justice contact; we use the 2002 wave of the 1997 NLSY, the year that most closely approximates the age distribution of the 1979 cohort analysis (once those under eighteen are removed). About 1,008 of the original 1997 cohort were not available for re-interview in 2002, and 395 of the original 1979 cohort were not in 1980. We assume that these cases are missing at random, an assumption consistent with past research using NLSY (Brame et al. 2014).⁴

Our primary outcome of interest is arrest, a critical entry point into the criminal justice system and experience with police and, as others have argued, a key mechanism for further involvement and embeddedness with the crim-

2. The NLSY was not originally designed for research on crime and delinquency, but its nationally representative sample—especially with significant numbers of racial and ethnic minorities—has produced a string of important criminological investigations into a variety of theoretical and methodological topics, including several of those discussed here.

3. As Tia Stevens and Merry Morash explain, “Both have a similar questionnaire design, a sample based on birth year using similar sampling methods, and oversampling of Black and Hispanic youth to allow for reliable statistical analysis for these subgroups” (2014, 80).

4. Robert Brame and his colleagues conducted sensitivity analysis using various estimators (lower and upper bounds) to ensure that those respondents missing from the analysis either because they weren’t interviewed in that round or because they did not answer the arrest question did not bias the prevalence estimates by subgroup (2014). They find that even if the missing cases were not missing at random, “only an extreme difference in the missing data patterns” of blacks and whites and of men and women “could overcome the difference we see in the observed data” (480).

inal justice system (Sampson and Laub 2003). Arrest, even absent formal conviction or adjudication, has been linked to several disconcerting outcomes including less earnings, unemployment, lowered educational attainment, and greater risk of dropping out of school (Grogger 1995; Bushway 1998; Uggen 2000; Bernburg and Krohn 2003; Blumstein and Nakamura 2009). As one recent study bluntly put it, “the collateral social and personal damage created by an arrest mortgages the futures of young people as they make the transition to adulthood” (Brame et al. 2012). Unfortunately, only the NLSY 1980 data include measures of being stopped by police (without arrest), so we cannot track changes over time in this lower-level contact that may show even less connection to criminality given the rationales of broken windows policing. We use arrest as a proxy for contact, given that it is asked in both cohorts and a large enough share of each cohort and by racial group experienced arrest to make analysis of subgroups possible. We focus on the “front end” of experiences with criminal justice, and future research should consider other points of contact. To compile the arrest measure, we use the initial question in 1980 and 1997 about whether the respondent has “ever been” arrested, booked, or charged. For the 1997 cohort, we also rely on the follow-up questions in each later round (up to and including 2002) of whether the respondent has been arrested “since the date of last interview.”⁵ Although some respondents are missing from subsequent rounds of the 1997 NLSY, we use the NLSY’s event history arrest measure.

Our main explanatory variable is self-reported criminal offending in the previous year measured in both cohorts. Respondents are asked whether they committed one of several offenses since the date of last interview

(that is, over the past year) as well as the number of times the act was committed. Offending in one year has a strong correlation with offending in other years and in the absence of explicit longitudinal measures, can be assumed to reflect prior offending (Stevens and Morash 2014; Jolliffe et al. 2003; Herrenkohl et al. 2000). Because the 1980 NLSY does not query respondents about prior criminal offending in 1980 or include measures of offending in the initial 1979 wave of that cohort survey, we use a one-year measure in both. The criminal offending profiles of the two cohorts show substantial divergence. On balance, a much larger share of the 1979 cohort reported doing at least one unlawful act. Among the 1979 cohort, 52 percent reported one offense in the last year (not including drug use; including drug use, the share is 56 percent); in comparison, only 15 percent of the 1997 cohort reported engaging in at least one illegal offense (if drug use is included, that share rises to 31 percent).

Our first measure of self-reported offending is a dichotomous measure that equals 1 if the respondent reported any of the offenses excluding drug use and a 0 if they reported no illegal acts excluding drug use.⁶ We further divide the crime measure into reported property and violent offenses, again as dichotomous measures. For a subset of analyses, we use a continuous measure of self-reported offending based on an index of crime severity and frequency.

Beyond offending profiles, few differences between the respondent samples in each cohort in terms of gender, age, region, and urbanicity are notable; in both the 1979 and 1997 cohort, about half of the sample are non-Hispanic whites, and roughly similar proportions are Latino and black. College enrollment and the share in poverty were both higher in the later cohort and a larger share lived in the central city in the later cohort. Respondent

5. Although our main analysis relies on a cumulative measure of arrest among young adults in the NLSY, in the appendix, we replicate the results using a more limited arrest measure, namely, arrest in the last year alone. Doing so attempts to deal with the concern that our measure is biased towards finding greater arrest prevalence in the later cohort given multiple opportunities to report in 2002 relative to 1980 and addresses the mismatch between measuring cumulative arrest and noncumulative offending.

6. Our measure excludes items that were not asked in both cohorts—gambling, fighting, threatening to hit someone, and membership in a gang.

demographics by cohort are presented in table A2. For most of the analyses to follow, our focus remains on a comparison between black and non-Hispanic white respondents. The reason for this is simple: the Latino population in 1979 is not comparable to that in the late 1990s for many reasons, the most important being that the 1982 and 1993 immigration acts changed migration patterns, bringing many more low-skilled immigrants and many more of Mexican descent.

Scope of Study and Limitations

Although the NLSY was not explicitly designed for the study of crime, its inclusion of questions on self-reported delinquency and arrest has made it an important source of data to examine a range of criminological phenomena such as self-control theory (Hay and Forrest 2008), the relationship between gang membership and drug use (Bjerregaard 2010), and the effects of dropping out of school on delinquency (Apel et al. 2008). Our study builds on and advances prior work using the NLSY to study crime and delinquency in two important ways. First, we focus on shifts occurring between cohorts rather than on cohort-specific behaviors. Several recent studies have used the 1979 and 1997 NLSY to compare outcomes and expectations across cohorts. These include behaviors such as the changing skills of youth and labor market outcomes (Altonji, Bharadwaj, and Lange 2012, 783); the role of education in determining wages (Castex and Dechter 2014, 689); inequality in postsecondary education (Bailey and Dynarski 2011, 1, 19); the changing effect of family income and ability in education achievement (Belley and Lochner 2007); changing college expectations (Reynolds and Pemberton 2001); high school dropout rates in urban and rural areas; and the association between dropout rates and paid employment during high school (Jordan, Kostandini, and

Mykerezzi 2012; Warren and Cataldi 2006). Although some note difficulties in comparing specific variables across surveys and the change from pencil and paper to computer-assisted instruments, these studies provide evidence of the reliability of making cohort comparisons using the NLSY once attrition is accounted for through survey weights. Despite recent attention to the cumulative prevalence of arrest among today's youth, our study is one of the first to take a similar cross-cohort perspective on arrest and criminality.⁷

Second, instead of exploring criminal offending or contact with legal authorities in isolation, we examine the relationship between self-reported offending and arrests. Only two studies to our knowledge have used the NLSY cohort comparisons to examine patterns in justice system involvement (conditional on delinquency) among adolescent youth, finding that minority youth were more likely to be convicted and confined after accounting for offending and that this disparity grew over time (Stevens and Morash 2014, 77; Stevens, Morash, and Chesney-Lind 2010).

Our study is not without limitations, though we took steps to minimize their impact on our analyses and inference. First, relying on a population survey not designed for the specific task of measuring and tracking offending and contact with legal authorities raises some concerns. Such a survey likely underrepresents offenders as well as those currently incarcerated (though the NLSY makes efforts to re-interview in correctional facilities), so our estimates of criminal justice contact and offending are likely to be more conservative than what actually exists in the United States. In addition, our examination of self-reported offending and arrest rely on measures that are not as expansive as we would ideally like; delinquency measures exclude more serious offenses like rape and vehicular

7. A set of studies by Brame and colleagues analyze the cumulative prevalence of arrest in the NLSY 1997, finding that a commanding share of American youth were arrested at least once by young adulthood (2012, 2014). Roughly 30 percent of the cohort had experienced arrest by the time they reached twenty-three years old. Arrest risk was extremely high for black young men, just shy of half (48 percent) by age twenty-three and 30 percent by age eighteen, versus 38 percent and 21.5 percent for white young men. Our study differs from Brame's by focusing on arrest by a given year rather than cumulative arrests by a certain age.

homicide and do not represent all index and non-index offense possibilities.⁸

Second, our analysis proceeds from the assumption that our measures of offending and arrest are reliable and valid; that they are equally so for both blacks and whites; and that they do not become more or less valid depending on the cohort or period. The analysis of self-reported delinquency and arrest measures is a mainstay in criminological research, and despite its unique advantages, it is also not without limitations. Because our key explanatory and dependent variables rely on subjective recollections of arrest and offending that are not validated in official records, they may contain measurement error that could bias results and our conclusions. This error is of two sorts. On the delinquency measure, respondents may misreport actual offenses as not being a crime (or vice versa), have difficulty recalling the frequency or severity of offenses, or face social desirability incentives to underreport their offending behavior. On the arrest measure, respondents may misremember the age, timing, or frequency of arrest, confuse a police stop for an arrest and thus overreport arrests, or face similar social desirability concerns to conceal their contact with legal authorities, resulting in biased reports. These possibilities have been the subject of extensive scholarly debate in criminology. We discuss each in turn.

A central debate surrounds the relationship between self-reported arrest as compared to official arrest records.⁹ In general, in reviews of the many studies in this domain, there is “moderate” to “moderate-to-strong” agreement in the reliability and validity of self-reported measures of arrest vis-à-vis official records (Thornberry and Krohn 2003; Piquero, Schubert, and Brame 2014), and congruence between self-reported and official arrests is stable over time (Piquero, Schubert, and Brame 2014). Validations have occurred across both general and serious offending samples and in a host of datasets: The Pathways to Desistance, the Project on Human Development in Chicago Neighborhoods, The National Longitudinal Study of Adolescent to Adult Health, the Seattle Social Development Project, the Pittsburgh Youth Study, The Dunedin Longitudinal Study, the Cambridge Study in Delinquent Development, and the National Youth Survey Family Study (Bersani and Piquero 2017; Hindelang, Hirshi, and Weis 1981; Krohn et al. 2013; Maxfield, Weiler, and Widom 2000; Farrington et al. 1996; Thornberry and Krohn 2000; Pollock et al. 2015; Piquero, Schubert, and Brame 2014). The findings from these studies suggest that self-reports are a “fairly good representation of official reports” of petition-arrest, capturing approximately 80 percent of arrests in arrest records and exhibiting a high level of agreement in both the prev-

8. The lack of some index crimes makes it difficult to compare these self-report arrests with official UCR data, which is generally tracked based on index crimes.

9. For a review of this and related debates, see Terrence Thornberry and Marvin Krohn (2003), Delbert Elliot (1995), and Alex Piquero and his colleagues (2014). “The overall validity of self-report data is in the moderate-to-strong range, especially for self-reports of being arrested” (Thornberry and Krohn 2003, 61). As Elliot describes:

Self-reported data have their own sources of error and should not be accepted uncritically. . . . But conceptually and operationally they are more appropriate measures for studying the causes of criminal behavior and describing the distribution and dynamics of criminal behavior in a general population. Subject to some variation, the validity of self-reported offending based on “known” arrests is about 80 percent. Validity of arrests based on known self-reports is as high as 25 to 50 percent for serious offenses and as low as 1 percent for minor offenses. Given that arrest and self-report data produce different distributions of offenders and offenses in the general population and specific subpopulations, self-reports are likely to produce the better estimates. (1995, 3)

More recently, Alex Piquero and his colleagues conclude that “the high level and stability of the agreement is striking and adds to the emerging story about the validity of these two methods for measuring arrest” (2014, 547, and, for a comprehensive summary of the studies that have assessed concordance between official records and self-reports, see table 1).

alence and frequency of arrest (Piquero, Schubert, and Brame 2014; Pollock et al. 2015). Michael Maxfield, Barbara Weiler, and Cathy Widom, for example, found that 47.5 percent of those in their sample of 1,196 young adults from a Midwestern metropolitan area had an arrest officially recorded by authorities, close to the 45.6 percent who self-reported an arrest (2000, 98).

More recent studies have gone beyond simple correspondence to explicitly match self-reported arrests to official arrests (Farrington et al. 2010; Hirschfield et al. 2006). For example, Nancy Morris and Lee Ann Slocum systematically investigated self-reported arrest errors in a sample of 350 women in a jail and found an extremely high degree of congruence between whether an arrest was reported and whether it was officially documented: 88 percent of women who reported that they were arrested over a period of three years could be matched to official arrest (2010). Recall of the frequency of arrest also exhibited high levels of matches to official data, and to a lesser extent, the timing of arrest: between 35.1 and 39.9 percent of reported arrests were recalled accurately to within just a month of the official arrest date.

In general, where measurement error was evident, it was most likely to be in the direction of overreporting of arrest by those who did not have official arrest documented (most of those with an arrest accurately report being arrested), misidentification of the date or age of the arrest (accuracy erodes for arrests further in the past), and errors in arrest frequency for those who reported more frequent arrests; accuracy was lowest among those with more trivial offenses or adjudication outcomes and accuracy highest among serious offenders and adult relative to juvenile offenders (Pollock et al. 2015; Krohn et al. 2013; Morris and Slocum 2010; Huizinga and Elliot 1986; Elliot 1995). Thus, the preponderance of studies, we and others find,

point to self-report of arrest as a valid and reliable indicator.¹⁰

The validation of self-reported offending is much more difficult because no objective measure of offending exists.¹¹ Although many studies find a positive correlation between self-reports of offending and arrest, for obvious reasons (selection for arrest is not the same as delinquency) it is less strong than findings between self-report arrests and official arrests. Criminologists have rightly questioned, and some have abandoned, the practice of using arrest to understand the dynamics and distribution of offending or to generalize to criminals in the population, one calling it “indefensible” (Elliot 1995, 9). Instead, they tap official responses to offending and the discretion of agencies. Official arrests neither do an adequate job at describing the incidence and distribution of offending in the population, which is far more extensive in self-reports, victimization surveys, and crimes known to police, nor adequately capture the individuals who self-report offending. Most offenders are never arrested and most crime is never reported, and the probability of “arrest per self-reported serious violent offense” is shockingly low (2 percent). Specifically, the correlations between arrests for index crimes and self-reported index offending rates are small, hovering around 0.38, and arrest rates explained just 9 to 14 percent of the variation in offending based on self-reports (Elliot 1995). Arrest rates are not necessarily accurate predictors of offending patterns nor do they accurately distinguish offenders from non-offenders. Even the “worst offenders” based on official arrests bear almost no relationship to the worst offenders based on self-reports, with more than 75 percent of one group missing from the other (Elliot 1995). Offense patterns and estimates of the prevalence of offending by demographic group based on both sources of data look remarkably different

10. One notable exception is a study of Chicago youth by David Kirk, which finds that 45.5 percent of youth who had an official arrest did not report an arrest and 23.4 percent who were not arrested reported that they were (2006). Nonetheless, Kirk concludes that self-report measures can serve as a reliable indicator of actual arrests particularly when trying to explain between person differences or when comparing group differences and not “within-individual change” (126).

11. For a trenchant and seminal critique of the field’s tendency to put faith in official arrest as an unbiased indicator of actual offending, see Elliot 1995.

(Pollock et al. 2015). Knowing arrest history, in short, does not allow one to say much of anything about offending in the population, nor do arrest samples come close to being representative of the population of offenders. On these grounds, we follow a growing group of experts who have argued for relying on offending self-reports as a more suitable method.¹² Readers should note, however, that our measure of offending is subject to errors in recall, flawed understanding of whether something is a criminal offense, and social desirability pressures.

Another concern with implications for our study is differential validity, or the extent to which the correspondence between self-report and actual incidence of arrest or court referral might vary for different groups. Because one of our central theoretical arguments surrounds growing black-white differences in the relationship between offending and contact across cohorts, and our modeling strategy assumes that self-reports of offending and arrest are equally valid, caution is warranted given the spectrum of different findings about the validity of self-reported offending and criminal history by black Americans. Disagreement across studies exists about whether and how extensive a problem differential validity is. Some studies find evidence of substantial differences in validity, and thus challenge our assumption (Hindelang et al. 1981; Huizinga and Elliot 1986; Maxfield, Weiler, and Widom 2000; Kirk 2006). Other studies contradict them, finding little significant variation by race and a strong agreement between self-reported and official records obtained regardless of race or ethnic group (Farrington et al. 1996; Bersani and Piquero 2017; Jolliffe et al. 2003; Piquero, Schubert, and Brame 2014; Thornberry and Krohn 2003; Piquero and Brame 2008).¹³

The studies finding systematic underreporting of offending by blacks were based on a concordance strategy using an unrepresentative sample of local arrest records that assumed no differential validity by race in official arrest records (Huizinga and Elliot 1986; Hindelang et al. 1981). As Elliot contends, this “assumption [was] seriously challenged by Geerken (1994) who concluded that there were serious racial biases in local arrest records which overstate the arrests of blacks relative to whites” (1995, 7). Many other studies using different methods to assess validity of self-reports from polygraph tests to peer reports of offending and others, Elliot goes on to observe, “have all failed to show significant race differences” (1995, 7).

Second, and more important, is that in all of the studies that support differential validity, the direction of bias was in underreporting, not overreporting; in other words, blacks with a criminal record were more likely than whites to underreport offenses (Hindelang et al. 1981; Huizinga and Elliot 1986) and arrests (Kirk 2006; Maxfield, Weiler, and Widom 2000; Krohn et al. 2013). Positive bias—reporting an arrest when there was no official arrest—when it occurred was more likely among whites (Maxfield, Weiler, and Widom 2000; Krohn et al. 2013). Thus, if differential validity of arrest self-reports by race is a problem in our study, it will lead to bias our results in a conservative direction. If differential validity of offending (given greater underreporting of offenses among blacks) is a problem, there is little reason to believe that offense underreporting would not affect both the earlier and later cohorts *and* that it would be accompanied by the simultaneous underreporting of arrests. In both scenarios, the conclusions we reach about a changed relationship between offending and contact are unlikely to be exaggerated.

12. “Although the two measures are positively related, as we would expect, the two cannot reasonably be regarded as measures of the same phenomenon, and it is self-reports, not arrests, that provide the more complete picture of illegal behavior” (Pollock et al. 2015, 70; see also Elliot 1995). And, given the limits of official administrative data and differences in reporting across jurisdictions, “the best option currently available is to rely on self-reported survey data” (Brame et al. 2014, 482).

13. One study not only failed to replicate findings of underreporting by blacks, but also found that “black males generally had the *highest validity* in these analyses” (Jolliffe et al. 2003, 194, emphasis added). Similarly, Alex Piquero, Carol Schubert, and Robert Brame find that the “correspondence between the prevalence estimates for the two arrest measures appears to be consistently higher for Blacks” than for whites and Hispanics (2014, 541).

A final issue related to the reliance on self-reports for testing claims of cross-cohort shifts is that self-reports may themselves exhibit a cohort effect, that the manifestation of under- or overreporting depends on the era or period in question. Studies have found that under- or overreporting of arrests remains remarkably consistent over the adolescent through young adult life course (Emmert et al. 2017; Piquero, Schubert, and Brame 2014). Research addressing whether self-reporting behavior remains unchanged across cohorts, a crucial assumption on which our analysis rests, is conspicuously lacking, however. One possibility is that perceptions of arrest differ across the periods; given proactive policing tactics that made stops more common in the later period, perceptions about what constitutes an arrest may have been subject to more confusion (Piquero, Schubert, and Brame 2014; Pollock et al. 2015).

It is also possible that the self-reporting of offending may have been more prone to social desirability bias in the later NLSY cohort, given the politicization of crime and drugs in the 1980s and 1990s and their accompanying stigmas in political discourse. If true, then we would expect the crime measures to be biased in the direction of less reported offending among more recent cohorts, a trend that was an artifact of social desirability concerns and not representative of actual offending in the young adult population. However, on this logic, such bias should also have affected reporting of arrests by the later survey cohort. Thus, if social desirability bias was driving cohort-specific underreporting of offending and contact with legal authority, we should see more pronounced underreporting of both arrest and offending in the later NLSY cohort. It is highly

unlikely that we would see both overreporting of arrest concurrent with underreporting of offending among the later cohort. More likely would be underreporting of both. Social desirability bias, therefore, may lead to artificially lower levels of both arrest and offending in the later period, but would not pose significant problems to our decoupling argument or finding of a changed relationship between offending and arrest.

With these limitations acknowledged generally and with attention to how our analysis specifically might suffer from bias introduced by the deficits of the self-report method, we proceed cautiously. Fortunately, the design of the NLSY is helpful in this regard; arrests are collected using a life event calendar method, which has been shown to reduce recall error; in addition, each arrest event is followed through to adjudication outcomes, which increases the likelihood that the data provide accurate estimates (Morris and Slocum 2010). Our decisions in the construction of our NLSY merged cohorts dataset were designed to specifically minimize these potential limitations and biases. To minimize the danger of false positives of arrest in the self-reports, our measures of arrest exclude those who cannot recall the year of their arrest, thus producing a conservative estimate of arrest by excluding those who offer hazy arrest details.¹⁴ To address the possibility of greater self-reporting of arrest in the 1997 cohort, we conduct additional analysis among only those who reported an arrest and an official charge.¹⁵ We also confine our analysis to respondents in their early adult years. The logic is that adolescents are more likely to mistakenly recall arrest given greater ambiguity in police practices (Pollock et al.

14. According to the NLSY, “if respondents cannot provide the arrest date (both month and year) or the year of the arrest, the arrest is not populated in the arrest event history array.”

15. If they reported that they had been arrested, respondents were asked several additional questions about whether the police had formally charged them with a specific offense. In 2002, 387 respondents (4.3 percent) reported being charged with an offense in the last year. Using a cumulative measure based on this item in prior waves, 1,558 respondents (17.4 percent) reported being charged with an offense. Using this more restrictive measure, a smaller share of respondents in 2002 reported ever having a charge or having a charge in 2002 (relative to the original arrest measure, which included 25 percent of respondents who “ever” had an arrest and 6 percent who reported being arrested in the last year). Using this measure is likely an underestimate of arrest relative to the 1979 cohort.

2015).¹⁶ When validity in self-reporting behavior differs by race, it “disappears in the early adult years” (Elliot 1995; Pollock et al. 2015; Thornberry and Krohn 2003). Finally, our analysis does not depend on the details of the arrest—when it occurred, how frequently, for what offense, or even what took place as a result—just whether it took place; thus, we are less concerned about self-report errors that arise from the inability of respondents to locate their arrests in time or recall frequency.

Question Wording and Sample Design Differences

Our analysis is also based on the viability of comparing the 1979 and 1997 NLSY cohorts. Some minor changes in the survey instruments and design occurred between the 1979 and 1997 cohorts, raising potential concerns about comparisons. Of note, in the 1997 survey, respondents were asked a binary yes or no question for each offense, and then asked the number of times they committed particular acts, whereas in the 1979 survey respondents were only asked the number of times they committed a particular delinquent act on a scale of 0 (never) to 6 (more than fifty times). For our analysis, we recode the 1997 data into a frequency item so that it lines up with the 1979 data. It may be that the framing of the response choices elicited more self-reported delinquency in 1979 than 1997. Additionally, a few other questions were only asked of respondents who said they had stolen something valued at more than \$50 in the 1997 survey (such as a question about joy riding), whereas in the 1979 cohort this question was asked of all respondents and may have elicited more responses.

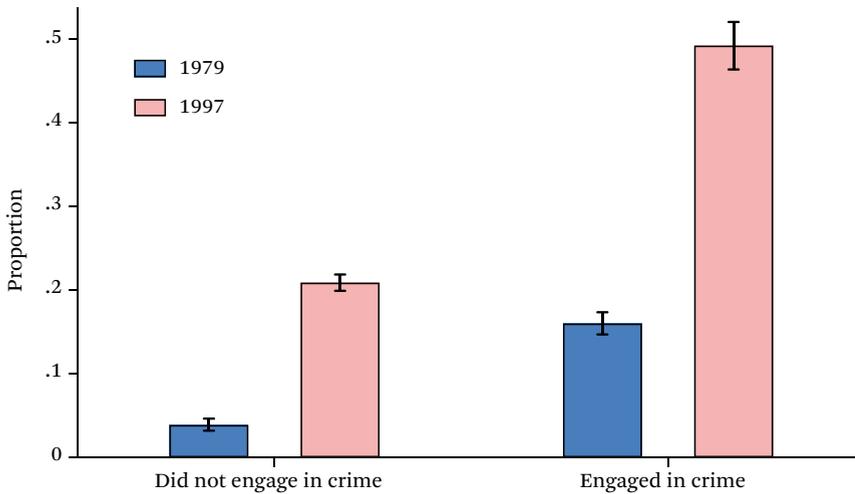
Another difference is that more crime questions were asked in 1979 than in the 1997 cohort, which year tends to collapse the same information into fewer queries. In 1979, respondents are explicitly asked whether they broke into a building to steal something as well as about auto theft and shoplifting. In 1997, only those who reported stealing something were subsequently asked whether they stole by

breaking and entering or whether they stole a car. In addition, whereas 1979 respondents are asked about using force to obtain things, only those who reported stealing are asked whether they used a weapon in 1997. Finally, 1979 respondents are simply asked about conning someone and about knowingly holding or selling stolen goods, but 1997 respondents are asked whether they engaged in other property crimes.

Although the wording of the items related to offending does differ in important ways between the 1979 NLSY and the 1997 NLSY, we do not think that offending-pattern differences are merely an artifact of question wording or respondents being asked more items in regards to offending behavior. For example, studies based on the Monitoring the Future survey of youth document similar trends in delinquency that support higher criminal offending among the 1979 cohort (Keyes et al. 2017). If we compare the self-reported criminal offense measures item by item, and focus on items that have very similar question wording, a larger share of the 1979 cohort consistently reports unlawful behavior than the 1997 cohort (for the raw share of each cohort that self-reported each of several offenses, see table A2).

Queries on contact with legal authorities also show some differences in wording between the two cohorts. In 1980, respondents are asked, “Not counting minor traffic offenses, have you ever been booked or charged for breaking a law, either by the police or by someone connected with the courts?” In 2002, they are asked, “Have you ever been arrested by the police or taken into custody for an illegal or delinquent offense (do not include arrests for minor traffic violations)?” Because of the different question wordings used to compile our arrest measure, we examine whether the results are sensitive to different measures. Because the question in 2002 is not as specific as it is in 1980, it is possible that the slight wording difference in 2002 was more prone to overreporting of arrests. To address this possibility, we examine how the results hold up if we use the

16. For example, police in one study indicated that they would sometimes decide “to file the case as arrest after dropping the individual off so the adolescent might not know whether he or she was arrested” (Pollock et al. 2015, 78-79).

Figure 2. Odds of Arrest by Self-Reported Offending and Cohort

Source: Authors' compilation based on the NLSY (Bureau of Labor Statistics 2014, 2015).

Note: Self-reported crime does not include drug use. Analysis limited to those respondents at least eighteen years of age. The 1979 cohort analysis excludes military and low-SES white oversamples. Analysis is unweighted; analysis with weights is not different.

more conservative measure: “Did the police charge you with an offense?”

It is also possible that respondents in the 1997 cohort misreported being stopped by police as an arrest (Pollock et al. 2015), especially since NLSY 1997 did not separately query respondents specifically about police stops (as distinct from formal arrests or charges) as it had for the NLSY 1979 cohort. Unfortunately, we know of no studies that examine the link between police stops and arrest perceptions, though some do suggest that confusion is likely (Elliot 1995). However, as discussed, we use a more conservative measure of arrest (being charged) and replicate our findings.

Other changes, such as a switch from PAPI (paper and pencil interviewing) in the 1979 cohort, to CAPI (computer-assisted personal interviewing) in the 1997 cohort, may have increased the level of delinquency reported among the 1997 respondents. Despite the potential that these changes altered the overall self-reported delinquency between the cohorts, we have no reason to suspect that these changes would have differentially affected white and black respondents. In other words, we have no reason to suspect that these changes would im-

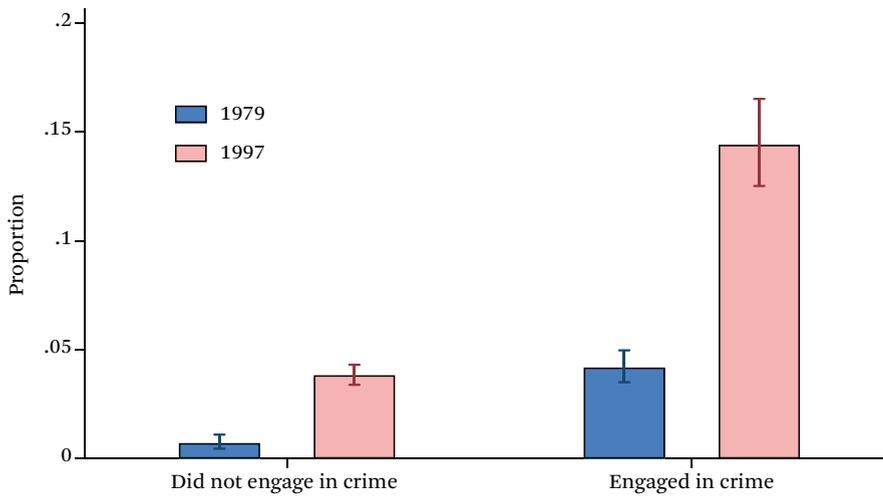
pact our findings regarding the differences in predicted probability of arrests for blacks and whites between the two cohorts.

RESULTS

We begin with basic plots of bivariate relationships of arrest outcomes for those who reported being engaged in crime and those who did not by cohort and race. Next, we turn to a multivariate investigation of the cohort data. Instead of distributions, we explore the importance of self-reported offending as a predictor of arrest in each cohort and by racial group. We examine whether the influence of offending differs by cohort and by racial group within and across cohorts. Finally, we return to the quadrants from figure 1 using a multinomial logit to examine the odds of landing in each quadrant.

Bivariate Results

The bivariate relationship between arrest and offending among the two NLSY cohorts offers a first descriptive consideration of our argument (see figure 1). Figure 2 plots the basic odds of arrest separately by self-reported criminal offending for both cohorts. As it clearly

Figure 3. Odds of Incarceration by Self-Reported Offending and Cohort

Source: Authors' compilation based on the NLSY (Bureau of Labor Statistics 2014, 2015).

shows, the 1997 cohort shifts decidedly; a greater share of young adult Americans are having contact despite not reporting criminal involvement. Indeed, the share of NLSY respondents in the later cohort who reported no offending and had been arrested was larger (0.21) than those who *had* done something unlawful but evaded arrest in the earlier cohort (0.15).

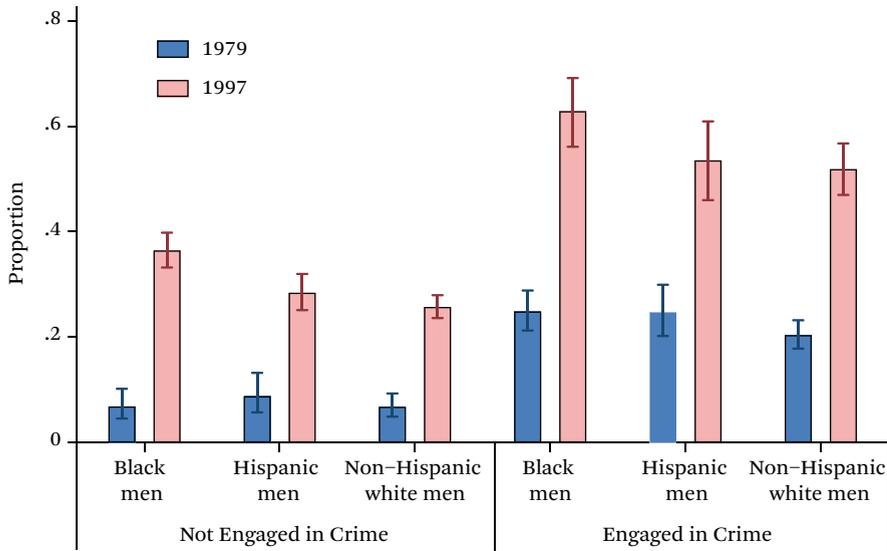
Figure 3 repeats the basic premise of figure 2 but with a more punitive form of criminal justice contact: incarceration. As with arrest, figure 3 shows that the 1997 cohort experiences a greater level of confinement without reporting offending than the 1979 cohort: 0.04 percent of the 1997 cohort versus 0.01 percent of the 1979 cohort reported having been confined without reporting committing an offense. Moreover, the level of contact is so much higher among the later cohort that, among those engaged in crime, their odds of being incarcerated are roughly equivalent to the odds of arrest for the 1979 cohort.

Finally, figure 4 disaggregates the odds of arrest by race and gender to detect any bivariate relationship. Two important patterns emerge from figure 4. First, the decoupling of criminality from arrest appears to have affected all members of both cohorts. That is, the black, Hispanic, and white respondents of the 1997 cohort all reported increased arrest in the ab-

sence of reported offending. This suggests that the decoupling of criminality from contact occurred at perhaps a larger scale than anticipated. However, the second pattern seen in figure 4 is that the decoupling of criminality from arrest was largest for black respondents. The increase in the odds of being arrested without having reported criminal involvement between the 1979 and 1997 cohort of black men is roughly 419 percent. Considered another way, black men who do not report engaging in crime in 1997 have larger odds of arrest than their counterparts who do report it in 1979. That outcome is striking and one we return to later.

Multivariate Results

The bivariate results suggest movement toward the decoupling of criminality from criminal justice contact, but multivariate analyses are needed to more fully understand the underlying relationships. The normative argument guiding our analysis is that the relationship between offending behavior and criminal justice exposure should be quite strong such that offending is a primary predictor of arrest, that the relationship should be relatively stable across the two cohorts, and the connection between criminality and exposure to arrest should not diverge substantially by noncrime statuses such as race or education. We might worry, for example, if Americans committing many vio-

Figure 4. Odds of Arrest by Offending, Demographic Group, and Cohort

Source: Authors' compilation based on the NLSY (Bureau of Labor Statistics 2014, 2015).

lent acts were never detained or, conversely, if many Americans were being arrested but had not engaged in illegal behavior. It would raise questions about the function of the criminal justice system if arrest patterns had little to do with offending patterns or if the connection between crime and arrest was arbitrary, dwarfed by other factors that have little to do with breaking the law.

We investigate these issues by merging the NLSY cohorts into one data file and conducting our analysis on the combined file with a year variable differentiating the two cohorts. We model arrest outcomes in a logistic regression where the outcome to be explained is reported arrest and our main explanatory variables are the dichotomous (self-reported) property crime and violent crime measures. To account for slight sampling differences across the cohorts, we control for various demographic and socioeconomic measures: age, gender, race, region, urbanicity, education, family income with miss-

ing values imputed, and leaving school before completing high school (see table A1).¹⁷ The results of these regressions are presented in figures 5 through 9. Full regression parameters are presented in the appendix (see tables A3 through A6).¹⁸

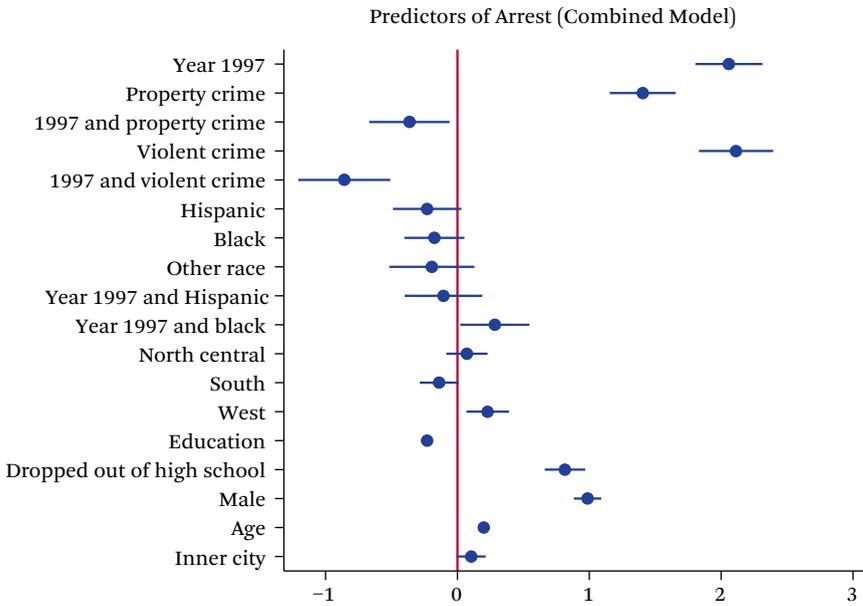
As we can see in figure 5, one of the best predictors of being arrested is engaging in property crime or violent crime; both of these parameters are large and statistically significant. Substantively, this indicates a strong relationship between criminal justice exposure (arrest) and self-report of engaging in crime. As we would expect, gender, age, urbanicity, and education level also affect the odds of arrest. Being in the later cohort (that is, the year dummy) has a very large influence on arrest; respondents have a greater probability of being arrested if they were unlucky enough to be moving through young adulthood two decades after the early 1980s cohort.

But the question that our analysis hinges

17. We also include sampling weights and run the analyses with and without the weights. Results do not depend on the weights; for convenience, we report the unweighted results.

18. We also produced similar results using a more restrictive measure of arrest—being charged—as well as a noncumulative measure of arrest—arrest in last year only (see table A3). Full results for all models are available from the authors on request.

Figure 5. Results of Logistic Regression Predicting Arrest, 1979 and 1997 NLSY



Source: Authors' compilation based on the NLSY (Bureau of Labor Statistics 2014, 2015).

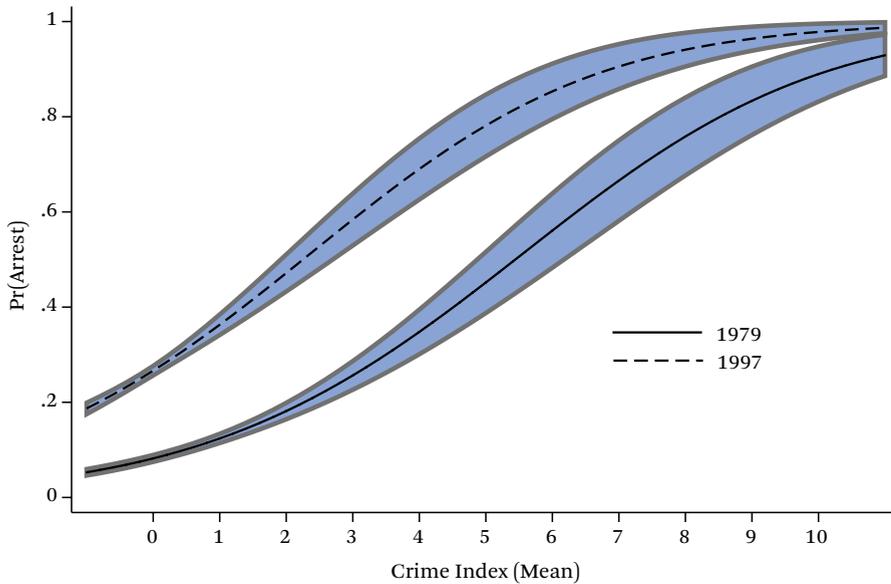
on is whether the influence of criminal offending on arrest likelihood changes across the cohorts. It does, and sharply so. In the logit model predicting arrest, an interaction term that captures the interaction of cohort and self-reported property or violent offending declines in importance for the 1997 cohort relative to respondents in the 1979 cohort. We see this in models with cohort interaction effects as well as if we examine the effects of self-reported offending separately by cohort. For example, for the 1979 cohort, self-reported property and violent offending measures explain 0.08 of the variance in arrest; in 1997, these measures explain 0.05 of the variance in arrest. Committing a property or violent crime (self-report) explains arrest outcomes less well in the 1997 cohort than in the 1979 cohort. That is a concerning dynamic, and one we need to explore further.

Figure 5 also indicates the growing influence of racial membership. A cohort-race interaction term suggests that the influence of being black on arrest grows over time, matter-

ing more for the 1997 cohort than their earlier counterparts.

The analyses thus far provide some support for the idea that self-reported criminal offending and exposure to criminal justice has shifted—decoupled even—over time. The next set of results relies on a different, more elaborate measure of criminal behavior using measures in both iterations of the NLSY. We developed a crime index, a scale of items about the frequency of a respondent's committing one of six crimes—*theft under \$50, theft over \$50, assault, selling drugs, damaging property, and using hard drugs (never, one time, two times, three to five times, six to ten times, eleven to fifty times, and more than fifty times)*.¹⁹ These six items use relatively similar question wording across the two surveys. The alpha is 0.713, indicating that the items load well as a scale. The results that follow rely on the index of all six delinquency items; analyses were also run using each measure of offending separately to ensure that the results were not being driven by one type of offending. To avoid an abun-

¹⁹ Marijuana use is excluded in our index because in 1997 the item uses a different scale (last thirty days instead of last year) than in 1979. Results do not change when marijuana use is adapted for the index, however.

Figure 6. Predicted Probability of Arrest by Offending and Cohort

Source: Authors' compilation based on the NLSY (Bureau of Labor Statistics 2014, 2015).

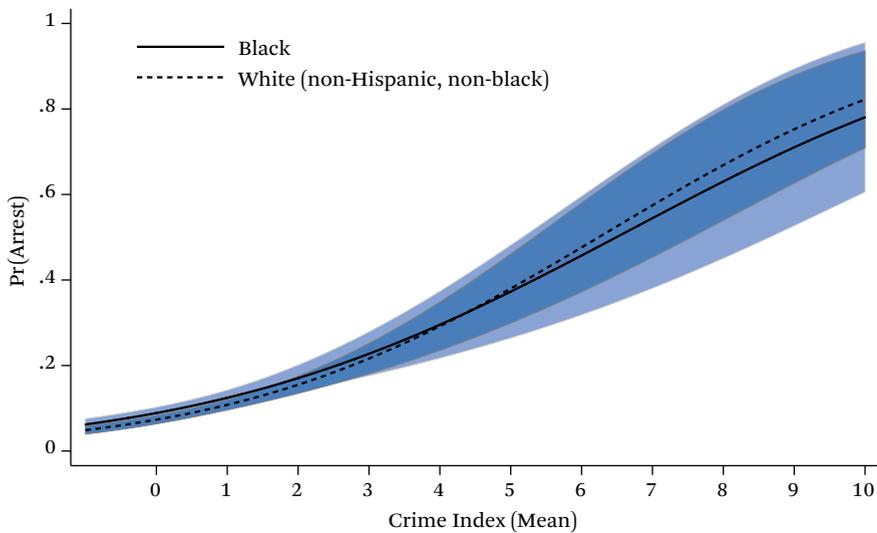
Note: Unweighted. Logistic regressions with controls for age, gender, race, region, urbanicity, education, leaving school before completing high school. Confidence intervals appear in gray.

dance of zeros, we mean-centered the index.²⁰ The resulting measure has a mean value of 0.16 among the 1979 cohort and -0.19 among the 1997 cohort.

Figure 6 plots the results of a logistic regression where generational cohort is interacted with the continuous measure of self-reported offending, the crime index. Rather than plotting the regression coefficients as before, we plot the marginal probabilities of arrest at each level of crime for each cohort separately. The interaction term allows the relationship between self-reported criminal offending and arrest to have a different slope by cohort. The results are striking. Viewed in this way, one sees clearly that not only is the 1997 cohort more likely to be arrested than the 1979 cohort, the lines do not converge at any point on the crime continuum. The relationship between self-reported crime and arrest is indeed different

based on cohort, even if the direction of the relationship (more crime leading to increased probability of arrest) is similar. Moreover, the probability of arrest is higher at every level of self-reported criminality (including no offending) in 1997, and the relationship between self-reported crime and arrest becomes flat sooner. Perhaps the most interesting part of the figure is toward the lower values on the crime index, where the divergence between the probability of being arrested conditional on one's generational cohort is large. For example, committing few to no crimes in 1979 (self-reported) translates into a very low probability of arrest; among those in the later cohort, reporting few to no crimes translates into a much higher risk of arrest, sometimes on the order of 20 percent or more. Thereafter, the odds of arrest among the 1997 cohort grow in a steep line until they level off at around a 7 on the crime index.

²⁰ Because standard models are unable to distinguish between a no-arrest outcome due to no offense and a zero outcome due to other reasons (no enforcement), a zero-inflated Poisson regression was appropriate. Although not presented here, we ran the analysis using a zero-inflated Poisson (ZIP) regression so that we could model separate processes leading to a zero outcome and the diagnostics did not indicate it fitting the model better.

Figure 7. Predicted Probability of Arrest by Self-Reported Offending, 1979

Source: Authors' compilation based on the NLSY (Bureau of Labor Statistics 2014, 2015).

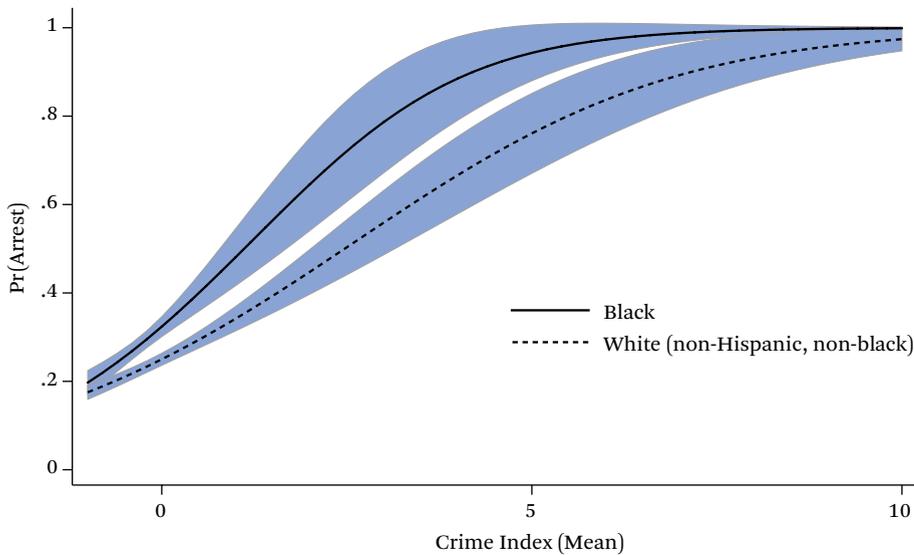
Note: Unweighted. Logistic regressions with controls for age, gender, race, region, urbanicity, education, leaving school before completing high school. Confidence intervals appear in gray.

Among the 1979 cohort, by contrast, arrest probabilities grow more slowly and the steep increase really only sets in after reaching the halfway mark on the index. The cohort lines only begin to converge among those respondents who indicated substantial involvement in crime, but at no point do arrest probabilities mirror each other across the two generations. The 1997 slope takes on a concave downward shape, whereas the 1979 slope is concave upward, at least at the low end of the crime index.²¹

How is this changing relationship between exposure to arrest and reported criminality it-

self interacting with race? This is where we see some of the most convincing evidence that the crime-arrest connection is increasingly fraught and has become more racially inflected over time. Again, we model the relationship in the same way as before but this time we run separate regressions by year with an interaction term to capture the interacting influence of race and the crime index. Figure 7 plots the margins for the 1979 cohort for blacks and whites separately. Strikingly, the lines for blacks and non-Hispanic whites almost completely overlap, indicating that the relationship be-

21. Our finding that one had a much larger likelihood of arrest at low levels of offending or non-offending in the later cohort could be explained by the shift towards probation-related arrests without the commission of a new crime in the later period. Respondents were not asked whether they were specifically arrested for a probation or parole violation until 2008, unfortunately, and they weren't asked this at all in 1979. However, earlier survey waves of the NLSY 1997 cohort do provide a measure of who is currently on probation from prior arrests and convictions. For example, about 332 respondents in the NLSY waves prior to 2002 who were arrested, charged, went to court, and were convicted or pled guilty reported that they were on probation as a result of reported arrests ("Were you put on probation?"). Of these 332, 186 respondents reported no offending since the date of last interview. Of these, only twenty-one respondents reported being arrested in 2002. Thus, the likelihood that our core findings are an artifact of respondents on probation whose new arrests were for probation violations instead of the commission of new crimes is trivial. We repeat the analysis controlling for being on probation in a prior year (that is, those whose new arrest could have been a probation violation) and the results do not change. In addition, the results are nearly identical as before after excluding respondents who reported being on probation in a prior year (1997 to 2001) from the analysis altogether. These results are presented in the appendix.

Figure 8. Predicted Probability of Arrest by Self-Reported Offending, 1997

Source: Authors' compilation based on the NLSY (Bureau of Labor Statistics 2014, 2015).

Note: Unweighted. Logistic regressions with controls for age, gender, race, region, urbanicity, education, leaving school before completing high school. Confidence intervals appear in gray.

tween self-reported offending and arrest outcomes is quite similar for blacks and whites in this earlier cohort, and it is similar regardless of where on the crime continuum one focuses. In short, if black young adults are being arrested more than their white peers net of reported criminality, we are hard pressed to find evidence of it in the 1979 NLSY. At least for this generation then, and at least in their early adulthood, arrest outcomes are mostly egalitarian conditional on reported offending. The absence of an obvious racial disparity in arrest among young adults in the 1970s comports with existing bodies of knowledge and is mostly consistent with the chronological development of the carceral state. In his recent book on the turn toward punitive policies, James Forman reminds us that many of the tactics and policies that would drive up arrests, and particularly those of young urban and poor black men, had not yet occurred by the late 1970s and their “catastrophic impact on black communities wasn’t yet apparent” (2017, 219). Indeed, in the lead-up to the expansion in prisons and drug arrests, incarceration and arrest rates were low and crime and violence were high and getting worse (Pfaff 2017; Forman 2017).

The situation had changed dramatically by the late 1990s. Figure 8 examines arrest probabilities for blacks and non-Hispanic whites in the cohort that was in early adulthood around 1997 along the same crime index as before. The change from 1979 to 1997 is remarkable. The relationship between reported crime and arrest tilts upward for both blacks and whites, but the increase is much more pronounced among blacks. At the low end of the crime index, both blacks and whites were arrested at a probability of about 0.2. But the odds of arrest for blacks increase and increase more quickly at each point along the crime index. So marked is the increase in arrest conditional on reported crime for blacks that, in contrast to 1979, at no point after the lowest crime value do their odds of arrest intersect with whites. Looking back to figure 7 from the 1979 NLSY, we can see that the racial split that emerges for the 1997 cohort is a break from the relatively recent past, establishing itself in a matter of just one generation. For example, in 1979, both blacks and whites had a probability of about 0.25 of arrest if they were a 3 on the crime index (remember, the index is six crimes with six levels of frequency). In 1997, blacks’ probability of arrest jumps to

a whopping 0.8 if they were a 3 on the index (relative to 0.6 for whites). Thus, two things are happening—all people experience a significant jump in arrest odds in 1997 relative to earlier and the rise is particularly salient for blacks, who by 1997 experience much higher chances of arrest at every level of reported offending. Put differently, black Americans' exposure to arrest is both higher than their black counterparts of one generation past and markedly different from that of their white counterparts of the same generation. Those two dynamics deserve much more attention than extant scholarship has given them. Many books and essays have been written on the increase in criminal justice exposure over time and their various political causes as well as their racial dimensions, including by us. Many have suggested that crime was only partly a cause. But this is the clearest analysis to date to document that arrest exposure and its relationship to crime changed in one generation, and a racial disparity emerged that was not present before.²²

Up to this point, we have been concerned with the relationship between reported offending and arrest exposure by generation and racial groups. Let us return to the simple typology that was the springboard for our exploration—the two-by-two of contact and criminality. In the next set of analyses, we divided the NLSY samples into the four quadrants, mirroring our theoretical discussion: arrested, reported a crime; no arrest, reported a crime; arrested, did not report a crime; and no arrest, did not report a crime. Respondents are assigned to the “did not report committing a crime group” if they were a zero on the (non-mean-centered) crime index. Does membership in each quadrant change over time? And if so, is there a distinct pattern for blacks and whites, given that results pointed to an emerging racial disparity among 1997 respondents? Once these quadrants were established, we used a multinomial logit to analyze the relative risk of ending up in one of the quadrants by race and year (with the baseline group being no arrest and no crime). The multinomial ap-

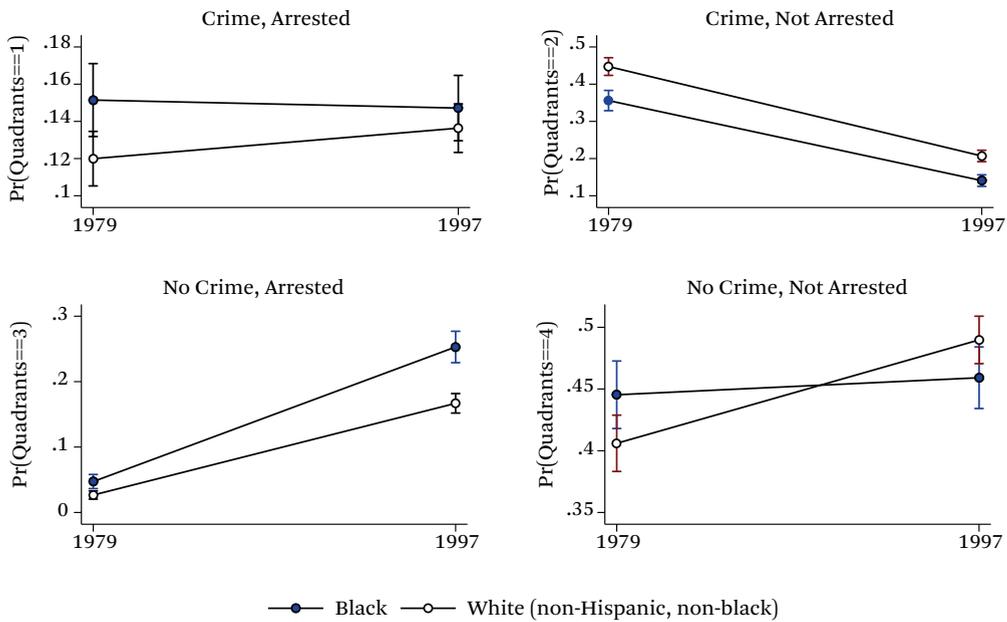
proach allows us to model the paired outcomes of arrest and reported offending resulting in four distinct types, rather than using reported offending to predict arrest as we did in the preceding analysis.

Figure 9 plots the predicted probabilities of landing in a quadrant by cohort and race based on the results of the multinomial logit for men only, holding all of the controls at their mean values. Quadrant 1 shows no significant change; the probability of being in this quadrant remains unchanged for blacks and ever so slightly increases for whites. Quadrant 2 shows steep changes in the likelihood of respondents ending up there and in the direction we would expect; for both black and white men, the probability of being in the category of committing crime (self-reported) and not being arrested shifts significantly downward. White men are somewhat more likely than black men to belong to this category in both cohorts, but the change across cohorts is similar for both black and white men. Thus, we might say that the criminal justice system became more adept at making contact with actual offenders, regardless of race.

Quadrant 3 is where much of the action is: it shows a significant and sizable increase in the likelihood of being in the category of experiencing arrest without indicating crime commission and it is especially pronounced for black men. Specifically, for black men, the predicted probability of being in quadrant 3—of being exposed to arrest without having reported breaking a law—rises from about 0.05 in the 1979 cohort to 0.25 in the 1997 cohort. That change occurs net of age, region, urbanicity, and education. White men also experience a surge in the likelihood of belonging to this category from one generation to the next, albeit not as substantial.

This finding exposes a serious and unappreciated distortion in the modern criminal justice system. The established narrative surrounding criminal justice has focused on increasing contacts across the board with occasional nods to racial or other disparities based on neighborhood and various other factors. The key take-

22. The effects for men alone are even more pronounced, especially at the lower end of the offending index (results available from the authors).

Figure 9. Multinomial Logit Regression Results

Source: Authors' compilation based on the NLSY (Bureau of Labor Statistics 2014, 2015).

away from our analysis, however, is this: the story of the last four decades is really about quadrant 3. A benefit of our approach is that although some assume that race matters everywhere and all the time in the criminal justice system, and others assume that the relationship between offending and arrest remained unchanged even as arrest increased, we can show that race matters but really comes to matter primarily in one quadrant. By looking at arrest outcomes regardless of offending patterns, scholars conflate quadrants 1 and 3 and thereby underestimate changes happening in the latter. If we focus only on arrest outcomes or only on offending outcomes in isolation from one another, we miss that basic but important pattern. Our criminal justice system, it could be said, got both more and less efficient—more efficient in the sense that offenders (of any race) became more likely to be arrested (quadrant 2), but less efficient and less legitimate in that more law-abiding Americans were exposed to arrest over time, particularly black men (quadrant 3).

Our estimates of the changing relationship across cohorts depend on the assumption that our measures of offending and arrest are reli-

able and have external validity; it hinges on the claim that under- or overreporting in self-identification of arrests or offending, if it exists, remains constant across cohorts and racial groups. We have good reason to believe that such a strong assumption of equal validity across group-cohort is warranted. Still, even if we relax this assumption and allow that some underreporting of both offending and arrests for blacks, particularly in the later period amid a political discourse around “superpredator” kids and “lock ‘em up” policies, and even if we adjusted both estimates up, the basic relationship we find between offending and arrest holds. For such dramatic cross-cohort and race results to be explained entirely by measurement error, it would have to be that underreporting of offending was both worse than prior studies have led us to believe and that underreporting of arrest by blacks declined in the later cohort. In the worst-case scenario, a shift over the cohorts to both overreport arrests and underreport criminality occurred and this shift was greatest for blacks. To the best of our knowledge, no evidence exists in the extant prior literature to support these two possibilities. With all of these considerations in mind,

our findings and conclusions are based on one examination of the best dataset we have at present and should be explored in future research with additional datasets.

THE GREAT DECOUPLING

Many explanations have been offered for the expansion in punishment in America before the dawn of the twenty-first century—too much law and too little local democracy, an anxious American public and populist pressures, shifts in modern penology away from the rehabilitative ideal, our dysfunctional political institutions with their weak welfare state and all-powerful prosecutor, neoliberal penalty, and the racialized punitive bidding wars of American politics. Such explanations are important. Yet, they all mischaracterize the development as expanding police oversight and punishment in isolation. What transpired in the last half century was not only an expansion of the state's authority and citizens' increased contact with the state's punitive arm, it was a decoupling that transformed the historical relationship between criminality and exposure to arrest among Americans.

According to the NLSY data, perhaps the best data we have to examine a relationship between arrest and offending at the individual level in the early 1980s, self-reported criminality and contact were strongly related. Moreover, blacks were no more likely to be exposed to arrest than whites at a given level of reported offending. Overall and regardless of reported offending patterns, the share of those over eighteen years of age who reported being arrested was 10 percent for both blacks and whites. Among the generation of young adults in the late 1990s, in stark contrast, the ability of criminal behavior to explain variation in arrest outcomes lessened dramatically. And the relationship became especially distorted among blacks, whose odds of arrest surge upward even at low levels of reported offending. Put differently, the offending-contact relationship for the 1979 cohort approximates a system that arrests those who are unlawful and leaves alone those who are law abiding (and does so for blacks and whites equally), though this earlier system likely missed many individuals who engaged in crime (quadrant 2). Over time, it

appears that in addition to arresting larger shares of individuals committing crime, the system expanded its purview to those who were not actively offending.

We have modeled this relationship at the individual level among a sample of all Americans. If the shifting upward slope that emerges in 1997, particularly among black Americans, is true to reality, how many arrests does this represent over time? How many Americans were exposed to criminal justice but did not behave unlawfully? Our data do not provide definitive estimates but because black men who had not reported engaging in crime had odds of arrest at 0.36, the number is likely very large. If the decoupling persists or widens over time, we will have institutionalized a system that departs from common normative assumptions that the justice system should target actual offenders and leave alone those who abide by the law.

Such an inquiry may unintentionally reinforce what Naomi Murakawa and Katherine Beckett term “the penology of racial innocence”; they warn scholars, rightly in our view, against the practice of studying criminal justice by “exposing moments of bias” and caution against the widespread practice in our fields of study of controlling for criminality, as though crime itself is innocent of the operation of racial power (Murakawa and Beckett 2010). As others have argued, crime itself exposes key ways the state fails to ameliorate deep social risks: “when persons from the ghetto choose crime, however, they do so under conditions of material deprivation and institutional racism. Thus their criminal activity might express something more, or something other, than a character flaw or a disregard for the authority of morality” (Shelby 2007, 136).

Yet we believe that the quadrant exercise exposes an underlying tendency within our criminal justice system that has been routinely erased in scholarship and has for far too long helped further the “ideology” of black criminality (Muhammad 2011). That is, without knowing how criminality and contact relate, we unwittingly convey a view of our criminal justice system as legitimate and efficient and those who are exposed to it as deserving targets. The implications of shifting quadrants matters not only as an academic exercise, but also as a

pressing matter of public policy; reform efforts will likely fail to deliver a more just and fair system if our collective focus remains on lessening the contact in quadrant 1 without attending to the surplus in quadrant 3, those Americans who remain committed to the law but have experienced police sanction. If we do not recognize that decreases in the share of young Americans in quadrant 2 (an outcome to be celebrated, by some more than others perhaps) arguably came at the price of vast and unwarranted expansions in quadrant 3 (which we should all find worrisome), we miss that criminal justice developments can have spillovers born of “improvements” that undermine those very successes. We may also be dismayed when efforts to decarcerate fail to move many people having lower-level contacts into the “no contact” quadrant.

Admittedly, we have only begun to understand the transformation of criminal justice as

it related to actual crime. But we hope to have nudged scholars in relevant fields toward greater recognition of the shifting crime-contact link. Our exploration (and findings) were limited to arrest, a key entry point in the criminal justice system to be sure, but only one of the many points where the shifting connection between offending patterns and exposure may be in evidence. Thus, a natural extension to the study would consider other points in the criminal justice system, such as incarceration, and their underlying relationship to offending. Policymakers may naturally consider how to not just decarcerate but how to make criminality less orthogonal to contact. What interventions can repair a system that not only expands the share of the population having contact with surveillant authorities, a system that is both more severe and intrusive, yes, but perhaps just as concerning, more unhinged from actual law breaking?

APPENDIX

Table A1. Sampling Differences and Adjustments in NLSY Cohorts

| | NLSY 1979 | NLSY 1997 | How Our Analysis Deals with Differences Between 1979 and 1997 NLSY Cohorts |
|----------------------|--|--|---|
| Samples | <p>Cross-sectional sample: Nationally representative sample of individuals born 1957 to 1964 and living in United States as of first survey round</p> <p>Oversamples of black and Hispanic individuals, born 1957 to 1964</p> <p>Economically disadvantaged non-black non-Hispanic oversample, born 1957 to 1964</p> <p>Military sample (born 1957 to 1961 and serving in the military as of September 30, 1978)</p> | <p>Cross-sectional sample: Nationally representative sample of individuals born 1980 to 1984 and living in United States as of first survey round</p> <p>Oversamples of black and Hispanic individuals, born 1980 to 1984 (n = 2473)</p> | <p>Because the oversamples of low-SES whites and military servicepeople in the 1979 NLSY are not included in the 1997 NLSY, we omit them from our analysis (as instructed by the NLSY Tutorial on constructing comparable samples, https://www.nlsinfo.org/site/nlsy97/nlsdocs/nlsy97/tutorials/comparing97-79/comparing97-79_tutorial.html, accessed August 25, 2018)</p> |
| Year analyzed | 1980 (second round) | 2002 (sixth round) | <p>We analyze these rounds because 1980 is the only round for the 1979 NLSY cohort when criminal justice contact and criminal offending variables are asked and 2002 is the round when the age ranges of the 1997 cohort respondents are most similar to the 1980 respondents from the 1979 cohort.</p> |

| | | | |
|---|---|---|---|
| Age range of initial sample | Youth fourteen to twenty-two by December 1978 | Youth twelve to sixteen as of December 1978 | We examine only youth at least eighteen by 1980 and 2002, the years of our analysis. This omits 3,527 respondents from the NLSY 1979. But the age range of eighteen to twenty-three is now consistent across cohorts. |
| Sampling design | Stratified multistage area probability sampling | Stratified multistage area probability sampling | |
| Administration of the survey | PAPI | CAPI | One potential issue in comparing between the two cohorts is that the NLSY switched from using paper and pencil interviews (PAPI) to using computer-assisted personal interviews (CAPI) in 1989 (Interview Methods). This change may have led to differences in reporting delinquent behavior. In 1989, the compatibility of these two methods were tested using CAPI for half of the Ohio interviews and PAPI for the other half (Bradburn et al. 1992, 3). The authors found that of the 139 variables they examined, only four of these "reached conventional levels of significance" (4). Two of these variables were related to alcohol use in CAPI, which leads the authors to consider that CAPI may be a more anonymous form survey and lead to more reporting (8). Therefore, we might potentially expect higher levels of reporting of criminal behavior on the NLSY 97, where only CAPI was used, versus NLSY 79, where PAPI was used for the question regarding criminal behavior. |
| Question wording on key outcome of interest | "Not counting minor traffic offenses, have you ever been booked or charged for breaking a law, either by the police or by someone connected with the courts?" | "Have you ever been arrested by the police or taken into custody for an illegal or delinquent offense (do not include arrests for minor traffic violations)?" | These questions are worded differently and the 1997 version may have been interpreted in a more expansive way to include mere police stops. Therefore, we replicate our analyses using an arrest measure in 1997 that further asks respondents who reported arrest: "did the police charge you with an offense?" |
| | | "Since the date of last interview on <input type="checkbox"/> , have you," | |

Source: Authors' compilation.

Table A2. Descriptive Statistics

| | NLSY 1979 (n = 5,837) | NLSY 1997 (n = 8,683) |
|--|--------------------------|--------------------------|
| Ever arrested (not for minor traffic violations) | 10.2 | 24.9 |
| Ever charged with an offense | 10.2 | 17.4 |
| Arrested in the last year | 4.3 | 6 |
| Mean age of first arrest | 17.4 | 16.3 |
| Criminal offending | | |
| Destroy property | 19 | 3.9 |
| Theft (under \$50) | 19 | 5 |
| Theft (over \$50) | 5 | 2 |
| Attack with intent to injure or kill | 10 | 6 |
| Marijuana distribution | 10 | 4.5 |
| Hard drugs distribution | 2 | 1.7 |
| Used marijuana | 45 | 21 |
| Used hard drugs | 17.5 | 5 |
| Crime Index (mean) | .306 | .127 |
| Men | 49 | 51 |
| Age (mean) | 20 | 20 |
| Less than high school | 30 | 30 |
| Less than high school and not currently enrolled | 15 | 15 |
| High school and not currently enrolled | 27.5 | 23 |
| High school grad and enrolled in college | 15 | 24 |
| Family income (mean) ^a | \$18,156 | \$52,767 |
| Under poverty line ^a | 22.7 | 25.5 |
| Non-Hispanic white | 50 | 51 |
| Hispanic | 20 | 22 |
| Black | 30 | 27 |
| Black men | 15 | 13.5 |
| South | 37 | 39 |
| Northeast | 19 | 17 |
| North central | 25 | 22 |
| West | 19 | 22 |
| Urban | 76 | 76 |
| Central city ^b | 23 | 33 |

Source: Authors' compilation.

Note: Respondents age eighteen or older only.

^a Because of high missingness, values were imputed through a multiple imputation procedure.

^b Of the 1979 cohort, 23 percent are in the central city (though a quarter of the sample is listed as "central city not known").

Table A3. Results of Logistic Regression Predicting Arrest, 1979 and 1997 NLSY Combined

| | Ever Arrested | Arrested in Last Year | Ever Charged with Offense | Arrested in Last Year Controlling for Probation |
|-------------------------------------|----------------------------------|---------------------------|----------------------------------|--|
| 1997 cohort | 2.15*** (0.131) | 1.06*** (.210) | 1.73*** (.133) | 1.00*** (.211) |
| Property offense | 1.39*** (0.127) | 1.35*** (.190) | 1.39*** (.127) | 1.35*** (.191) |
| 1997*property offense (interaction) | -0.348* (0.156) | .418 (.227) | -285 (.159) | .409 (.227) |
| Violent offense | 2.12*** (0.143) | 2.03*** (.205) | 2.11*** (.143) | 2.03*** (.205) |
| 1997*violent offense (interaction) | -0.857*** (0.178) | -.211 (.246) | -0.88*** (.180) | -.234 (.246) |
| Hispanic | -0.242 (0.132) | 0.189 (.179) | -203 (.133) | 0.197 (.179) |
| Black | -0.191 (0.115) | -.026 (.162) | -179 (.115) | -.023 (.162) |
| Other race | -0.261 (0.192) | -.107 (.347) | -.558* (.232) | -100 (.349) |
| 1997*Hispanic interaction | -0.142 (0.150) | -.662** (.225) | -.314* (.156) | -.654** (.225) |
| 1997*black interaction | 0.257 (0.134) | -.089 (.202) | -.030 (.139) | -.069 (.202) |
| Male | 0.992*** (0.054) | 1.005*** (.096) | 1.028*** (.059) | .983*** (.097) |
| Education (years) | -0.215*** (0.021) | -.165*** (.033) | -.187*** (.022) | -.162*** (.034) |
| Dropout | 0.814*** (0.079) | .836*** (.124) | .900*** (.084) | .818*** (.124) |
| Family income | -2.41e-06** (7.37e-07) | -7.54e-07 (1.23e-06) | -2.30e-06** (8.18e-07) | -7.27e-07 (1.23e-06) |
| Age | 0.189*** (0.019) | .011 (.030) | .205 (.019) | .003 (.030) |
| North central | 0.072 (0.080) | -0.001 (0.131) | -.086 (0.086) | -0.011 (0.131) |
| South | -0.147 (0.76) | -0.088 (0.121) | -0.176 (0.082) | -0.069 (0.122) |
| West | 0.213* (0.083) | 0.077 (0.135) | 0.172 (0.089) | 0.094 (0.135) |
| Urban/rural | .078 (.069) | .114 (.108) | .141* (.072) | .101 (.109) |
| On probation in prior years | | | | .641*** (.179) |
| N | 12,936 | 12,939 | 12,936 | 12,939 |

Source: Authors' compilation.

Note: Boldface if significance at the .06 level. Standard errors in parentheses. Sample includes those at least eighteen years old.

* $p < .05$; ** $p < .01$; *** $p < .001$

Table A4. Predicted Probability of Arrest by Offending and Cohort

| | | | |
|-----------------------|----------------------|----------------------------|---------------------------|
| 1997 cohort | 1.64*** (0.066) | Education | -0.205*** (0.020) |
| Mean crime index | 0.503*** (0.033) | Dropped out of high school | 0.818*** (0.080) |
| 1997*mean crime index | 0.030 (0.052) | Male | 1.03*** (0.054) |
| Hispanic | -0.411*** (0.079) | Age | 0.168*** (0.019) |
| White | -0.091 (0.065) | Urban | 0.159* (0.067) |
| North central | 0.093 (0.080) | Family income | -2.21e-06** (7.59e-07) |
| South | -0.111 (0.076) | N | 12,766 |
| West | 0.189* (0.084) | | |

Source: Authors' compilation based on the NLSY (Bureau of Labor Statistics 2014, 2015).

* $p < .05$; ** $p < .01$; *** $p < .001$

Table A5. Predicted Probability of Arrest by Offending

| | 1979 | 1997 |
|---|-----------------------|-------------------------|
| White | -0.0045 (0.136) | -0.155 (0.083) |
| Mean crime index | 0.425*** (0.060) | 0.810*** (0.117) |
| White*mean crime index (interaction) ^a | 0.036 (0.074) | -0.292* (0.128) |
| Male | 1.27*** (0.128) | 0.877*** (0.070) |
| Urban | 0.133 (0.145) | 0.235** (0.081) |
| Education (years) | -0.167** (0.049) | -0.279*** (0.029) |
| Dropout | 0.979*** (0.175) | 0.699*** (0.109) |
| Family income | -6.68e-06 3.69e-06 | -1.41e-06 (8.11e-07) |
| Age | 0.153*** (0.039) | 0.215*** (0.026) |
| North central | 0.023 (0.166) | 0.051 (0.166) |
| South | -0.205 (0.167) | -0.111 (0.098) |
| West | 0.442* (0.187) | 0.122 (0.121) |
| N | 4486 | 5623 |

Source: Authors' compilation based on the NLSY (Bureau of Labor Statistics 2014, 2015).

^a Reference category is black.

* $p < .05$; ** $p < .01$; *** $p < .001$

Table A6. Multinomial Logit Regression Results, Men Only

| Quadrant 1 | | Quadrant 2 (cont.) | |
|-------------------|------------------------|---------------------------|------------------------|
| 1997 Cohort | -0.316** (0.105) | Family income | 1.20e-06 (1.15e-06) |
| White | 0.159 (0.107) | Age | -0.058 (0.030) |
| Urban | 0.156 (0.110) | North central | -0.114 (0.109) |
| Education (years) | -0.265*** (0.041) | South | -0.223* (0.108) |
| Dropout | 1.012*** (0.146) | West | -0.050 (0.133) |
| Family income | 6.63e-07 (1.30e-06) | Quadrant 3 | |
| Age | 0.107** (0.034) | 1997 Cohort | 1.71*** (0.138) |
| North central | -0.249 (0.137) | White | -0.199* (-0.103) |
| South | -0.397** (0.132) | Urban | 0.324** (0.108) |
| West | 0.151 (0.156) | Education (years) | -0.315*** (0.039) |
| Quadrant 2 | | Dropout | 0.640*** (0.145) |
| 1997 Cohort | -1.75*** (0.094) | Family income | 1.60e-08 (1.15e-06) |
| White | 0.340*** (0.089) | Age | 0.245*** (0.034) |
| Urban | 0.286** (0.093) | North central | 0.232 (0.138) |
| Education (years) | -0.032 (0.036) | South | -0.064 (0.134) |
| Dropout | 0.078 (0.141) | West | -0.040 (0.173) |

Source: Authors' compilation based on the NLSY (Bureau of Labor Statistics 2014, 2015).

Note: Baseline group is 4.

* $p < .05$; ** $p < .01$; *** $p < .001$

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