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Examining the Consequences of the “Prevalent Life Events” of Arrest and Incarceration among an Urban African-American Cohort

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Abstract

The life course perspective has traditionally examined prevalent adult life events, such as marriage and employment, and their potential to redirect offending trajectories. However, for African Americans, the life events of arrest and incarceration are becoming equally prevalent in young adulthood. Therefore, it is critical to understand how these “standard” criminal justice practices, which are designed to deter as well as punish, affect deviance among this population. This study evaluates the long-term consequences of criminal justice intervention on substance use and offending into midlife among an African American community cohort using propensity score matching and multivariate regression analyses. The results largely point to a criminogenic effect of criminal justice intervention on midlife deviance with a particularly strong effect of young adult arrest on rates of violent and property arrest counts into midlife. The theoretical and policy implications of the findings are discussed.

Introduction

According to the life-course perspective, lives are shaped by multiple long-term trajectories that are bound by both social and structural context and represent different dimensions of life’s components, including the family, career, health, and offending to name a few (Elder,

1985). Embedded within these long-term trajectories are transitions, which are short-term discrete events. These transitions have the potential to alter one's trajectory on any number of these dimensions of life (Elder, 1985). Drawing from sociological research and in light of the decline in crime in young adulthood evidenced by the age-crime curve, researchers have investigated the impact of an array of life events prevalent in the transition to adulthood, such as marriage, employment, parenthood or entrance into college or the military on offending (see Siennick & Osgood, 2008 for a review).

Yet, in the lives of African Americans, certain life events such as arrest and incarceration are becoming more prevalent than marriage or employment. A recent study using the National Longitudinal Survey of Youth 1997 (NLSY97), a nationally representative sample, revealed that close to 50% of African American males (48.9%) and almost one-fifth of African American females (18.4%) are arrested by age 23 in the US (Brame, Bushway, Paternoster, & Turner, 2014). Moreover, criminal justice presence is concentrated in disadvantaged neighborhoods, where African Americans tend to reside, making the experience of arrest (and subsequently incarceration), a more common event in these neighborhoods (Kirk, 2008). This phenomenon is consistent with Elliott's (1994) analysis of the National Youth Survey, which revealed that the black to white self-reported offending ratio was lower than the corresponding arrest ratio (3:2 versus 4:1), indicating that offending by African Americans is more likely to result in an arrest than offending by whites.

Estimates for incarceration are similarly staggering as evidenced by the Department of Justice findings that "if current trends continue, 1 of every 3 African American males born today can expect to go to prison in his lifetime...and 1 of every 18 African American females" (Mauer, 2011, p. 88S). In fact, it has been argued that criminal justice intervention is approaching the status of an "expected" adult life event, particularly for African American males (Pettit & Western, 2004). As Western and Wildeman state (2009, p. 232),

...from a life course perspective, we can compare imprisonment to other significant life events that are commonly thought to mark the path through young adulthood. Life course researchers have previously studied college graduation, military service, and marriage as key milestones that move young men forward in life to establishing a household and a steady job. Comparing imprisonment to these life events suggests how the pathway through adulthood has changed by the prison boom.

Beyond acknowledging the prevalence of arrest and incarceration among young African Americans, it is critical to understand how these "standard" criminal justice sanctions, which are designed to deter as well as punish, affect deviance (i.e., substance use and offending) across the full life course among this population. Understanding the long-term consequences are of particular importance given the evidence suggesting that African Americans are less likely to desist from substance use and more likely to develop substance use disorders into mid-adulthood than their white counterparts (French, Finkbiner, & Duhamel, 2002). Thus, graphic representation of the drug use curve for African Americans shows its upward climb after adolescence but, unlike whites, continues its upward trajectory to an older age before beginning to decline (see also, Doherty, Green, & Ensminger, 2008). Similarly, Elliott's (1994) analysis of the National Youth Survey also suggests that African Americans persist in

criminal offending longer than whites and therefore, do not follow the typical declining age-crime curve of whites (see also Doherty and Ensminger, 2014). In light of the prevalence of the “life events” of arrest and incarceration for African Americans and the preliminary evidence that African Americans may not follow the same pattern with respect to the age-deviance relationship as whites, the investigation into the impact of these “life events” over the full life course is needed.

Thus, the primary goal of the current study is to evaluate the long-term consequences of criminal justice intervention on substance use and offending into midlife among a community cohort of urban African American males and females from a life course perspective.¹ We emphasize the idea that individuals are affected by the direct impact and accumulation of childhood experiences which can influence transitional life events, defined here as arrest and incarceration, which together can then affect long-term opportunities, behaviors, and decisions (Laub & Sampson, 2003). Theoretically, arrest and incarceration can 1) create a specific deterrent effect, leading to a reduction of offending and substance use; 2) encourage continuation of deviance either directly or through initiating a process of cumulative disadvantage that leads to increased crime and substance use; or 3) have no effect on deviance. Next, we outline these competing hypotheses about the potential impact of arrest and incarceration on future deviance and review the extant empirical evidence regarding each.

The Potential Impact of Arrest and Incarceration on Deviance

Deterrent Effect

One major goal of arrest and incarceration is to deter future crime and drug use among those experiencing the sanction, known as specific deterrence (Cornish & Clark, 1986). This specific deterrent effect is explained by the fact that the pain generated by the experienced sanction serves to alter the rational calculus that then discourages future criminality (i.e., the threat of punishment outweighs the benefits of crime).

To date, there has been ample research on the deterrent effect of criminal justice intervention. Huizinga and Henry (2008) provide a thorough review of the contribution of longitudinal probability studies to our understanding of specific deterrence from arrest. Overall, although some find a deterrent effect from arrest (e.g., Smith & Gartin, 1989) or perceived risk of arrest (Matsueda, Kreager, & Huizinga, 2006), the “evidence suggests that arrest and sanctions either do not have much effect or increase subsequent delinquent behavior” (Huizinga & Henry, 2008, p. 245). Further, studies of incarceration produce “little evidence that prisons reduce recidivism” (Cullen, Jonson, & Nagin, 2011, p. 494; see Nagin, 2013 for a review).

The specific focus in this study is the potential for deterrence among African Americans. While the theory of deterrence has a fairly simple and intuitive appeal, in actual practice it is very complex, resting on a number of assumptions that may not be valid or at least may not

¹To be clear, we do not intend to speak to or investigate *why* these events are prevalent among African Americans but instead focus on examining the long-term consequences of these life events.

be valid for all, particularly African Americans. As Fagan and Meares (2008, p. 229) state, “historically, punishment has been part of social control when formal and informal social controls are reciprocal and complementary, and when punishment is perceived as fair substantively and procedurally.” Thus, if informal social controls and fairness are not present, the ability of the punishment to elicit a deterrent effect may be compromised. The fact that African Americans tend to engage in fewer conventional roles that foster informal social control than whites, such as marriage (Dixon, 2009) and employment (Wilson, 1996), may make them less affected by deterrent mechanisms. Moreover, Tyler (2003, p. 295) finds that “minority groups are less likely to accept decisions [of legal authorities] because they feel unfairly treated,” which may also contribute to a weakened deterrent response.

To date, studies on the specific deterrent impact of criminal justice sanctions have not focused on those at highest risk to receive these sanctions (i.e., disadvantaged African Americans), have not studied the long-term potential of deterrence, and have been limited in their ability to control for the multitude of childhood and adolescent characteristics and other circumstances that can impact arrest, incarceration, and future offending.

Criminogenic Effect

Labeling theory, in contrast to deterrence, posits that criminal justice intervention will not deter but instead will encourage continuity in behavior resulting from the stigma of contact with the criminal justice system and identification as a criminal (Becker, 1963; Lemert, 1951). In addition to this direct labeling effect of criminal justice intervention on future crime and substance use, criminal sanctions may also increase the risk for deviant behaviors indirectly by limiting opportunities for attaining conventional social bonds, such as employment and marital prospects (e.g., Bernburg & Krohn, 2003; Davies & Tanner, 2003; Huebner, 2005; Pager, 2003; Sampson & Laub, 1993; 1997) and facilitating deviant peer associations (Bernburg, Krohn, & Rivera, 2006; Wiley, Slocum, & Esbensen, 2013).

With respect to the potential criminogenic effect among African Americans, specifically, many researchers have found iatrogenic effects of criminal justice interventions for high-risk individuals (Morris & Piquero, 2011), those living in disadvantaged areas (Kubrin & Stewart, 2006), and those with few conventional social ties (Sherman, Smith, Schmidt, & Rogan, 1992). These characteristics also tend to be ones that commonly describe urban African Americans, as they are more likely to have multiple risk factors (e.g., single parent household, poor educational attainment), tend to reside in more disadvantaged communities, and tend to have fewer conventional social ties than whites (e.g., low rates of marriage, high rates of divorce, and high rates of unemployment) (Wilson, 1987).

For instance, Bernburg and Krohn (2003) investigated the impact of arrest and police contact in youth on subsequent crime from the Rochester Youth Development Study and found that these official interventions foster crime in early adulthood through reduced life chances for educational achievement and employment, especially among African Americans and those from impoverished backgrounds. Lopes and colleagues (2012) extend the time frame under investigation for this same sample, which is 65% African American, and study the impact of juvenile police contact on adult deviance (to age 30) as well as education, employment, and financial hardship (i.e., welfare receipt). Consistent with Bernburg and Krohn (2003), they

find evidence of a criminogenic effect of police intervention as a juvenile on both non-criminal and criminal outcomes that extends into young adulthood.

Morris and Piquero (2011) looked at the impact of arrest on subsequent offending longitudinally using the National Youth Survey and controlled for selection bias through trajectory and propensity score modeling. These researchers find evidence in favor of a labeling effect, particularly for high rate offenders. Finally, Kubrin and Stewart (2006) studied incarcerated offenders returning to the community and found that the type of neighborhood (concentrated disadvantage and concentrated affluence) predicted recidivism probability, above and beyond individual factors, with more recidivism among those returning to disadvantaged areas. Thus, in disadvantaged communities where arrest and incarceration are common, deviance may be facilitated rather than discouraged among residents with few conventional ties. This may be especially true for African American males who experience arrest and incarceration at a higher rate than African American females.

However, much of the literature on the criminogenic effects of criminal justice sanctions focus on early criminal justice contact occurring as a juvenile. Moreover, with few exceptions (e.g., Lopes et al., 2012), most studies focus on the short-term effects of juvenile sanctioning on deviance in adolescence or early adulthood leaving adult contact and its potentially pervasive impact into midlife understudied.

Irrelevant Effect

Finally, there may be no effect of arrest or incarceration on future deviance as these prevalent life events become the norm rather than the exception. As Nagin (1998, p. 4) states, “if fear of stigma is a key component of the deterrence mechanism, such fear would seem to depend on the actual meting out of the punishment being a relatively rare event.” Although some hypothesize that minorities are more vulnerable to labeling due to structural disadvantages (e.g., Sampson & Laub, 1997), it may be that minorities will be largely unaffected by criminal justice interventions. The logic follows that in order for a criminal justice intervention to create a “deviant” label and instigate a criminogenic effect, the arrested or incarcerated individual needs to be perceived as “fundamentally different from others” (Bernburg, 2009, p. 188). Thus, arrest and incarceration may be rendered ineffective as more people within a racial minority experience it, reducing the ability to create a meaningful social reaction (resulting in neither a deterrent nor a criminogenic effect). This point is exemplified through Hirschfield’s (2008, p. 585) qualitative interviews; racial minority juveniles in inner-city Chicago revealed that “arrests generally caused little stir outside of informants’ immediate families.”

Current Study

Thus, the core question is, does criminal justice intervention in adulthood impact substance use and/or offending across the life course among a cohort of disadvantaged African Americans? To investigate this question, we employ data from a community cohort of African Americans all living in the Woodlawn community of Chicago in first grade in 1966–67 (Kellam, Branch, Agrawal, & Ensminger, 1975; Ensminger, Anthony, & McCord, 1997;

Doherty & Ensminger, 2013, 2014) and investigate the potential effect of adult criminal justice intervention into midlife. This is a particularly critical question among a population not only disproportionately experiencing these life events, but “expecting” them in the natural progression of one’s life course (Brame et al., 2014; Pettit & Western, 2004), and whose desistance in offending and substance use seems to occur later in the life course (Elliott, 1994; French et al., 2002). A review of the extant empirical evidence suggests the expectation of not only a lack of a deterrent effect of criminal justice intervention on future offending but a criminogenic one.

Moreover, we attempt to overcome the many issues facing research examining the impact of criminal justice sanctions on later deviance (see Huizinga & Henry, 2008). First, we separate the impact of an arrest from the impact of the more severe criminal justice sanction of incarceration, as these two levels of intervention may have different effects (see Smith & Gartin, 1989). Second, the use of a community cohort allows for comparisons of those who have had no criminal justice interaction with those with varying degrees of intervention; thus we can investigate the “absolute” as well as the “relative” effects of criminal justice intervention (Bernburg, 2009, p. 193).² Third, we compare those experiencing each criminal justice sanction with similarly situated counterparts who did not experience a similar sanction by using propensity score matching. Fourth, we include self-reported and official indicators of future deviance, including substance use as well as violent and property offending. Finally, we investigate this question separately for males and females. One might expect gender differences given the lower prevalence of arrest and incarceration among women rendering these “life events” still relatively rare in the lives of women, especially as compared with their African American male counterparts.

Data and Methods

The Woodlawn Study

The Woodlawn Study is a long-term community-based study, which began in the mid-1960s and takes a life-course approach to studying the developmental patterns and correlates of substance use and crime (Kellam et al., 1975). Male and female first graders from Woodlawn, an inner-city community in the South Side of Chicago, were first assessed in 1966 (N=1242) and have been followed longitudinally through mid-adulthood. Only 13 families (1%) declined participation, minimizing selection bias.

Data from multiple sources, were collected at four separate time points (age 6, 16, 32, and 42). During first grade, teachers were asked about each child’s classroom behavior; clinicians observed the children in standardized play situations; and mothers were interviewed about the child and his or her family background (e.g., income, family structure, residential mobility, mother’s education) and child rearing practices (e.g., discipline, affection) among others. Ten years later, when the cohort members were age 16, a subset of the original cohort and their mothers still living in the Chicago area were interviewed (N=705). In 1992 and again in 2002, when the “children” were 32 (referred to as the young

²As Bernburg (2009) explains, using samples that have all undergone criminal justice sanctioning can underestimate the labeling effect by solely comparing the severity of sanctions rather than the presence of a sanction.

adult interview) and 42 (referred to as the midlife interview), a search was performed to locate everyone in the initial cohort. In both periods, participants were asked questions tapping into a variety of social, psychological, and behavioral domains (n=952 in 1992 and n=833 in 2002).³

Throughout the study, Woodlawn has remained a predominantly African American neighborhood similar to many others struggling with crime, gangs, substance use, and as a consequence, police presence. Moreover, by 1992 (age 32), although most of the cohort members had moved out of Woodlawn, they continued to represent urban dwellers enduring significant disadvantage. By age 32, the majority of the cohort self-reported living in a neighborhood with moderate to heavy drug trafficking (62%) and gangs (66%) and 39% were living below the poverty level. By age 42, 26% were living below the poverty level, 40% self-reported living in a neighborhood with moderate to heavy drug trafficking and 42% self-reported living in a neighborhood with gangs.

Data on arrests, charges, and sentencing were obtained in 1993 and again in 2012 from criminal records searches. Throughout the study, reports of mortality have been gathered from family members and neighbors as well as through searches of the National Death Index, with the most recent search conducted in 2009.

Measures

Independent variables: Arrest and incarceration

Arrest: We define the life event of arrest as being arrested at some point between ages 17 and 32, but not incarcerated. The arrest information is drawn from the Chicago Police Department “rap sheets” and the Federal Bureau of Investigation (FBI) data collected in 1993 as well as information gleaned from the updated criminal history files from the Illinois Criminal Justice Authority collected in 2012. A person is considered in the “arrested” group if he or she was arrested for a violent (e.g., homicide, assault, rape, and robbery), property (e.g., burglary, larceny, auto theft, fraud, and criminal damage), drug (e.g., narcotics, both selling and possession, and driving under the influence), public order, non-violent sex, or weapons offense. Traffic offenses are not included. (See Doherty and Ensminger, 2014 for more details).

Incarceration: In order to separate the effect of incarceration from arrest, we compare those who were incarcerated with those who were arrested but not incarcerated between ages 17 and 32. Incarceration data was drawn from two sources. In young adulthood, individuals were asked whether they had been incarcerated and if so, how long the most recent period lasted. For those with no young adult interview (20%), we supplemented this information with data from the criminal record searches (whether an individual was sentenced to jail or prison).

³The reasons for not being interviewed at ages 32 and 42, respectively, include death (N= 46 and 86), too incapacitated to be interviewed (N=3 and 0), refusal (N=39 and 135), and inability to locate (N=202 and 185). Thirty-six cohort members and 18 cohort members were interviewed in jail or prison at age 32 and 42, respectively.

Although some might argue that dichotomizing the measures of arrest and incarceration as opposed to measuring dosage or exact timing of these life events is problematic, we are working under the assumption that experiencing any of these events even once, or experiencing them for a short time with respect to incarceration, can impact later life outcomes (Schnittker & John, 2007).

Dependent variables: Substance use and offending

Substance Use Measures: Long-term substance outcomes are defined as use and problems over the past ten years (ages 33 to 42) and are drawn from the midlife interview. Long-term *illegal drug use* is measured as a binary variable of any use of marijuana, cocaine, crack, LSD, hallucinogens, heroin, or nonmedical use of prescription drugs, in the past 10 years (i.e., since the young adult interview) compared to no use or use, but not in the past 10 years. We also measure the long-term outcome of *drug disorder* (abuse or dependence), which is a binary measure of one or more self-reported symptoms compared to no self-reported symptoms between ages 33 to 42. Symptoms were assessed using the Composite International Diagnostic Interview (CIDI) (Kessler et al., 1994).

Offending Measures: Past 10-year *self-reported violent offending* and *self-reported property offending* measures are drawn from a series of offending questions from the midlife interview. These variables are measured as variety scores ranging from 0 to 8 for violent offending (e.g., in the past year did you beat up someone to get money, force someone to have sex, get in a gang fight) and 0 to 13 for property offending (e.g., in the past year did you steal something worth at least \$10, break into someone's home, use a stolen credit card).⁴ Long-term *official violent offending* (e.g., homicide, assault, rape, and robbery) and *official property offending* (e.g., burglary, larceny, auto theft, fraud, and criminal damage) are measured as the number of official arrest counts incurred between ages 33 and 42 drawn from the arrest data collected from the Chicago Police Department, FBI, and Illinois Criminal Justice Authority.⁵

Covariates: Childhood and adolescent risk factors—In order to better isolate the impact of each life event on the variety of deviant outcomes, we take into account the many variables on which those experiencing and not experiencing the arrest or incarceration could differ. Controlling for these variables within a conventional regression analysis may not adequately take into account the many ways the two populations are likely to differ (Rubin, 2001). To reduce the chance of bias due to the possible violation of multiple regression modeling assumptions (e.g., linearity, absence of multicollinearity) we use propensity score analyses, discussed in detail in the analytic plan, to match individuals on several variables,

⁴A variety score was chosen to establish an index of offending giving equal weight to each offense. The use of variety scores allows for the distinction between the seriousness of offenders, as those who are more serious tend to commit more types of crimes, while safeguarding against the concern with frequency scales that less serious but more frequent offenses dominate the distribution. Moreover, variety scores have repeatedly been found to be reliable measures of offending (see Sweeten, 2012).

⁵Each arrest was coded to allow up to three offense counts (or charges). One concern with using arrest data is the potential bias with overcharging individuals for the same basic crime. To err on the conservative side, we reduced the number of charges in 6% of arrest incidents in which we thought that some charges were part of the same incident. We also reduced the maximum number of charges allowed to the three most serious. The decision to include up to three charges, as opposed to more or fewer, was based on the fact that 99% of the arrest entries had three or fewer charges per arrest with the vast majority having one offense count per arrest (91%).

which allows for a potential causal relationship between arrest or incarceration and the midlife deviant outcomes to be more strongly supported (Rosenbaum & Rubin, 1983).⁶

We include a large number of potential common factors associated with offending and substance use (Hawkins, Catalano, & Miller, 1992; Hawkins et al., 1998) to reduce the possibility that an identified relationship between arrest or incarceration and later deviance is spurious. In our propensity score analyses we use 26 childhood and adolescent measures including socio-demographics (e.g., poverty), family background factors (e.g., family size, family discipline), early social adaptation factors (e.g., first grade academic achievement, aggression), and adolescent risk behaviors (e.g., adolescent offending and early onset of substance use). Appendix A describes each of the 26 variables in detail. Since this is a community cohort of African Americans who were all in the first grade in 1966–67, race and age are controlled by the nature of the cohort. In addition, since the children grew up in Woodlawn, the neighborhood context is the same for all individuals during many of the formative years.

Additional controls

Past year (age 32) substance use and offending: Given that past behavior is the best predictor of future behavior, we also control for past year young adult (age 32) deviance when estimating the long-term impact of arrest and incarceration. *Young adult illegal drug use* is drawn from the young adult interview and is a self-reported binary measure based on questions about the use of marijuana, cocaine, crack, LSD, hallucinogens, or heroin, as well as nonmedical use of prescription drugs, within the past year. Past year *self-reported offending* is drawn from a series of questions in the young adult interview. This measure is a variety score ranging from 0 to 27 different types of violent, property, weapons, and drug crimes. Past year *official offending* is measured by the total number of offense counts at age 32, drawn from the arrest data collected from the Chicago Police Department, FBI, and Illinois Criminal Justice Authority.

Young Adult Marriage and Employment: In addition to a direct effect of criminal justice intervention on future crime and substance use, criminal sanctions may also increase the risk for deviant behaviors by limiting opportunities for attaining conventional social bonds, such as employment and marital prospects. Thus, we include two measures of *social bonds*: 1) whether the person had never been married by age 32, and 2) the number of times a person had been unemployed for three months or more by age 32. It should be noted that while conceptually these factors may function as mediators, we include them in this analysis as potential *confounders* of the relationship between early adult criminal justice intervention and midlife deviance to test the robustness of the findings. We treat these factors as confounders because they are captured in the young adult time period concurrently with arrest and incarceration; thus, no causal or mediational conclusions can be drawn.

⁶The overall purpose of the matching approach is to replicate the distribution of background covariates that would be found in a randomized experiment, thereby matching covariate distributions in an exposed and comparison group. While matching procedures are useful in minimizing the bias present in observed confounders (Stuart & Green, 2008) thereby better isolating the effect of treatment with “all else being equal,” we acknowledge that the propensity score approach does not replace a randomized experiment.

Sample Size—The final sample was reduced from the original 1,242 cohort members for several reasons. First, we restricted the sample to those who were alive through age 42 (and thus, had the opportunity to experience young adult criminal justice intervention and midlife consequences) (n=1,158). Second, we removed those individuals whose data showed discrepancies based on official and self-report data (n=40) (i.e., self-reporting time served with no criminal record match (n=36) or having no criminal record match yet being interviewed in jail/prison (n=4)). Third, we restricted the sample to those who were not “continuously” incarcerated from ages 33 to 42, our window of measurement for long-term consequences, with continuously incarcerated defined as eight or more years (n=8). The rationale for this decision was that we wanted adequate “time on the street” for individuals to have the opportunity to engage in deviant acts in midlife.

Fourth, we restricted the sample to those with complete midlife interview data, which reduced the sample to 793 (64% of the original cohort; 69% of the cohort not dead by age 42). Of the 793 in the final sample, 487 were not arrested between ages 17 and 32 (61%), 196 were arrested but not incarcerated (25%), and 110 were incarcerated (14%). Although we use a variety of imputation methods (i.e., mean and multiple imputation) to handle missing data for the covariates,⁷ we restrict the sample to those with complete outcome data for two key reasons. First, complete case analysis is the recommended treatment of missing data when missing on the outcome, according to White, Royston, and Wood (2011), as imputing on the outcome does little more than introduce “noise” in the final models (i.e., larger standard errors when imputed outcomes are included). Second, imputing outcomes involves specifying a regression model to predict the missing values using the covariates and treatment assignment as the independent variables (Graham, 2009). When imputing outcome measures, a researcher runs the risk of allowing the imputation model to drive the eventual treatment effect estimates.

We acknowledge that this decision to include only those with complete outcome data may lead to bias in our estimates given that the restriction criterion likely removes the more serious offenders who are lost to follow-up (due to death, refusal, or inability to locate). In fact, analyses to assess whether there are systematic reasons for missing cases reveal that individuals who were interviewed at mid-life were more likely to have graduated from high school and less likely to be in poverty in first grade or adolescence compared to those who were not. Although there was no difference between those who were interviewed at mid-life and those who were not with respect to having an arrest record, being incarcerated, or substance use in young adulthood, those missing a mid-life interview had a higher mean number of total arrests and higher self-reported violence in young adulthood (for the females

⁷Since all participants completed an initial childhood interview, missing data on these items was minimal. However, missing data exists for data drawn from the adolescent and young adult interviews (i.e., the propensity score matching variables and past year (age 32) control variables). For the propensity score phase of the analysis, we included a missing data indicator for those missing an adolescent interview. We then used mean imputation for all of the covariates missing less than 10%. One variable, first grade classroom conduct scores, was available for public school children only, resulting in close to 17% missing. Therefore, in addition to mean imputation, we also included a missing data indicator for that variable. In the main analyses, for any variables missing five percent or less, we employ mean imputation. For the remaining eleven variables with more than five percent missing (ranging from 7.1% to 16.7%), we employ multiple imputation by chained equations (Graham, 2009; Stuart, Azur, Frangakis, & Leaf, 2009) allowing us to retain all individuals who completed a midlife interview, had complete arrest and incarceration histories and were not deceased before adulthood. Forty data sets were imputed using STATA 13, as recommended by Graham, Olchowski, and Gilreath (2007).

only). Thus, the estimates may be conservative ones in that the higher risk individuals are more likely to be excluded from the sample.

Analytic Plan

Propensity score matching—Propensity score matching is a common procedure used to estimate similar distributions of observed covariates among a treatment and comparison group, attempting to simulate what would have occurred among confounders had the “treatment” been randomly assigned, as in an experiment. We use the MatchIt Program (Ho, Imai, King, & Stuart, 2006) found in the R statistical package to conduct propensity score matching (PSM) (Hansen, 2004; Rosenbaum, 1991) on the two “treatment” groups of individuals experiencing each life event between the ages of 17 and 32 in order to minimize the differences in observed confounders.

Although several propensity score matching techniques are available, we employ a full matching strategy due to its ability to consider and retain the full sample of participants.⁸ Full matching minimizes the sum of the distances in propensity scores between all pairs of treated and comparison individuals with the overall goal of reducing the distance among scores for the entire sample (see Guo & Fraser, 2010; Rosenbaum, 2002). Thus, full matching creates a number of matched sets, which can include any number of both treatment and comparison subjects. This technique allows all individuals to inform the matched sets by including at least one treated individual with multiple comparison individuals or at least one comparison individual with multiple treated individuals within each matched set (subclass). This approach is ideal because it does not force each treated individual to be matched with a specific number of comparison individuals regardless of the similarity of propensity scores (Stuart & Green, 2008).

Within the matching procedure, we fixed the sets to include an exact match on gender to allow for gender-specific analyses (see Green & Stuart, 2014). Exact matching on gender ensures that men are matched with men and women are matched with women. After propensity scores were estimated and matched sets were identified, each individual was assigned a weight based on the ratio of treatment to control individuals within each set to be used in the regression analysis. The analysis for the arrested versus non-arrested group created 168 matched sets based on propensity scores ranging from 0.05 (very low) to 0.93 (high). Treatment to comparison ratios for the arrested group range from 1:1 to 1:21. The matching procedure for incarceration resulted in 81 matched sets with propensity scores ranging from 0.04 to 0.92. Treatment to comparison ratios for the incarcerated group range from 1:1 to 1:35.

After estimating propensity scores and matching individuals within matched sets based on these scores, we performed a series of diagnostic checks as described by Stuart and Green (2008) to assess the adequacy of the matches. First, we examined the balance of each covariate through an examination of its standardized difference before and after matching for

⁸Other common techniques include simple $k:1$ matching, which selects k comparison individuals for each treated individual and variations of these methods (e.g., caliper settings which employ a restricted range so that matches outside of a certain ‘distance’ are excluded). However, these and related methods are often criticized for the restrictions they set on the data and their tendency to exclude unmatched treated and comparison individuals, reducing the generalizability of estimates (Rosenbaum & Rubin, 1983).

each variable, its square, and all two-way interactions.⁹ Rubin (2001) suggests that standardized differences should be less than 0.25 after matching. Using a conservative approach, we considered standardized differences of less than 0.20 to be good matches (Ho, Imai, King, & Stuart, 2007). Second, we assessed graphical displays, including displays of propensity score distributions, quantile-quantile plots for continuous covariates, and plots of standardized differences of means to assess the validity of the balance.

Main regression analyses—Having minimized the issue of selection and established temporal ordering between our main independent and dependent variable, we are better situated to estimate the independent associations between the life events under study and their potential impact on offending and substance use. To do this, we use logistic, Poisson, and negative binomial regression in STATA 13 to estimate the association of arrest or incarceration (experienced between ages 17 and 32) on offending and substance use between ages 33 to 42. All reported analyses adjust for propensity weights. The model used to predict each association varies depending on the nature of each outcome. For example, incidence rate ratios (IRR) from Poisson and negative binomial regressions are reported for all of the offending outcomes – official offense counts and self-reported variety scores. Since the substance use outcomes are dichotomous, odds ratios (OR) estimated with logistic regressions are reported. All analyses are conducted separately for men and women.

The analysis for each outcome of interest begins with a model estimating the propensity score weighted effect of the life event of interest (young adult arrest or incarceration) on each midlife outcome. This initial model also includes any matching variables with standardized differences above 0.10 to further adjust for any remaining differences in the two groups (Ho et al., 2007). A second model then builds on the initial model by adding the past year (age 32) comparable behavior (substance use or offending) to better isolate the long-term impact of each life event. Finally, a third and final model controls for the possibility of spuriousness through cumulative disadvantage (Sampson & Laub, 1997) with the addition of whether someone had never been married by age 32 and the number of times a person had been unemployed by age 32. Thus, this final model provides the most rigorous test of the impact of a criminal justice sanction on future deviance while accounting for these potential contemporaneous social confounders.

Results

Dimensions of Young Adult Criminal Justice Intervention

Among the arrested but not incarcerated subsample (n=196), approximately one-third was arrested only once (38.3%, n=75), with 29.8% (n=34) of the males and 50.0% (n=41) of the females being arrested once in young adulthood. The average number of arrests per arrested individual is 3.33 (s.d. = 3.69) with a range of 1 to 32. Over two-thirds of the offenders had 3 or fewer arrest counts (69.9 %, n=137) and 5.0% (n=10) had 10 or more arrest counts. As shown in Table 1, the 114 arrested males averaged 4.03 offense counts and the 82 arrested females averaged 2.35 offense counts between ages 17 and 32. The average age of the first

⁹The final models for arrest do not include any interactions terms since all standard differences were below 0.20 without their inclusion. Three interactions are included in the final model for the incarceration group.

adult arrest was 22.5 years for males and 22.8 years for females with half of both genders arrested for the first time within the first 5 years of adulthood and the other half spread throughout the next 10 years.

Among those incarcerated (n=93 males and 17 females), males averaged over 8.11 offense counts in young adulthood; females averaged 5.25 offense counts with a first arrest on average at 19.9 and 22.9, respectively. The mean number of days incarcerated between ages 17 and 32 is 667.92 days for males with a range of 1 to 5475 days (or 15 of the possible 16 years between ages 17 and 32). However, 75% of those incarcerated males (n=70) spent fewer than 3 years incarcerated. For females, the median time incarcerated was only 14 days compared with 3 months for males (see Table 1).

Prevalence of Midlife Deviance

Table 2 depicts the prevalence of midlife deviance by level of young adult criminal justice sanction for males and females, separately. As expected, for both genders, those who were not arrested have the lowest percentage of substance use problems and the lowest self-reported and official offending in midlife compared with their arrested or incarcerated counterparts. For instance, incarcerated males had a rate of official violence of 1.16 in midlife, on average, compared to an average of 0.66 official counts of violence among those arrested but not incarcerated and an average of 0.06 among those not arrested in young adulthood; similarly, incarcerated females had a rate of official violence of 0.71 compared with 0.37 official counts of violence among those arrested but not incarcerated, on average, and 0.01 for those not arrested in young adulthood. Similarly, those incarcerated have substantially higher levels of offending (both self-reported and official), with the exception of female self-reported violence. However, these descriptive analyses do not take into account the variety of ways that those who are not arrested, arrested, and incarcerated in young adulthood are likely to differ. In an attempt to control for these differences, we use propensity score matching.

Propensity Score Matching

Table 3 displays the background characteristics of the sample as well as standardized differences before and after matching for each life event and its corresponding comparison group. Before matching, those who were arrested were significantly different on 13 of the 26 covariates from those not arrested; those who experienced incarceration were significantly different on 7 out of the 26 covariates from those who were not incarcerated before matching. After matching, all differences were no longer significant and all standardized differences were less than 0.20, indicating good matches (Ho et al., 2007).

Long-term Impact of Arrest on Substance Use and Crime

Table 4 (Model 3) shows the relationship between each life event and the outcome of interest, separately by gender, controlling for past year deviant behavior at age 32, marriage, and employment. For the males, arrest significantly increases the odds of drug use and experiencing at least one drug disorder symptom by two to three times (OR= 2.87 and 3.11, respectively). While arrest is largely not associated with self-reported offending (violent or property) for males,¹⁰ there is a very strong association between young adult arrest and

official arrest counts in midlife. For instance, males who were arrested, but not incarcerated, between ages 17 and 32 have a rate of violent arrests in midlife that is 17 times their propensity-matched non-arrested counterparts (IRR=17.50). Similarly, arrested males have an 8.42 times higher rate of property arrest counts compared with those without an arrest in young adulthood, independent of young adult offending, marriage, and employment.

The results for the females reveal a significant association between young adult arrest and all types of deviance in midlife, controlling for past year deviant behavior at age 32 and young adult marriage and employment (see Table 4, Model 3). Similar to the males, arrested females are close to two to three times more likely to report drug use and a drug disorder symptom in midlife (OR= 1.91 and 3.40, respectively), have a rate of violent arrests in midlife that is 17 times larger (IRR=17.04), and have a rate of property arrests that is close to 14 times larger (IRR=14.07) than their non-arrested counterparts. Unlike males, however, arrest in young adulthood also impacts self-reported offending with females arrested in young adulthood having 5.92 times the rate of self-reported violence and 2.75 times the rate of self-reported property offending than those not arrested. Moreover, there is a gender interaction effect with respect to arrest on self-reported violence with arrested women having a significantly higher rate of self-reported violence than arrested males (data not shown).

Long-term Impact of Incarceration on Substance Use and Crime

Table 4 also shows that, among those arrested, experiencing incarceration in young adulthood does not increase the odds of drug use or reporting drug disorder symptoms for males; yet, incarceration significantly increases violence and property offending (both self-reported and official). After controlling for past year offending at age 32 and young adult marriage and employment, males experiencing incarceration in young adulthood have a rate of self-reported and official offending that is two to three times higher than those not incarcerated (IRR of 3.16 and 2.35 for self-reported violence and property, respectively; IRR of 1.94 for official violence, all $p < 0.05$) (see Model 3). The effect for official property offending was not significant for the males.

For females, incarceration largely does not impact midlife deviance with the results showing no significant effects except for official property offending (IRR=2.41, $p < 0.05$). Interaction analyses indicate that although the impact of incarceration on self-reported violence is not significantly different from zero for females (IRR = 0.53), this association is significantly different than the males (data not shown) and in the direction of a deterrent effect rather than a criminogenic one.

Discussion

In line with prior research, the Woodlawn men experienced rates of arrest and incarceration approaching the percentages of those married among this sample (44.4% of the males were

¹⁰While the impact of arrest on self-reported property offending for the males becomes statistically significant once employment and past year age 32 offending are entered into the model, this significance is likely due to an increased precision in the estimate due to employment accounting for some of the residuals. Replication of this finding in future research is needed to better understand this effect.

arrested between ages 17 and 32 and 51.4% were married in that time frame). Thus, in this population of men that appears to experience criminal justice intervention at similar rates to other “expected” adult life events (see, e.g. Petit & Western, 2004), it is imperative to empirically study the impact of these events on the long-term consequences over the life course. For women, both arrest and incarceration were less common with 22% of the females arrested. Thus, for the Woodlawn women, criminal justice sanctions might still be considered relatively “rare” events (or at least rarer than experiencing other life events).

The primary goal of this study is to ascertain whether criminal justice intervention experienced in young adulthood is associated with future crime and substance use into midlife among African Americans. Although, *theoretically*, these life events could deter an individual from future crime and deviance, could have criminogenic effects, or have no effect on deviance, extant empirical evidence suggests that arrest and incarceration would be criminogenic among this population. In line with this body of literature, we find no significant deterrent effects to support a deterrence argument; we find limited support for the notion that the commonality of sanctioning among this population might breed indifference to its impact; and, we find consistent evidence for a criminogenic effect of criminal justice intervention on this cohort for both men and women, indicating that the high prevalence of arrest and incarceration for African Americans is counterproductive.

The most striking finding in this study is the clear association between arrest in young adulthood and all types of deviance for both men and women, especially on midlife official offense counts (with rate differentials that are 8- to 17-fold) indicating a strong secondary sanctioning effect (Lieberman, Kirk, & Kim, 2014). Moreover, although we were not able to control for all known childhood and adolescent risk factors or all of the possible factors proposed to confound the relationship between sanctions and future deviance (see e.g., Bernburg et al., 2006; Wiley, Slocum, & Esbensen, 2013), these analyses used propensity score matching with 26 early life covariates in addition to controls for short-term behavior (at 32) and the potential blockage of concurrent social role opportunities (i.e., marriage, employment).

While overall the evidence supports a criminogenic effect, there are some nuances worth mentioning. For one, females seem to experience a criminogenic effect from young adult arrest on all forms of deviance (substance use, self-reported offending, and official offending) while males experience this effect for official offending and substance use outcomes only (not self-reported offending). This finding suggests some support for the “commonality breeds indifference” argument of arrest on self-reported offending for males but not females. It may be that since arrest is not as prevalent of a life event for females, arrested women are still seen as “fundamentally different” (Bernburg, 2009, p. 188) from their non-arrested female counterparts leading to both secondary deviance and secondary sanctioning.¹¹

¹¹This effect could also be a result of the differential treatment of law enforcement of female and male adult offenders, with females treated in a way that creates secondary deviance for females but not for males.

When the “relative” difference in sanctioning is examined (i.e., arrest but no incarceration versus incarceration) there is a consistent criminogenic effect from incarceration on both self-reported and official offending for men, yet the effect for women is less consistent. With only one effect reaching significance (incarceration on official property crime), it could be that “the labeling process has run its course by that time” for women (Paternoster & Iovanni, 1989, p. 385; see also Bernburg, 2009). However, it should be noted that the lack of significant findings may also be a result of a lack of power as only 17 women were incarcerated in young adulthood. Interestingly, although not significant, incarceration shows a decrease in substance use for both genders suggesting that incarceration could potentially be a deterrent for substance use, perhaps through detoxification or possibly treatment.

Unpacking the Criminogenic Findings

We now turn to a discussion of the possible explanations behind the findings of a criminogenic effect, especially regarding the young adult arrest findings. Labeling theory posits a social process whereby a criminal justice label assigned to the offender (e.g., as a result of arrest) creates a stigmatic effect resulting in secondary deviance and thus, increased offending (Lemert, 1951). While this study did not investigate this process directly, labeling theory is an important potential explanation for the observed findings, especially given the consistency of these findings with direct tests of labeling. Moreover, this study adds to a growing research base (e.g., Lopes et al., 2012) that suggests the labeling process may not be limited to adolescence and may have long-term effects. However, labeling may not be the only process contributing to the observed criminogenic effect. Several additional explanations beyond a labeling explanation, which are outlined next, should be explored in future research among African Americans as well.

First, this criminogenic effect may also be rooted in the high-risk nature of the Woodlawn cohort. During the period from 1966 to 1972, Woodlawn had the highest rate of male juvenile delinquents of the 76 Chicago community areas (33.5 per 100 males between the ages of 12 and 16) and was the fifth poorest community (Council for Community Services, 1975), with more than half living below poverty (53%). Moreover, in young and mid-adulthood, a large percentage of the Woodlawn cohort lived in neighborhoods with moderate to heavy drug trafficking and gang presence. Morris and Piquero (2011) found that arrest amplifies subsequent delinquency for adolescents at higher risk, taking sample selection bias into account. Moreover, if neighborhood context plays a key role in the probability of arrest and the neighborhood context of the initial arrest does not change, then additional offending and arrests are likely to occur for individuals remaining in this high-risk context (Huizinga & Henry, 2008).

A second complimentary explanation might be one of citizens questioning police legitimacy. The prevalence of arrest and incarceration among African Americans in disadvantaged neighborhoods can also breed cynical attitudes toward the police. This, in turn, can erode the perception of legitimacy of the criminal justice system, which is essential for deterrence. According to the theory of procedural justice, offenders who are treated in ways that they define as fair, regardless of the punishment they receive, will be less likely to reoffend (Tyler, 1990). In contrast to deterrence theory, which takes an instrumental view of crime,

procedural justice is premised on the notion that conformity depends more on being treated fairly during interactions with criminal justice professionals rather than on the sanction imposed. In fact, procedural injustice and a loss of legitimacy can lead to defiance, especially among those living in communities of alienation and social isolation (Sherman, 1993). However, if cynicism regarding police legitimacy or defiance against the criminal justice system were driving the results, we would expect offending behavior to increase, regardless of how it is measured (i.e., self-reported and official offending). In this study the difference in significance (for males) and magnitude (for females) of the impact of arrest on self-reported compared to official offending indicates that arrest has less of a criminogenic impact on actual offending behavior but instead increases future sanctioning among men.¹²

Another possible explanation contributing to the difference in the impact of arrest on self-reported and official offending measures is that the arrested group may differ in the nature and context of their offenses that make them more visible to police intervention (e.g., they are in public, against strangers) and hence the stronger association with official crime. Or, it may be that once someone has an arrest record, they become known to the police and (either consciously or unconsciously) may be targeted as a potential suspect. Indeed, in line with this latter suggestion, Liberman, Kirk, and Kim (2014) recently found evidence for “secondary sanctioning” as opposed to “secondary deviance” in the wake of a first juvenile arrest among the Project on Human Development in Chicago Neighborhoods (PHDCN) sample. Overall, future research should investigate these potential mechanisms between official intervention and future deviance beyond labeling to better understand the dynamics at play behind the deviance-criminal justice intervention nexus among African Americans.

Theoretical and Policy Implications

These findings have implications in understanding the longitudinal patterns of substance use and offending for African Americans. If indeed African Americans tend to “age out” of substance use (French et al., 2002) and criminal offending (Elliott, 1994) later than whites, could the high rates of arrest, in particular, be playing a role in delaying that desistance? Future research should acknowledge the impact of arrest on substance use and offending over the full life course of African Americans and model its role in shaping the age-crime curve for this population. In fact, although this study is not able to identify or test the sources/mechanisms for the iatrogenic effects of arrest and incarceration, the conclusions from this study add evidence to Hirschfield’s claim that the “normalization” of mass arrests and incarceration among African American disadvantaged communities “call[s] for theories which...implicate the justice system in helping perpetuate delinquency, crime, and imprisonment” (Hirschfield, 2008, p. 597; see also, Liberman, Kirk, & Kim 2014). Future theoretical work is needed to address this potential direct role of the criminal justice system on perpetuating criminal careers (i.e., “secondary sanctioning”) (Liberman, Kirk, & Kim, 2014), especially among African Americans who disproportionately experience criminal justice intervention. Specifically, the goal of specific deterrence embedded in arrest and incarceration needs to be questioned and “the reconfiguration of criminal justice along the

¹²While the differences between self-reported and official offending may be due to accuracy in self-reporting, high correlations between the two measures limit this explanation ($r=0.308$ for self-reported violent offending and official violent arrests, ages 33–42; $r=0.349$ for property ($p<0.01$)).

lines of reintegrative justice, substantive rationality, and proportional and procedural fairness” (Fagan & Meares, 2008, p. 229) should be considered. Theoretical research should then be coupled with policy and evaluation research to better understand how the consequences of criminal justice sanctions can be ameliorated through revamped criminal justice policy and/or early prevention and intervention policies.

In sum, this study contributes to the literature by expanding the investigation of the impact of “prevalent life events” in adulthood on future deviance to include arrest and incarceration. We employ a life course perspective and use a prospective longitudinal study with measures of substance use, self-reported and official crime, as well as incarceration information, social factors, and a multitude of early childhood and adolescent risk factors to control for criminal propensity. Moreover, the sample constitutes a community cohort, as opposed to an offender sample (resulting in potential selection bias) or a general population sample (resulting in low rates of deviance). Although, extrapolating these conclusions from a community cohort to other populations and applying them to theory development should be done prudently, the Woodlawn cohort may exhibit several similarities to other urban African Americans across the United States, especially those who experienced the prison boom in young adulthood. Finally, although the sample for these analyses represents a select one in that those excluded are those who died by age 42, were continuously incarcerated, refused an interview, or were unable to be located, the results may have revealed even stronger associations had those higher risk individuals been included in the sample. Therefore, the strong effect of criminal justice intervention on future deviance cannot be ignored.

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Appendix A. Childhood and Adolescent Matching Covariates

	Measure	Description
Socio- Demographics	Gender	a binary variable of male or female
	Poverty	a binary variable of whether the family's income in 1967 fell below the poverty line for the household size ^a
	Mother's Education	the number of years of school the mother had completed at the time of the interview in 1966–1967 (range 0–18)
Family	Family Size	the number of children < 19 years old in the household during childhood (range 1–15)
	Female-Headed Household	a dichotomous variable of whether a child was living in a "mother alone" household or not in first grade
	Residential Mobility	the number of times a child had moved in the six years between his or her birth and the time of the interview in 1966–1967 (range 0–9)
	Family Discipline	a composite score of mother report at the 1966–67 interview (range 1–9): how often was the child spanked (range: never to almost every day), and how often the child got punished (range: hardly ever to always) ($r = .27, p < .001$)
	Family Affection	a summed score (range 1–7) ($r = .19, p < .001$) of two questions: how often did the mother play with/read to the child; how often did the child get taken out (range: never to every week)
	Mother's depression	based on mother reports of frequency of feeling sad or blue on a scale of 0 to 3, ranging from hardly ever to very often
	Mother's anxiety	based on mother reports of frequency of feeling nervous, tense, or edgy on a scale of 0 to 3, ranging from hardly ever to very often

	Measure	Description
Early Social Adaptation	Aggression	first grade teacher observation rating of aggressive behavior, ranging from 0 to 3, adapting to severely maladapting
	Shyness	first grade teacher observation rating of shy behavior, ranging from 0 to 3, adapting to severely maladapting
	Inattention	first grade teacher observation rating of ability to focus, ranging from 0 to 3, adapting to severely maladapting
	Underachievement	first grade teacher observation rating of achievement, ranging from 0 to 3, adapting to severely maladapting
	Immaturity	first grade teacher observation rating of maturity, ranging from 0 to 3, adapting to severely maladapting
	Reading Grades	first grade teacher rating of reading skills (range: unsatisfactory to excellent)
	Math Grades	first grade teacher rating of math skills (range: unsatisfactory to excellent)
	Classroom Conduct Scores	first grade teacher rating of each child's general classroom conduct (range: unsatisfactory to excellent)
Adolescent Risk Behaviors	Adolescent Status Offending	a mean scale of 6 status offenses (e.g., run away from home) drawn from the adolescent and young adulthood assessments (range: never, once, more than once)
	Adolescent Violent Offending	a mean scale of 14 violent offenses (e.g., get into a serious fight) drawn from the adolescent and young adulthood assessments (range: never, once, more than once)
	Adolescent Non-violent Offending	a mean scale of 9 non-violent offenses (e.g., damage school property) drawn from the adolescent and young adulthood assessments (range: never, once, more than once)
	Early Onset of Smoking	a binary variable of smoking a full cigarette before age 15
	Early Onset of Alcohol	a binary variable of drinking more than a sip of beer, wine, or hard liquor before age 15
	Early Onset of Marijuana	a binary variable of initiating marijuana use before age 15
	High School Dropout	a binary variable indicating whether someone dropped out of school prior to graduation versus being a high school graduate or receiving a GED
Teen Parent	a binary variable indicating whether someone became a parent before age 20 as opposed to never a parent or a parent after age 20	

^aFor those who were missing data on this variable, poverty was assessed using the measure of whether the family was supported by welfare. Those who received welfare were considered to be below the poverty line since the eligibility requirement for receiving welfare in Illinois at this time was living below the poverty level. Moreover, welfare benefits were not sufficient to raise a family income to above the poverty level (US Department of Health and Human Services, 2007). Poverty and welfare are highly associated in this sample ($\chi^2 = 392.65, p < .001$).

Table 1

Summary Statistics of Young Adult Criminal Dimensions (Ages 17 to 32)

<i>Among those arrested but not incarcerated (n=196)</i>						
Total Offense Counts (17 to 32)						
	Mean (sd)	Range	25% Quartile	50% Quartile	75% Quartile	
Males (n=114)	4.03 (4.29)	1 to 32	1.00	3.00	5.00	
Females (n=82)	2.35 (2.32)	1 to 15	1.00	1.50	2.25	
Age of First Adult Arrest						
	Mean (sd)	Range	25% Quartile	50% Quartile	75% Quartile	
Males (n=114)	22.45 (4.21)	17 to 31	19.00	22.00	25.25	
Females (n=82)	22.84 (4.34)	17 to 31	19.00	22.00	26.00	
<i>Among those incarcerated (n=110)</i>						
Total Offense Counts (17 to 32)						
	Mean (sd)	Range	25% Quartile	50% Quartile	75% Quartile	
Males (n=93)	8.11 (7.89)	1 to 51	3.00	6.00	11.00	
Females (n=17)	5.25 (5.83)	1 to 20	2.00	2.50	6.25	
Age of First Adult Arrest						
	Mean (sd)	Range	25% Quartile	50% Quartile	75% Quartile	
Males (n=93)	19.92 (3.29)	17 to 31	17.00	19.00	21.75	
Females (n=17)	22.88 (5.24)	17 to 31	18.00	21.00	28.50	
Days Incarcerated (17 to 32)						
	Mean (sd)	Range	25% Quartile	50% Quartile	75% Quartile	
Males (n=93)	667.92 (1137.38)	1 to 5475	9.50	90.00	1016.25	
Females (n=17)	210.36 (274.22)	1 to 730	1.00	14.00	540.00	

Midlife Deviance (age 33 to 42) among the Woodlawn Cohort by Level of Young Adult Criminal Justice Intervention (age 17 to 32)

Table 2

Midlife Measures (range)	Level of Young Adult Criminal Justice Intervention			Significance Tests ^a	
	Not Arrested (n=143)	Arrested, But Not Incarcerated (n=114)	Incarcerated (n=93)	Test Statistic	P-value
Males					
Percent who used drugs (0-1)	28.7%	52.6%	47.3%	$\chi^2 = 16.81$	<0.001
Percent with 1+ drug use disorder symptom (0-1)	7.7%	21.9%	24.7%	$\chi^2 = 14.78$	<0.01
Mean variety score of self-reported violence (0-8)	0.18	0.32	0.83	F = 13.73	<0.001
Mean variety score of self-reported property (0-13)	0.24	0.38	0.80	F = 5.53	<0.01
Mean number of official violent offenses (0-20)	0.06	0.66	1.16	F = 12.78	<0.001
Mean number of official property offenses (0-21)	0.13	0.87	1.66	F = 10.64	<0.001
Self-reported incarceration (0-1)	5.6%	19.3%	38.7%	$\chi^2 = 40.41$	<0.001
Females					
Percent who used drugs (0-1)	23.6%	48.8%	41.2%	$\chi^2 = 21.82$	<0.001
Percent with 1+ drug use disorder symptom (0-1)	5.5%	23.2%	17.7%	$\chi^2 = 26.04$	<0.001
Mean variety score of self-reported violence (0-8)	0.08	0.54	0.53	F = 25.67	<0.001
Mean variety score of self-reported property (0-13)	0.08	0.51	1.12	F = 30.08	<0.001
Mean number of official violent offenses (0-20)	0.01	0.37	0.71	F = 29.3	<0.001
Mean number of official property offenses (0-21)	0.03	0.55	1.12	F = 34.52	<0.001
Self-reported incarceration (0-1)	1.7%	12.2%	29.4%	$\chi^2 = 39.85$	<0.001

^a ANOVA tests are used for young adult life event group comparisons and continuous level midlife measures; chi-square tests are used for young adult life event group comparisons and categorical level midlife measures.

Table 3

Comparison of groups before propensity score matching on covariates: means or percents, standardized differences (before and after matching), and statistical significance

Covariates	Arrested, not incarcerated (n=196)	Not Arrested (n=487)	Std. Diff. Before Matching ^d		Std. Diff. After Matching ^d		Incarcerated (n=110)	Not Incarcerated (n=196)	Std. Diff. Before Matching ^b		Std. Diff. After Matching ^b	
			Mean	SE	Mean	SE			Mean	SE	Mean	SE
Poverty Status	58.77%	48.86%	*0.20		-0.05		59.57%	58.77%	0.02		0.02	
Mother's Education	10.46	10.82	-0.16		0.05		10.24	10.46	-0.10		-0.10	
Female-headed household	39.29%	32.65%	0.14		-0.05		48.18%	39.29%	0.18		0.18	
Family Size	4.52	4.09	*0.20		-0.03		4.65	4.52	0.05		0.05	
Residential Mobility	2.25	2.14	0.06		-0.03		2.61	2.25	0.22		0.22	
Family Discipline	5.69	5.27	**0.23		0.00		5.92	5.69	0.14		0.14	
Family Affection	4.78	4.76	0.02		0.06		4.46	4.78	*-0.23		-0.23	
Mother's Depression	1.44	1.38	0.07		0.04		1.49	1.45	0.04		0.04	
Mother's Anxiety	0.95	0.77	*0.19		-0.05		0.86	0.96	-0.12		-0.12	
TOCA - Shyness	0.43	0.43	0.00		0.03		0.60	0.43	0.17		0.17	
TOCA - Immaturity	0.60	0.56	0.04		-0.01		0.81	0.60	0.20		0.20	
TOCA - Inattention	0.63	0.40	**0.22		0.05		0.90	0.63	*0.24		0.24	
TOCA - Underachievement	0.68	0.59	0.10		0.06		0.82	0.68	0.14		0.14	
TOCA - Aggression	0.64	0.33	**0.30		0.07		0.87	0.64	0.22		0.22	
Reading Grades	2.52	2.48	0.06		-0.07		2.75	2.52	**0.34		0.34	
Math Grades	2.53	2.49	0.06		-0.05		2.74	2.53	*0.28		0.28	
Classroom Conduct Scores	2.32	2.17	*0.21		-0.05		2.60	2.32	**0.39		0.39	
Adolescent Status Offending	0.76	0.61	**0.28		-0.07		0.84	0.76	0.16		0.16	
Adolescent Violent Offending	0.31	0.20	**0.30		-0.06		0.46	0.31	**0.37		0.37	
Adolescent Non-violent Offending	0.34	0.27	*0.19		-0.02		0.51	0.34	**0.35		0.35	
Early Onset of Smoking	27.55%	21.30%	0.14		0.08		30.00%	27.55%	0.05		0.05	
Early Onset of Alcohol	29.21%	19.30%	**0.22		0.04		38.41%	29.21%	0.19		0.19	
Early Onset of Marijuana	18.50%	8.21%	**0.27		0.08		23.64%	18.50%	0.12		0.12	

Covariates	Arrested, not incarcerated (n=196)		Not Arrested (n=487)		Std. Diff. Before Matching ^d		Std. Diff. After Matching ^d		Incarcerated (n=110)		Not Incarcerated (n=196)		Std. Diff. Before Matching ^b		Std. Diff. After Matching ^b	
	%		%						%		%					
High School Dropout	29.55%		18.47%	**0.25	-0.04	34.22%	30.51%	0.08	-0.09							
Teen Parent	36.39%		29.89%	0.13	-0.03	34.55%	36.41%	-0.04	0.11							

^a Compares individuals who were arrested, but not incarcerated, with those who were not arrested.

^b Compares individuals who were incarcerated with those who were arrested but not incarcerated. All cases were exact matched on gender.

* $p < .05$,

** $p < .01$: variables significantly different based on t -tests.

Table 4
Long-Term Impact of Arrest and Incarceration in Young Adulthood (17 to 32) on Midlife Substance Use and Crime (33 to 42)

Treatment	Drug Use ^b Odds Ratio			Drug Disorder ^b Odds Ratio			Self-Reported Violence ^c Incidence Rate Ratio			Self-Reported Property ^c Incidence Rate Ratio			Official Violence Counts ^c Incidence Rate Ratio			Official Property Counts ^c Incidence Rate Ratio		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Males																		
Arrest	3.42***	2.83***	2.87***	3.34***	2.42*	3.11**	1.15	1.59	2.15	1.70	1.92	2.44*	18.01***	16.05***	17.50***	9.38***	8.20***	8.42***
Age 32 Outcome	4.27**	4.04**	4.04**	6.01***	4.76***	4.76***	1.37	1.37	1.25	1.06	1.15	1.06	1.26	1.26	1.23	1.24	1.24	1.36
Never Married		0.65	0.65	1.64		1.64	0.71	0.71	0.71	1.00	1.00	1.00	0.57	0.57	0.57	0.36	0.36	0.36
Times Unemployed		1.12	1.12	1.20*		1.20*	1.19	1.19	1.19	1.39*	1.39*	1.39*	1.20	1.20	1.20	0.98	0.98	0.98
Incarceration ^a	0.65	0.68	0.65	1.17	1.21	1.00	2.90***	2.78***	3.16***	2.96**	2.74**	2.35*	2.72**	2.27**	1.94*	2.60**	2.04*	1.64
Age 32 Outcome	4.90***	4.56***	4.56***	2.35*	2.06	2.06	1.07	1.07	1.08	1.16	1.16	1.17	1.47**	1.47**	1.42**	1.74***	1.74***	1.68**
Never Married		0.88	0.88	1.34		1.34	0.69	0.69	0.69	1.73	1.73	1.73	1.49	1.49	1.49	1.80	1.80	1.80
Times Unemployed		1.07	1.07	1.15		1.15	0.96	0.96	0.96	1.05	1.05	1.05	1.07	1.07	1.07	1.00	1.00	1.00
Females																		
Arrest	2.22**	1.91*	1.91*	3.88***	3.35**	3.40**	6.68***	5.73***	5.92***	3.00***	2.66**	2.75**	29.03***	16.21***	17.04***	21.13***	14.67***	14.07***
Age 32 Outcome	5.44***	5.56***	5.56***	3.60**	3.60**	3.52**	1.35*	1.35*	1.31	1.46**	1.46**	1.50**	3.80***	3.80***	4.06***	2.77***	2.77***	2.66***
Never Married		2.02*	2.02*	4.29***		4.29***	0.74	0.74	0.74	1.27	1.27	1.27	1.12	1.12	1.12	1.94*	1.94*	1.94*
Times Unemployed		0.98	0.98	0.98		0.98	1.02	1.02	1.02	0.97	0.97	0.97	0.95	0.95	0.95	1.02	1.02	1.02
Incarceration ^a	0.45	0.36	0.36	0.31	0.29	0.22	0.73	0.59	0.53	1.65	1.46	1.36	1.24	0.96	1.04	2.93**	2.43*	2.41*
Age 32 Outcome	3.77*			1.50	1.54	1.54	1.11	1.11	1.11	1.09	1.09	1.09	1.42*	1.42*	1.40	1.14	1.14	1.15
Never Married				5.28		5.28	1.80	1.80	1.80	1.47	1.47	1.47	1.83	1.83	1.83	0.70	0.70	0.70
Times Unemployed				1.02		1.02	1.03	1.03	1.03	1.04	1.04	1.04	0.98	0.98	0.98	1.02	1.02	1.02

^aAll incarceration analyses control for childhood and adolescent variables with a standardized bias greater than .10 in the propensity score matching procedure.

^bLogistic Regression, Odds Ratios reported

^cNegative Binomial for all models except Poisson Regressions were conducted for male incarceration on self-reported violence, female arrest on official violent and property offending, and incarceration on self-reported and official violent and property offending. Incidence Rate Ratios reported.

NA = This model would not converge.

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'10<.01
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'p<.05
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