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LABELING, LIFE CHANCES, AND ADULT CRIME: THE DIRECT AND INDIRECT EFFECTS OF OFFICIAL INTERVENTION IN ADOLESCENCE ON CRIME IN EARLY ADULTHOOD*

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Scholars have recently revitalized labeling theory as a developmental theory of structural disadvantage. According to this approach, official intervention increases the probability of involvement in subsequent delinquency and deviance because intervention triggers exclusionary processes that have negative consequences for conventional opportunities. The theory predicts that official intervention in adolescence increases involvement in crime in early adulthood due to the negative effect of intervention on educational attainment and employment. Using panel data on urban males that span early adolescence through early adulthood, we find considerable support for this revised labeling approach. Official intervention in youth has a significant, positive effect on crime in early adulthood, and this effect is partly mediated by life chances such as educational achievement and employment.

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The labeling perspective of deviant behavior has been the subject of considerable debate among students of crime and deviant behavior. This perspective argues that official intervention can be a stepping stone in the

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development of a delinquent career. In the late seventies and early eighties, critics argued that the labeling approach as originally presented (Becker, 1963; Lemert, 1967) was vague and ambiguous and failed to provide empirically testable propositions. Moreover, research findings failed to provide evidence consistent with the theory (Hirschi, 1980; Tittle, 1980). However, recent work suggests that a rejection of the labeling approach may have been unjustified. Efforts to modify labeling theory by explicating the social processes that translate deviant labeling into a deviant career or "secondary deviance" and by providing empirically testable propositions regarding the consequences of deviant labeling (see Paternoster and Iovanni, 1989, for a review of this issue) have provided theoretical clarity.

These theoretical developments have underscored the developmental nature of labeling theory. Sampson and Laub (1997:138) have characterized labeling theory as "truly developmental in nature, because of its explicit emphasis on processes over time" (see Loeber and LeBlanc, 1990). Deviant labeling, official labeling in particular, is seen as a transitional event that can substantially alter the life course by reducing opportunities for a conventional life (Becker, 1963; Link, 1982; Link et al., 1989). Thus, labeling is seen as being indirectly related to subsequent behavior through its negative impact on conventional opportunities. Sampson and Laub (1997) suggest that labeling is one factor that leads to "cumulative disadvantage" in future life chances and, thereby, increases the probability of involvement in delinquency and deviance during adulthood.

Recent reviews of labeling research have concluded that there is limited research on mediational processes and the long-term effects of labeling (Paternoster and Iovanni, 1989; Sampson and Laub, 1997). Moreover, the methodological rigor of prior research on the consequences of official labeling has been questioned, rendering an appreciable part of existing evidence open for reevaluation. The present study uses panel data that span early adolescence through early adulthood to explore the importance of labeling in the development of structural disadvantage. Our main purpose is to examine whether official intervention in adolescence increases involvement in crime in early adulthood due to the negative effect of intervention on educational attainment and employment.

DEVIANT LABELING AND STRUCTURED OPPORTUNITIES

Common to the classic labeling theories (Becker, 1963; Lemert, 1967) is the view that deviant labeling can have a profound, detrimental impact on the person's social standing and may thus be a crucial step in building a stable pattern of deviant behavior. Due to the widespread cultural imagery attached to deviant statuses, people tend to assume that deviants

possess undesirable traits allegedly associated with their status. In Lemert's (1967:17) famous conceptualization of secondary deviance, deviant behavior can be a "means of defense, attack, or adaptation to the overt and covert problems created by the societal reaction to primary deviation."

Although often referred to as a single theory, within the labeling perspective, there are arguments that differ in important respects. This fact has become increasingly apparent in recent theoretical work where scholars have explicated the processes through which deviant labeling may influence involvement in subsequent deviant behavior (Link, 1982; Link et al., 1989; Matsueda, 1992; Paternoster and Iovanni, 1989; Sampson and Laub, 1997). In a recent review of the literature, Liska and Messner (1999:118-125) identify two major theoretical perspectives. First, deviant labeling may influence subsequent deviance by altering the person's self-concept. This approach, which has been recently developed in the work of Matsueda (1992), highlights the role of the self and the dynamics of symbolic interaction; deviance amplification occurs when the labeled person conforms to the stereotypical expectations of others. The second approach focuses on the more tangible (social structural) aspects of social exclusion; deviance is stabilized due to blocked access to structured opportunities and conventional others.

In the present paper, we focus on the latter perspective and empirically examine the mediating role of structured opportunities. This perspective has received attention recently in developmental criminology, specifically in Sampson and Laub's (1993, 1997) life-course approach. These authors build on the systematic effort of Link and his colleagues to emphasize the social structural implications of labeling theory (Link, 1982; Link et al., 1989). Sampson and Laub describe how official intervention during adolescence may negatively impact future life chances and, therefore, increase the likelihood of later involvement in delinquency and deviance. This approach views public labeling as a transitional event (where the deviant label is not necessarily a permanent social status) that tends to push young people on a trajectory of structural disadvantage and involvement in deviance and crime. A key notion is that the application of a deviant label, which is most successfully (but not only) achieved by official intervention (Becker, 1963), during a crucial period in the life course tends to lead to marginalization from conventionally structured opportunities, particularly as these are shaped by education and employment. In turn, due to these problems, the likelihood of subsequent deviance increases. The exclusionary and stigmatizing effect of deviant labeling may be an important explanatory factor as to why some individuals continue to deviate later in life (Sampson and Laub, 1997:147-148):

Cumulative disadvantage is generated most explicitly by the negative

structural consequences of criminal offending and official sanctions for life chances. The theory specifically suggests a "snowball" effect—that adolescent delinquency and its negative consequences (e.g., arrest, official labeling, incarceration) increasingly "mortgage" one's future, especially later life chances molded by schooling and employment. . . . The theoretical perspective in turn points to a possible indirect effect of delinquency and official sanctioning in generating future crime.

Official intervention may negatively affect educational attainment by triggering stigma and exclusion in school. Bodwitch (1993) has found that students defined as having a delinquent character by school officials are subject to harsher disciplinary procedures, such as temporary suspension, transfer to another school, or even expulsion. Also, incarceration can directly impede educational opportunities. Educational attainment, in turn, shapes employment opportunities in adulthood. In addition, an official label may directly impede employment opportunities. First, many employers may avoid hiring known delinquents (Schwartz and Skolnick, 1962), and second, individuals who have experienced official intervention may expect and fear rejection from conventional others, including employers, and thus be less likely to apply for good jobs (Link, 1982). Blocked educational and employment opportunities weaken the "social and institutional bonds linking adults to society" (Sampson and Laub, 1997:144). Over time, then, the social marginalization caused by the stigma attached to the deviant label raises the likelihood of subsequent, even more stable, involvement in deviant activity.

STRUCTURAL LOCATION AND LABELING

Clarifications and extensions of labeling theory have emphasized the contingent nature of this theory. Official intervention, such as being arrested, convicted, or sent to a mental hospital, does not automatically lead to deviant labeling (Paternoster and Iovanni, 1989). Structural location, such as race or social class, may provide people with differential means to resist deviant labeling in the face of official intervention.

From a life-course perspective, Sampson and Laub (1997) argue that disadvantaged structural location should facilitate labeling effects. "Deficits and disadvantages pile up faster" among the disadvantaged (p. 153). Due to higher stakes in conformity and continuity in social resources over time, high structural location should diminish the effect of labeling. We also suggest that deviant labeling of disadvantaged youths who are processed by the police and the juvenile justice system is enhanced by the negative stereotypes that are already associated with these youths in the mainstream culture (see Gans, 1995). Conversely, other scholars have

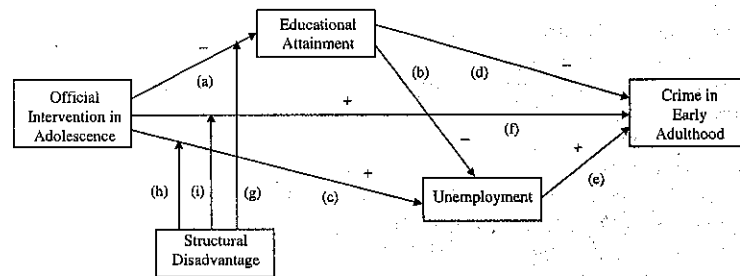
argued that higher status people may be more vulnerable to labeling than are the disadvantaged because they have more to lose (Ageton and Elliott, 1974; Jensen, 1972). Although official intervention may, indeed, impact higher status people more than it does lower status people, the argument of Sampson and Laub (1997) concerning the impact of labeling on conventional opportunities are compelling and more consistent with arguments relating to the structural effects of labeling. Hence, we hypothesize that the impact of official intervention on educational and employment opportunities and on early adult criminality will be stronger among the more disadvantaged offenders. Specifically, we examine whether race and poverty status moderate the effect of official intervention on educational and employment opportunities.

The theoretical model is depicted in Figure 1. Official intervention is hypothesized to have a negative effect on educational attainment (Path a). In turn, reduced educational attainment should increase unemployment in early adulthood (Path b). In addition, official intervention may carry a social stigma that blocks employment opportunities due to direct discrimination (Schwartz and Skolnick, 1962) or expectation of rejection (Link, 1982), independent of the effect such intervention has on educational attainment. Hence, official intervention should have a significant effect on unemployment, while controlling for educational attainment (Path c). Both educational attainment and unemployment should have direct effects on early adult crime (Paths d and e, respectively). Official intervention should also have a direct positive effect on adult crime (Path f). However, educational attainment and unemployment should mediate this effect on adult crime (Path ad and Path ce, respectively). Finally, the theory implies interaction effects; structural disadvantage should enhance the effect of official intervention on educational attainment, unemployment, and adult crime (Paths g, h, and i, respectively).

PRIOR RESEARCH

Although numerous studies have examined various consequences of official labeling (see Bernburg, 2002; Palarma et al., 1986; and Paternoster and Iovanni, 1989, for reviews), methodological problems of prior research limit what we can conclude from them. Specifically, much prior research suffers from the following deficiencies: (1) many studies include only individuals who have experienced intervention, (2) most studies are either cross-sectional or entail relatively short follow-up periods, (3) most studies do not investigate intervening processes that may mediate the relationship between official labeling and subsequent delinquent behavior, and (4) few studies examine whether the relationship between labeling and both structural mediators and subsequent delinquency is contingent on structural

Figure 1. Hypothesized Effects of Official Intervention in Adolescence on Crime in Early Adulthood



location. Our review of prior research is organized around these four deficiencies.

SAMPLE SELECTION

Paternoster and Iovanni (1989) conclude their review of the labeling literature by noting that the research has often been based on samples of individuals drawn from police records and similar nonrandom sources (e.g., Horowitz and Wasserman, 1979; Sampson and Laub, 1993; Smith and Paternoster, 1990; Zhang and Messner, 1994). This strategy is problematic for two reasons. First, comparisons are limited to a focus on the effects of different sanctions (juvenile court appearance, probation, or incarceration), suggesting that the relative harshness or seriousness of the sanction is related to the probability of subsequent delinquent behavior. The researcher is thus limited to examining the relative, rather than the absolute, effect of labeling. There is no comparison between those who have not experienced official intervention and those who have. This is particularly problematic considering that labeling theorists emphasize the importance of the initial labeling event as a dramatic event, a transition to a new status (Becker, 1963; Lemert, 1967), as follows (Paternoster and Iovanni, 1989:385):

When one takes for study a group which appears at the end of a long series of discretionary decisions, it is reasonable that the labeling process has run its course by that time. Having already experienced a repudiation of character and an exclusion from the normal routines of life, the "hard-core" offenders may be immune to additional labeling effects.

Consistent with this view, the relatively few studies that have used samples from the general population tend to support hypotheses derived from

labeling theory (Farrington, 1977; Hagan and Palloni, 1990; Palarma et al., 1986; Ray and Downs, 1986; Thomas and Bishop, 1984), whereas studies using samples of offenders have produced mixed results (e.g., Horowitz and Wasserman, 1979; Klein, 1974; McEachern, 1968; Smith and Paternoster, 1990).

Paternoster and Iovanni (1989) suggest that an additional problem with using samples of offenders is that too often the impact of the police is overlooked. Most offenders who experience any official intervention will have had an encounter with the police. Paternoster and Iovanni (1989:383) suggest:

Because only a small proportion of deviants undergo these experiences, it may be more germane from a theoretical and a policy standpoint to examine the consequences of an experience common to a larger number of juveniles, such as encounters with the police.

TIMEFRAME OF THE STUDY

Although panel data with long follow-up periods are required to study the long-term effects of labeling on structured opportunities, research on labeling effects has mostly been based on cross-sectional data and panel studies entailing short follow-up periods that "rarely include the developmental transition from adolescence to adulthood" (Sampson and Laub, 1997:139-141; examples include Ageton and Elliott, 1974; Horowitz and Wasserman, 1979; Ray and Downs, 1986; Smith and Paternoster, 1990; and Thomas and Bishop, 1984). A strategy of examining short-term rather than long-term effects is particularly problematic when emphasizing the mediating effects of structural constraints. Labeling may occur early in the life course, but impaired life chances may slowly unfold in the years to come. Official intervention during adolescence may have an observable impact as late as in early adulthood when individuals become fully affected by the deviant label's impact on life chances through diminished chances of education and stable employment.

Although studies examining the long-term effects of official intervention are limited, there is some evidence that supports this line of inquiry. Hagan and Palloni (1990) found a significant effect of conviction in adolescence on delinquent behavior in early adulthood, net of delinquency in adolescence and numerous control variables. Sampson and Laub (1993) show a significant amplification effect of length of incarceration on adult criminal behavior. However, the representativeness of these findings is limited. The study by Hagan and Palloni is limited to British working-class males, whereas Sampson and Laub limited their analysis of the effect of official intervention to individuals incarcerated during adolescence.

INTERVENING PROCESSES

Labeling theory argues that deviant labeling sets in motion various, specific social processes that impact subsequent deviant behavior. However, research has rarely examined the role of intervening processes in translating labeling into subsequent deviance. Although there is evidence suggesting that official labeling negatively affects opportunities for employment and education (Bodwitch, 1993; Freeman, 1991; Hagan, 1991; Link, 1982; Schwartz and Skolnick, 1962; Sullivan, 1989), researchers have failed to examine whether conventional opportunities mediate the relationship between official labeling and subsequent delinquent behavior. Paternoster and Iovanni conclude that "by failing to consider the requisite intervening effects, the bulk of these studies do not constitute a valid test of labeling theory" (1989:384).

Although research is limited on this point, recent studies provide evidence consistent with an intervening role of educational attainment and employment stability in the labeling process. Sampson and Laub's (1993) study found that employment stability in adulthood mediates the relationship between length of incarceration before age 17 and criminal behavior in adulthood (controlling for prior delinquency and other important factors). Although not providing a formal test of mediational processes, Sampson and Laub found that the effect of incarceration on subsequent criminal behavior drops below the significance level when controlling for job stability in adulthood.

There is some indirect evidence suggesting that official labeling may have a negative impact on educational attainment. Hagan (1991) found that police contact in adolescence negatively impacts occupational status in early adulthood, partly through educational attainment. However, this study is limited in that it controls for delinquent preferences rather than actual delinquent activities in adolescence. In studying the effect of adolescent delinquency on adult outcomes, Tanner et al. (1999) found that educational attainment partly mediates the relationship between police contact in adolescence and unemployment in adulthood, while controlling for delinquent behavior in adolescence. Finally, Menard and Morse (1984) provide some evidence supporting the notion that negative social labeling by teachers and by significant others leads to school alienation.

CONTINGENT RELATIONSHIPS

There has been limited research on how structural location, particularly race and social class, conditions the effect of labeling on subsequent life-course opportunities and delinquency. Existing research has generated inconsistent results. Hagan and Palloni (1990) found that being convicted of a crime had greater positive effects on subsequent delinquency for sons

of parents who had also been convicted. They interpret this finding by suggesting that official intervention when combined with a stigmatized status (parental conviction) generates a stronger labeling effect. There is some experimental evidence suggesting that arrest for domestic violence increases the likelihood of subsequent violence only among individuals who are unemployed (Berk et al., 1992; Sherman and Smith, 1992). Conversely, Ageton and Elliott (1974) found a weak but statistically significant relationship between police contact and subsequent delinquent "orientations" for white juveniles but not for blacks (see also Jensen, 1972).

PRESENT STUDY

The present study addresses hypotheses derived from structural labeling theory with panel data on a stratified random sample of males living in Rochester, New York. Data are available from when these youths were on average 13.5 years old to when they were 22 years old. The panel design, combined with the richness of the measurement space, allows us to contribute to the literature on labeling theory in three significant ways. First, we examine the effect of both police intervention and juvenile justice intervention on subsequent early adulthood crime using a random sample from a population of adolescents. Second, we examine the long-term effect of official intervention during adolescence on young adult criminality and test whether educational attainment and periods of nonemployment mediate this effect. Finally, we test whether the effect of official labeling on life chances and future crime is contingent on race and impoverished family background.

METHOD

The analysis is conducted with data from the Rochester Youth Development Study (RYDS), a multiwave panel study of the development of delinquent behavior among adolescents and young adults. This panel is based on an initial sample of 1,000 students selected from the seventh and eighth grades of the public schools in Rochester, New York, during the 1987-1988 academic year. Interviews were conducted at 6-month intervals over a 4½-year period (Waves 1 through 9) with each adolescent and his or her parent or primary caretaker. After a 2½-year gap, adolescents and parents were interviewed once a year for the next 3 years (Waves 10 through 12). All interviews were conducted in private settings; most were face-to-face, but in later waves some long distance interviews were completed by telephone. Chronic truants and students who had left the Rochester schools were interviewed at their homes, as were most parents. Data on subjects were also collected from school, police, courts, and social service agencies.

SAMPLE

The Rochester Youth Developmental Study was designed to oversample youth at high risk for serious delinquency and drug use because the base rates for these behaviors are relatively low (Elliott et al., 1989). To accomplish this while being able to generalize the findings to a population of urban adolescents, the following strategy was used. The target population was limited to seventh- and eighth-grade students in the public schools of Rochester, New York, a city that has a diverse population and a relatively high crime rate.

The sample was then stratified on two dimensions. First, males were oversampled (75% versus 25%) because they are more likely than females to be chronic offenders and to engage in serious delinquency (Blumstein et al., 1986). Second, students from high crime areas of the city were oversampled on the premise that subjects residing in high crime areas are at greater risk for offending. To identify high crime areas, each census tract in Rochester was assigned a resident arrest rate reflecting the proportion of the tract's population arrested by the Rochester police in 1986.

Because the true probability of each adolescent being selected is known, the sample can be weighted to represent all seventh and eighth graders in the Rochester Public Schools. We weight the sample using the procedure suggested by Kish (1965:77-9) in the analysis to follow.

There are 1,000 adolescents in the base panel. At Wave 12, 846 individuals remained in the study. This represents a retention rate of 85%. Comparing the characteristics of respondents who are included in the present analysis with the total sample indicates that attrition did not bias the sample (Krohn and Thornberry, 1999). We use only the male respondents in the present study, a total of 605 males.¹ A deletion of cases due to missing values results in 529 valid cases. Deleting the cases with missing values does not change the demographic composition of our sample.² The present analysis is based on data covering a nine-year time period from when

1. In the RYDS, questions tapping the homemaker role were not included. Therefore, we are unable to measure nonemployment among females.

2. There are no significant differences in the demographics between the cases that remain in the analysis and those lost due to incomplete data ($p > .05$; two-tailed). Neither is there a significant difference in our adolescent delinquency measure between the two groups ($p > .05$; two-tailed). To determine if our results are affected by the loss of these cases, we replicated the analyses presented by using a Markov Chain Monte Carlo multiple imputation procedure, which is in an experimental stage in SAS (SAS Institute, 2001; option "PROC MI" and "PROC MIANALIZE" in SAS; see Schafer, 1997). This procedure produces and combines several different sets of missing values that reflect the uncertainty about the predictions of the unknown missing values. Using this experimental procedure, we retain 582 of the total sample of 605 cases. The results are substantively similar to those presented in this paper.

the subjects were approximately 13 years old until they were about 22 years old.

MEASURES

OFFICIAL INTERVENTION

Using police records, we construct a dummy variable labeled *police intervention*, with "1" equal to having a recorded arrest or police contact in Waves 1 through 7, or approximately in the period between the ages of 13.5 to 16.5, and "0" equal to having neither arrest nor police contact in this period.³ We do not have reliable official records about intervention with the juvenile justice system during this period. However, we do have self-reported data on involvement with the juvenile justice system for serious forms of violence, property offenses, and drug use. If the subject indicated that he had contact with the police, he was also asked if he had further juvenile justice system involvement (put on probation, sent to a correctional center, referred to community service, put in detention, brought to court, or referred to a treatment program in Waves 1 through 7). At each wave, the subject was asked only about the most serious intervention; therefore, we only know if the subject had been through the juvenile justice system at least once. Thus, a second dummy variable, *juvenile justice intervention*, was constructed with "1" equal to some involvement with the juvenile justice system and "0" equal to no involvement. Because these two measures are expected to overlap, we use them as alternative measures of official intervention.

CRIMINAL BEHAVIOR IN EARLY ADULTHOOD

Wave 10 and Wave 12 interviews included self-report inventories from which information on offending over the past year was elicited. We compute three measures of adult criminal behavior from the items in this inventory. To measure involvement in criminal behavior at ages 19-20 (Wave 10), we use both a serious crime index (consisting of seven non-overlapping items, including robbery, gang fights, attacks with a weapon, breaking and entering, theft of \$50-\$100, theft of more than \$100, and car theft) and the number of drug sales in which subjects engaged. The serious crime measure was used in order to tap the type of street crime that would be expected among this population as a result of official intervention. Given the theoretical argument that intervention inhibits conventional opportunities, we also include drug sales to provide a measure of crime that could be assumed to be economically motivated. At ages 21-22

3. We have replicated the analyses reported in the present paper by using police contact and police arrest separately. This strategy produces a similar pattern of results.

(Wave 12), there is not sufficient variation on the serious crime index, and therefore, we use a general crime index consisting of 32 nonoverlapping items ranging in degree of seriousness from minor crimes such as vandalism to major crimes such as robbery and assault. In addition, we use a measure of drug sales similar to that from Wave 10. We discuss statistical issues regarding the dispersion of these variables in the Results section.

INTERVENING VARIABLES

Information about educational attainment and employment stability is obtained from the subject interviews. In New York State, a person is not allowed to attend high school on a regular basis after the age of 20. Hence, those subjects who report graduating from high school have done so no later than the time that we measure periods of nonemployment and adult crime.⁴ A dummy variable measures educational attainment, with "1" equal to graduating from high school and "0" equal to no high school graduation (includes GED). Another education variable that indicates whether the subject attended school in Waves 8 and 9 (1 = yes; 0 = no) is also used. Periods of nonemployment are measured by the proportion of months that the subjects reported being nonemployed in a given period. We define an individual as nonemployed during a month that he is not fully or partly employed, in school (college or high school), or serving in the military. We take the square root of nonemployment to adjust for skewness in the distribution (see Rummel, 1970:280-6). We use two measures of nonemployment. When we treat it as a dependent variable (Table 3) we measure it for ages 19-22. However, when we examine the intervening effect of nonemployment (on crime measured in Wave 12), we measure it at Waves 10 and 11 to assure proper temporal order.

CONTROL VARIABLES

In the analyses to follow, we control for a number of variables that are potentially related to official intervention, educational attainment, periods of nonemployment, and adult crime. Specifically, we control for adolescent serious delinquent behavior, race/ethnicity, parental poverty, and academic aptitude.

We measure adolescent serious delinquency with a self-report measure of cumulative serious delinquent activities in Waves 2 through 7, or approximately between the ages of 14 and 16. The measure consists of seven nonoverlapping items, including robbery, gang fights, attack with a

4. Seven males graduated from high school after Wave 10 (which ends at about age 20). We have replicated the analyses (not shown) reported in Tables 3, 4, and 5 coding these individuals as not having graduated. The results are substantively identical to those presented here.

weapon, breaking and entering, theft of \$50-\$100, theft of more than \$100, and car theft. A square root transformation of this variable is conducted to adjust for skewness in the distribution (see Rummel, 1970).

Information about race/ethnicity was obtained in the first wave of interviews. Two dichotomous variables represent the racial/ethnic categories African American and Hispanic, with white serving as the reference category (coded "0").

Poverty status is measured by a dummy variable with "1" equal to the household having an income below the poverty line and "0" equal to income above the poverty level. Information about the income of the household is obtained directly in interviews with the primary caretakers of the subjects in the first wave (when subjects were about 13.5 years of age). If income information was not available from Wave 1, we used data from Wave 2 or 3.

We measure subject's academic aptitude with the percentile ranking on the California Achievement Test in math in 1987 (when subjects are about 12 years of age). Thus, this measure serves as an indicator of academic ability in the year preceding the first wave of interviews.⁵ Table 1 shows the mean and standard deviation of the variables after weighting the sample. Experiencing juvenile justice intervention and particularly police intervention is quite common among males in our sample. Among the 529 males included in the analyses, 39% had a record of police intervention and 12% reported juvenile justice intervention between Waves 1 and 7.

RESULTS

Figure 1 will serve to organize the presentation of results, and the relevant paths will be referred to in reporting the findings. We first examine the effects of official intervention on educational attainment and employment. We then examine whether these measures of life chances mediate the effect of official intervention on adult crime.

DOES OFFICIAL INTERVENTION INFLUENCE LIFE CHANCES?

In Table 2, we examine the effect of official intervention on educational attainment (Path a in Figure 1). In Models 1 and 2, we present results from a logistic regression of high school graduation by age 22 on official intervention and the control variables. As predicted, both types of official intervention in adolescence significantly reduce the odds of graduating

5. In six cases, we substituted missing values on the math exam with percentile ranking on the California reading exam. Excluding those six cases produces identical findings to those reported here (N = 523). We replicated the results (not shown) using the reading exam instead of the math exam. This analysis also produced identical results to those reported here.

Table 1. Variable Descriptives ($N = 529$)

	Mean	S.D.
Police intervention, Waves 1-7 (ages 13.5-16.5) yes = 1, no = 0	.39	.49
Juvenile justice intervention, Waves 1-7 (ages 13.5-16.5) yes = 1, no = 0	.12	.32
Incidence of serious crime, Wave 10 (ages 19-20)	.36	3.76
Incidence of general crime, Wave 12 (ages 21-22)	54.52	214.86
Incidence of drug selling, Wave 10 (ages 19-20)	11.36	65.64
Incidence of drug selling, Wave 12 (ages 21-22)	33.97	219.27
High school graduation = 1, otherwise = 0	.53	.50
Attended school, Waves 8-9 (ages 17-17.5) yes = 1, no = 0 ^a	.86	.35
Nonemployment between nineteenth and twenty-second birthdays ^b	.32	.32
Nonemployment in Waves 10 and 11 ^b	.25	.32
Cumulative incidence of serious adolescent delinquency, Waves 2-7 (ages 14-16.5) ^b	.88	1.62
African American = 1, otherwise = 0	.55	.50
Hispanic = 1, otherwise = 0	.16	.37
Parental poverty = 1, otherwise = 0	.26	.43
Math score percentile, age 12 (California Achievement Test)	.60	.27

NOTES: Descriptives are weighted. Age indicates mean age unless otherwise specified.

^a Subjects who left school by Wave 7 were deleted prior to calculating descriptive statistics for this variable ($N = 429$).

^b Variable has been transformed by a square root transformation.

from high school, net of other variables in the model. The estimated odds ratios indicate that official intervention decreases the odds in favor of high school graduation by more than 70%.

The analysis in Models 1 and 2, however, does not temporally separate official intervention and subsequent educational disruption. In Models 3 and 4, we examine whether official intervention (in Waves 1 through 7) reduces the odds of attending school in a subsequent period (in Waves 8 and 9).⁶ To ensure a clear temporal design, males who have left school by Wave 7 (by about age 16.5) are dropped from the analysis. The findings

6. The RYDS data provide information on official intervention at six-month intervals from age 13.5 (Wave 1) to age 16.5 (Wave 7) and on staying in school to age 17.5 (Wave 9). We have separated intervention and staying in school temporally for a total of six pairs of successive wavespan (not shown), regressing staying in school at the

Table 2. Logistic Regression of High School Graduation and Attended School in Waves 8-9

Independent Variables	Model			
	1	2	3	4
	High School Graduation		Attended School in Waves 8-9	
Police Intervention	.26**	—	.26**	—
Juvenile Justice Intervention	—	.28**	—	.48*
Serious Adolescent Delinquency	.65**	.65**	.75	.80
Parental Poverty	.58**	.49**	.89	.80
Math Score	1.01**	1.01**	1.02**	1.01*
African American	1.79**	1.27	1.32	.89
Hispanic	.64	.61	1.16	1.14
Likelihood Ratio	141.0	112.9	31.1	15.0
d. f.	6	6	6	6
<i>N</i>	529	529	429	429

NOTES: Odds ratios are reported. In Models 3 and 4, all subjects who left school by Wave 7 are deleted from the analysis.

* $p < .05$; ** $p < .01$ (one-tailed).

support the causal direction suggested by labeling theory. Experiencing official intervention in adolescence is significantly associated with reduced odds in favor of staying in school in a subsequent period.

We created product terms (table not presented) to examine whether the effects of official intervention on educational attainment are dependent on race and poverty status (Path g). The interaction terms (race \times official intervention, impoverished family background \times official intervention) were not significant ($p > .05$; one-tailed test). Thus, our findings do not support the hypothesis that the effect of official intervention on educational attainment is contingent on structural location.

latter wavespan (Time k) on a variable coded "1" if the subject has experienced intervention at any prior wave (Time $k - 1$). For example, we looked at any reports of official contact in Waves 1 through 3 and then at whether they stayed in school in Waves 4 through 9, then any contact in Waves 1 through 4 and staying in school in Waves 5 through 9, and so on. To ensure a clear temporal design, youths who leave school during wavespan $k - 1$ were dropped from the analyses (minimum $N = 429$). The findings from this analysis are similar to those reported in Table 2. Official intervention during the $k^h - 1$ wavespan significantly reduces the odds in favor of staying in school during the subsequent k^h wavespan. See Bernburg (2002) for an analysis of this issue.

In Table 3, we assess the effect of official intervention on periods of nonemployment in early adulthood (Paths ab and c).⁷ To see the total effect of official intervention on nonemployment, we first regress nonemployment on intervention without including high school graduation (Models 1 and 3, respectively). As predicted, both police intervention and juvenile justice intervention are positively and significantly related to periods of nonemployment in adulthood (Path c).⁸

Educational attainment may mediate some of the effect of official intervention on nonemployment (Path ab). We examine this possibility in Models 2 and 4 by adding high school graduation. Educational attainment has a sizeable and significant negative effect on nonemployment and reduces the regression coefficient for police intervention by 33% (from .15 to .10) and the coefficient for juvenile justice intervention by 40% (from .10 to .06). Sobel's (1982) test of the significance of indirect effects indicates that educational attainment significantly mediates part of the effect of both police intervention and juvenile justice intervention on nonemployment ($t = 3.94$ and 2.76 , respectively).⁹ However, the effect of police intervention on nonemployment in early adulthood remains statistically significant and positive (Path c).

We created product terms (table not presented) to examine if the effect of official labeling on nonemployment is contingent on race and impoverished family background (Path h). The product terms were not statistically significant.

LABELING, LIFE CHANCES, AND ADULT CRIME

Does official intervention affect involvement in serious criminal activity in early adulthood? As our dependent variable in this part of the analysis is measured as event counts, the use of classic linear modeling is problematic (Beck and Tolnay, 1995; King, 1988). To deal with this issue, we use a

7. White's (1980) test for heteroscedasticity indicates that some of the models in Table 3 violate the assumption of homoscedasticity. To adjust for heteroscedasticity in the error variances, we follow White (1980) and obtain the standard errors from the consistent covariance matrix.

8. Forty-two males reported that they had been in a correctional facility for at least a month during the period when nonemployment was measured. As an individual may be defined as nonemployed due to incarceration, we have replicated the analysis in Tables 3, 6, and 7 (not shown), controlling for the number of months subjects were in a correctional facility. The results from this analysis are substantively identical to those reported here.

9. Sobel (1982) derives the following formula to obtain the standard error for an indirect Path xz : $SQRT(x^2S_z^2 + z^2S_x^2 + S_x^2S_z^2)$, where x is the unstandardized effect of the independent variable on the mediator and z the effect of the mediator on the dependent variable.

Table 3. OLS Regression of Nonemployment in Early Adulthood (ages 19–22)

Independent Variables	Model			
	1	2	3	4
Police Intervention	.15** (.04)	.10** (.03)	—	—
Juvenile Justice Intervention	—	—	.10** (.05)	.06 (.05)
Serious Adolescent Delinquency	.01 (.01)	-.00 (.00)	.02** (.01)	-.00 (.01)
Parental Poverty	.10** (.04)	.08** (.04)	.11** (.03)	.09** (.04)
Math Score	-.002** (.001)	-.001** (.001)	-.002** (.001)	-.001** (.001)
African American	.13** (.03)	.14** (.03)	.16** (.03)	.16** (.03)
Hispanic	.11* (.05)	.09 (.05)	.11 (.06)	.09 (.05)
High School Graduation	—	-.17** (.03)	—	-.19** (.03)
Intercept	.24	.33	.28	.36
R ²	.21	.27	.18	.25

NOTES: $N = 529$. Unstandardized coefficients are reported. Standard errors are in parentheses. (The standard errors are obtained from the consistent covariance matrix to adjust for heteroscedasticity.)

* $p < .05$; ** $p < .01$ (one-tailed).

modified Poisson regression procedure.¹⁰ Tables 4 and 5 present a series of Poisson regressions where the dependent variable is the number of serious crimes and drug sales in which males engaged at ages 19–20. In Tables 6 and 7, we use general crime and drug selling at ages 21–22 as the dependent variables.

10. The standard Poisson regression assumes that the conditional mean of the dependent variable is equal to its variance, an assumption that often does not hold empirically for count variables, and does not seem to hold for our dependent variables (see Hagan and Palloni, 1990; Sampson and Laub, 1993). The modified Poisson regression relaxes this restriction by adding an unknown error term to the Poisson regression (Beck and Tolnay, 1995). This unknown parameter and the effect parameters are then estimated using quasi-maximum likelihood. As our dependent variables display overdispersion, this method produces larger standard errors than the standard Poisson regression.

We first consider the effects of police intervention and juvenile justice intervention on involvement in serious crime at ages 19–20 (Table 4). Model 1 shows the effect of police intervention on serious crime, without controlling for educational attainment. As predicted, police intervention is significantly associated with increased serious crime in early adulthood, net of the control variables (Path f). The odds ratio for this effect indicates that police intervention in youth increases the predicted number of crime events at ages 19–20 by a factor of 1.63 ($e^{.49}$).¹¹

Labeling theory emphasizes the indirect effects of criminal labeling on subsequent deviant behavior. We have hypothesized that educational attainment should mediate some of the effects of official intervention on subsequent crime (Path ad). Consistent with this hypothesis, adding high school graduation to the equation (Model 2) produces a substantial drop in the regression coefficient for the effect of police intervention on early adult crime, about a 40% drop (compare Models 1 and 2). Moreover, educational attainment has significant, negative effects on adult crime (Path d). Educational attainment significantly mediates a part of the effect of police intervention on crime at ages 19–20 ($t = 2.40$).¹²

Models 3 and 4 present the same procedure for juvenile justice intervention. The results in Model 3 show that juvenile justice intervention has significant effects on crime at ages 19–20 (Path f), increasing the predicted number of serious criminal events by a factor of 5.31 ($e^{1.67}$). Adding educational attainment to the equation produces an 8% drop in the regression coefficient for the effect of juvenile justice intervention on crime (compare Models 3 and 4). The indirect effect of juvenile justice intervention on crime through educational attainment (Path ad) is statistically significant ($t = 1.67$; one-tailed).

We have hypothesized that the effect of official intervention in youth on early adult crime is contingent on structural location (Path i). As disadvantage cumulates more rapidly among the already disadvantaged, and because powerless groups may be relatively less able to resist public labeling when official intervention occurs, official intervention should have a stronger effect on subsequent deviance among minorities and people with impoverished backgrounds (Sampson and Laub, 1997). We assess this hypothesis by creating product terms, which we estimate simultaneously to test for the presence of an interaction effect between official intervention

11. Replicating this analysis using the general crime index instead of the serious crime index produces similar results.

12. It is possible that in some cases subjects have left high school prior to official intervention. To ensure that the results concerning the mediating role of educational attainment are robust to this possibility, we have added a dummy variable for leaving school by Wave 7 (analysis not shown). This procedure does not change the results reported in Tables 4 through 7.

Table 4. Modified Poisson Regression of Serious Crime at Ages 19–20 (Wave 10)

Independent Variables	Model					
	1	2	3	4	5	6
Police Intervention	.49* (.26)	.30 (.26)	—	—	-.41 (.54)	—
Juvenile Justice Intervention	—	—	1.67** (.26)	1.53** (.26)	—	-.36 (.71)
Serious Adolescent Delinquency	.24** (.04)	.22** (.04)	.15** (.04)	.14** (.05)	.24** (.04)	.15** (.05)
Parental Poverty	.63** (.24)	.60** (.23)	.63** (.23)	.60** (.23)	-.87 (.64)	-.04 (.37)
Math Score	.002 (.005)	.005 (.005)	.008 (.005)	.009 (.005)	.002 (.005)	.001 (.005)
African American	.54 (.32)	.62* (.33)	.55 (.32)	.58* (.32)	.41 (.42)	.19 (.36)
Hispanic	-.42 (.50)	-.38 (.49)	-.35 (.48)	-.35 (.47)	-.25 (.51)	.32 (.47)
High School Graduation	—	-.76** (.29)	—	-.58** (.28)	-.81** (.29)	-.63** (.28)
Interaction Terms						
Official Intervention × African American	—	—	—	—	.41 (.58)	1.86** (.72)
Official Intervention × Parental Poverty	—	—	—	—	2.01** (.70)	1.24** (.49)
Intercept	-2.28	-2.06	-2.73	-2.53	-1.93	-2.47
Deviance	1,263	1,245	1,185	1,175	1,235	1,148
d. f.	522	521	522	521	519	519

NOTES: $N = 529$. Unstandardized coefficients are reported. Modified standard errors are in parentheses. The standard errors from the Poisson regression are multiplied by a scale parameter, which is estimated by the square root of the deviance divided by the degrees of freedom.

* $p < .05$; ** $p < .01$ (one-tailed).

and structural location. In terms of race/ethnicity, we estimate a product term for African Americans only, collapsing whites and Hispanics into a single reference category.¹³

In Model 5, the interaction effect between police intervention and impoverished status of family is statistically significant. The interaction term is positive, which means that the positive effect of police intervention on crime is stronger among males who have impoverished family backgrounds. The effects of police intervention do not differ significantly by

13. Due to collinearity problems, we cannot estimate product terms for all three racial/ethnic groups in our data. Due to the unique societal status of African Americans, it seems most appropriate to estimate effects for this group.

race/ethnicity. In Model 6, both product terms are statistically significant. Juvenile justice intervention has stronger effects on adult crime among African Americans as well as among males from impoverished backgrounds. In sum, the results suggest that official intervention has stronger crime amplification effects among the disadvantaged.

In Table 5, we replicate the analysis in Table 4 by using the number of drug sales at ages 19–20 as the dependent variable. As before, both police intervention and juvenile justice intervention have significant, positive effects on drug selling (Path *f*; Models 1 and 3, respectively). Moreover, educational attainment accounts for more than 50% of the effect of police intervention (compare Models 1 and 2) and about 20% of the effect of juvenile justice intervention (compare Models 3 and 4). As predicted, educational attainment significantly mediates a part of the effect of police intervention and juvenile justice intervention on drug selling (Path *ad*; $t = 3.08$ and 2.26 , respectively).

In Models 5 and 6, we examine the interaction effects (Path *i*). Only one of the product terms is statistically significant in the direction predicted.¹⁴ Thus, the results in Model 6 indicate that the effect of juvenile justice intervention on drug selling is stronger among those from impoverished family backgrounds.

Theorists have highlighted the role of reduced employment opportunities in mediating the effects of labeling on adulthood crime (Sampson and Laub, 1993, 1997). In Tables 6 and 7, we further examine the mediating role of nonemployment (Path *ce*).¹⁵ To ensure a temporal separation between nonemployment and subsequent crime, we use a measure of general crime at ages 21–22 (Wave 12) and of nonemployment at ages 19–21 (in Waves 10–11). As there was insufficient variation on the serious crime

14. The interaction term for African American status in Model 5 is significant in the opposite direction to what we have predicted. This finding indicates that police intervention actually has weaker effects on drug selling at ages 19–20 among African Americans. We should recognize that there are ambiguities in the literature regarding how race moderates the effects of official labeling. For example, Ageton and Elliott (1974) found significant labeling effects for whites only, whereas Palarma et al. (1986) report the opposite finding. Our data do not allow us to examine the potential reasons why, for this type of crime, African-American status decreases the impact of police intervention on subsequent crime. It may be because of the different way in which the event is perceived among individuals in areas where these activities are more prevalent. Again, this is an issue to be examined in future research.

15. As individuals are defined as nonemployed during periods in which they are not in college, it is possible that some of the observed effects of nonemployment on subsequent crime could be confounded with the effects of not being in college. We have replicated the analysis in Tables 6 and 7 (not shown) controlling for a dummy variable indicating whether subjects were in college during any semester that falls in the Wave 10 to Wave 11 period. The results from that analysis are similar to those reported here.

Table 5. Modified Poisson Regression of Drug Selling at Ages 19–20 (Wave 10)

Independent Variables	Model					
	1	2	3	4	5	6
Police Intervention	.47** (.24)	.22 (.24)	—	—	1.51** (.45)	—
Juvenile Justice Intervention	—	—	1.08** (.25)	.87** (.25)	—	.04 (.53)
Serious Adolescent Delinquency	.22** (.04)	.20** (.04)	.19** (.04)	.17** (.04)	.19** (.04)	.16** (.04)
Parental Poverty	.43* (.22)	.40* (.22)	.47** (.22)	.41* (.22)	.52 (.35)	.10 (.28)
Math Score ₂	.019 (.005)	.022 (.005)	.022 (.005)	.025 (.005)	.023 (.005)	.026 (.005)
African American	.76** (.31)	.84** (.31)	.83** (.30)	.85** (.30)	1.78** (.42)	.74** (.31)
Hispanic	.60 (.40)	.56 (.39)	.57 (.39)	.54 (.38)	.46 (.38)	.68 (.39)
High School Graduation	—	-.91** (.25)	—	-.85** (.25)	-.90** (.25)	-.88** (.25)
Interaction Terms						
Official Intervention × African American	—	—	—	—	-1.75 (.48)	.65 (.55)
Official Intervention × Parental poverty	—	—	—	—	-.12 (.44)	.85* (.46)
Intercept	.02	.24	-.19	.04	-.47	.12
Deviance	33,831	32,963	33,078	32,313	32,071	31,973
d. f.	522	521	522	521	519	519

NOTES: $N = 529$. Unstandardized coefficients are reported. Modified standard errors are in parentheses. The standard errors from the Poisson regression are multiplied by a scale parameter, which is estimated by the square root of the deviance divided by the degrees of freedom.

* $p < .05$; ** $p < .01$ (one-tailed).

measure at ages 21–22 (there are only 16 cases in the nonzero category), we replace this measure with a measure of general crime in Models 1 through 4 for this age span.¹⁶

Models 1 and 3 in Table 6 show the effects of police intervention and juvenile justice intervention, respectively, on crime at ages 21–22. Police and juvenile justice intervention have significant, positive effects on early

16. When we did the analysis using the serious crime index at ages 21–22, we obtained findings similar to those reported in Table 6. However, coefficients for some of the control variables in our model were unstable due to an insufficient number of cases.

adult crime, further demonstrating the long-term impact of formal criminal intervention during adolescence on adult crime (Path f). Educational attainment is not statistically significant, however, even without controlling for nonemployment (results not shown). When both educational attainment and nonemployment are taken into account in Models 2 and 4, nonemployment but not educational attainment is significantly related to subsequent crime (Path e).

Table 6. Modified Poisson Regression of General Crime at Ages 21–22 (Wave 12)

Independent Variables	Model					
	1	2	3	4	5	6
Police Intervention	.51** (.19)	.32 (.21)	—	—	.07 (.35)	—
Juvenile Justice Intervention	—	—	1.01** (.22)	.91** (.22)	—	.02 (.50)
Serious Adolescent Delinquency	.11** (.04)	.10** (.04)	.06 (.05)	.04 (.05)	.10** (.04)	.05 (.05)
Parental Poverty	-.05 (.20)	-.14 (.20)	-.03 (.20)	-.13 (.10)	-.14 (.34)	-.11 (.24)
Math Score	.003 (.004)	.004 (.004)	.004 (.004)	.005 (.003)	.004 (.004)	.005 (.003)
African American	.44* (.25)	.34 (.25)	.53** (.24)	.40 (.24)	.19 (.30)	.18 (.26)
Hispanic	.32 (.31)	.23 (.31)	.35 (.31)	.28 (.31)	.26 (.32)	.35 (.31)
High School Graduation	—	-.20 (.21)	—	-.16 (.20)	-.21 (.21)	-.16 (.23)
Nonemployment (ages 19–21)	—	.81** (.32)	—	.82** (.31)	.79** (.27)	.80** (.27)
Interaction Terms						
Official Intervention × African American	—	—	—	—	.38 (.40)	1.20** (.52)
Official Intervention × Parental Poverty	—	—	—	—	-.02 (.42)	-.09 (.42)
Intercept	3.18	3.16	3.14	3.06	3.24	3.15
Deviance	117,080	114,933	114,475	112,126	114,728	110,781
d. f.	522	520	522	520	518	518

NOTES: $N = 529$. Unstandardized coefficients are reported. Modified standard errors are in parentheses. The standard errors from the Poisson regression are multiplied by a scale parameter, which is estimated by the square root of the deviance divided by the degrees of freedom.

* $p < .05$; ** $p < .01$ (one-tailed).

As predicted, periods of nonemployment significantly mediate the effect of police intervention on crime at ages 21–22 ($t = 2.50$), producing a 38%

drop in the effect of police intervention (Path ce; compare Models 1 and 2).¹⁷ Nonemployment produces a 10% drop in the effect of juvenile justice intervention (Models 3 and 4), but this mediation effect is not significant ($t = 1.60$).

Finally, product terms are added in Models 5 and 6. Only one product term is significant (see Model 6). As predicted, the results indicate that the positive effect of juvenile justice intervention on subsequent crime is stronger for African Americans than for whites and Hispanics (Path i). The effect of official intervention on crime at ages 21–22, however, is not significantly enhanced by impoverished family background.

In Table 7, we replicate this procedure for drug selling at ages 21–22. Again, police and juvenile justice interventions are significantly and positively associated with drug selling (Path f; Models 1 and 3, respectively). Adding educational attainment and periods of nonemployment to the equations in Models 2 and 4 reduces the effects of official intervention on adult crime, as expected. The intervening variables account for about 40% of the effect of police intervention and about 9% of the effect of juvenile justice intervention. Nonemployment has a significant and positive effect on subsequent involvement in drug sales (Path e) and significantly mediates the effects of police intervention on subsequent drug selling (Path ce; $t = 3.45$). Nonemployment, however, does not significantly mediate the effects of juvenile justice intervention on drug selling. Although educational attainment has a substantially significant effect on drug selling (not shown), nonemployment fully accounts for these effects. Finally, one of the product terms in Models 5 and 6 is statistically significant. Again, the effect of juvenile justice intervention on adult drug selling is stronger among African Americans.

To summarize our findings, we combine the results from equations predicting high school graduation and periods of nonemployment with those predicting early adult crime in path models (Figures 2 through 5). The figures provide an overview of the paths depicting direct and indirect effects. Overall, the overview shows that our results provide consistent support for the hypotheses presented in Figure 1. Official intervention in adolescence has positive indirect effects on adult crime through reduced educational attainment and nonemployment across alternative measures of intervention and adult crime. High school graduation is indirectly related to the measures of adult crime through nonemployment, whereas nonemployment is directly related to these outcome measures.

17. We replicated the analysis in Table 3 by regressing nonemployment at ages 19–21 (Waves 10 to 11). The coefficients and standard errors from this analysis were used along with the coefficients and the standard errors in Tables 6 and 7 in testing the significance of the mediating effects of nonemployment (Sobel, 1982).

Table 7. Modified Poisson Regression of Drug Selling at Ages 21-22 (Wave 12)

Independent Variables	Model					
	1	2	3	4	5	6
Police Intervention	.90** (.25)	.52** (.26)	—	—	1.26** (.63)	—
Juvenile Justice Intervention	—	—	1.62** (.25)	1.48** (.25)	—	.46 (.74)
Serious Adolescent Delinquency	.09* (.05)	.07 (.05)	-.01 (.01)	-.04 (.06)	.06 (.05)	-.02 (.06)
Parental Poverty	.13 (.23)	-.09 (.22)	.18 (.23)	-.09 (.22)	-.54 (.52)	.01 (.29)
Math Score	.005 (.004)	.008 (.004)	.008 (.004)	.009 (.004)	.008 (.004)	.009 (.004)
African American	3.20** (.96)	2.96** (.94)	3.29** (.94)	3.06** (.91)	3.55** (1.01)	2.79** (.92)
Hispanic	2.92** (.99)	2.70** (.97)	2.99** (.97)	2.87** (.94)	2.58** (.97)	2.91** (.94)
High School Graduation	—	-.20 (.27)	—	-.10 (.26)	-.24 (.27)	-.09 (.26)
Nonemployment (ages 19-21)	—	.17** (.04)	—	.18** (.03)	.17** (.03)	.18** (.03)
Interaction Terms						
Official intervention × African American	—	—	—	—	-1.09 (.66)	1.21* (.74)
Official Intervention × Parental Poverty	—	—	—	—	.63 (.58)	-.20 (.45)
Intercept	-.60	-.83	-.68	-1.04	-1.14	-.91
Deviance	79,974	75,447	76,694	71,621	74,816	71,161
d. f.	522	520	522	520	518	518

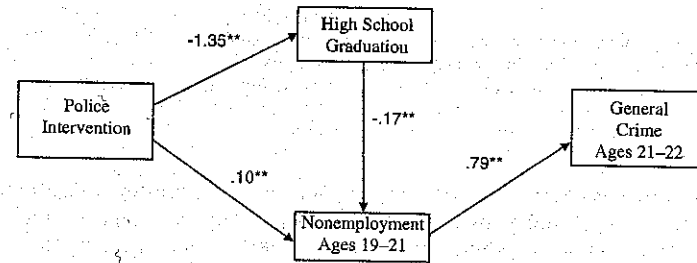
NOTES: *N* = 529. Unstandardized coefficients are reported. Modified standard errors are in parentheses. The standard errors from the Poisson regression are multiplied by a scale parameter, which is estimated by the square root of the deviance divided by the degrees of freedom.

* *p* < .05; ** *p* < .01 (one-tailed).

DISCUSSION

We examined whether official labeling increases the probability of involvement in subsequent crime and deviance by triggering processes that have negative consequences for conventional opportunities. We hypothesized that official intervention in adolescence increases involvement in crime in early adulthood due to the negative effect of intervention on educational attainment and employment. Using panel design data that spans early adolescence through early adulthood, our findings lend considerable

Figure 2. Effects of Police Intervention on General Crime (Unstandardized Coefficients)

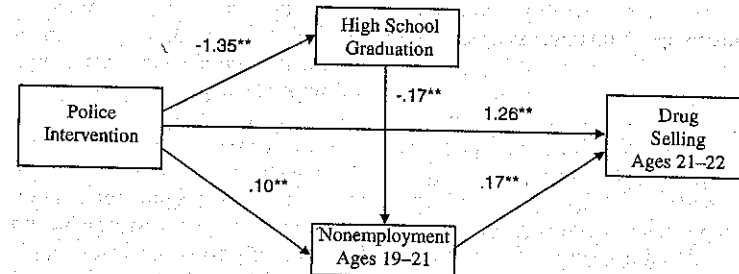


* *p* < .05; ** *p* < .01 (one-tailed).

support to the structural implications of the labeling approach (Becker, 1963; Link, 1982; Link et al., 1989; Sampson and Laub, 1997).

The present analysis goes further than most prior studies by temporally

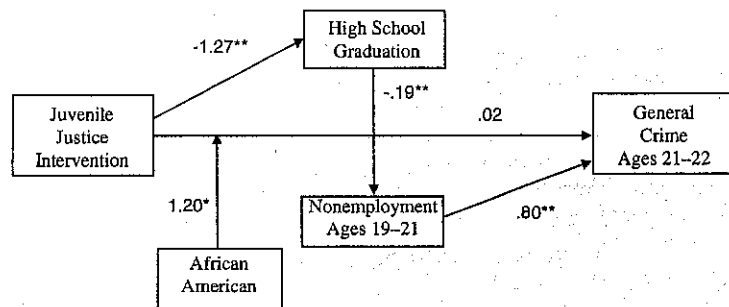
Figure 3. Effects of Police Intervention on Drug Selling (Unstandardized Coefficients)



* *p* < .05; ** *p* < .01 (one-tailed).

separating official intervention from subsequent life-course outcomes and criminal behavior. We find that official intervention affects educational attainment by decreasing the odds that those labeled will graduate from high school. In turn, educational attainment has a direct effect on employment and mediates the long-term effect of official intervention on adult crime. Further research is needed to determine the processes by which

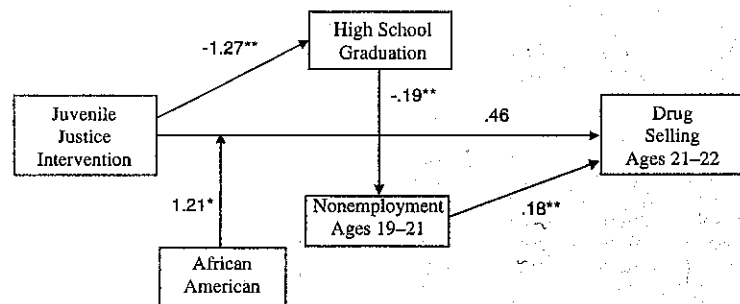
Figure 4. Effects of Juvenile Justice Intervention on General Crime (Unstandardized Coefficients)



* $p < .05$; ** $p < .01$ (one-tailed).

official labeling affects educational attainment. However, Bodwitch (1993) provides some evidence that may account for these effects. In an ethnographic study of high school students, Bodwitch found that students

Figure 5. Effects of Juvenile Justice Intervention on Drug Selling (Unstandardized Coefficients)



* $p < .05$; ** $p < .01$ (one-tailed).

who are defined as troublemakers are disciplined by school officials in ways that conspire to push students out of school. One way in which students acquire the troublemaker label may be involvement with juvenile

justice authorities.¹⁸ Although the current study cannot address the specific processes involved, results do confirm a link among official intervention, educational attainment and subsequent employment problems, and criminal involvement.

We have replicated results from other studies (Freeman, 1991; Schwartz and Skolnick, 1962; Western and Beckett, 1999) indicating that official intervention during adolescence negatively influences employment in young adulthood. Moreover, we go beyond prior research by showing that periods of nonemployment, along with educational attainment, partially mediate the effect of official intervention on adult involvement in crime. Our findings thus underscore the role of socioeconomic life-course outcomes in transforming official intervention into subsequent criminal involvement (Sampson and Laub, 1993, 1997).

Our results are consistent with the hypothesis that official intervention during adolescence influences criminal involvement as late as early adulthood when individuals become fully affected by blocked life chances shaped by education and employment. But, in addition to the indirect effects that both intervention by the police and the juvenile justice system have on adult crime, official intervention also directly influences subsequent criminality. The fact that official intervention has a direct effect on adult crime even after controlling for educational attainment, employment, and a number of other potential covariates, including adolescent delinquent behavior, may indicate the influential role that such an experience has on the lives of those so labeled, especially African-American males. It also suggests that we have not measured other intervening processes that may be at work. For example, labeling theorists have emphasized the importance of the development of a deviant identity (Lemert, 1967; Matsueda, 1992), blocked access to conventional others (Becker, 1963; Link et al., 1989; Sampson and Laub, 1997; Zhang and Messner, 1994), and association with unconventional others (Adams, 1996; Becker, 1963; Bernburg et al., 2002). Future research should examine the role of these processes in translating the impact of official labeling on subsequent deviance.

Some theorists have argued that disadvantaged structural location should facilitate labeling effects (Sampson and Laub, 1997). Other researchers suggest that labeling has more of an effect on people of

18. We have examined this possibility in a supplementary analysis (not shown). Subjects were asked if school officials were told about their involvement with the police/justice system. In an analysis deleting 27 males who had dropped out of school by age 15, we found that males who report school notification by age 15 are significantly less likely to graduate from high school, net of controls ($p < .01$, one-tailed test). See Bernburg (2002) for further analysis of this issue.

advantaged location because these people have higher stakes in maintaining a nondeviant status (Ageton and Elliott, 1974). To date, there has been limited research on this issue. Although our findings are not conclusive on this point, they lend more consistent support to the former approach. The effect of police and juvenile justice intervention on early adult crime is significantly stronger among males with impoverished backgrounds. Also, the effect of juvenile justice intervention on adult crime is significantly stronger among African Americans. It may be that a higher structural location provides people with the necessary resources and commitment to conventional pursuits to resist deviant labeling in the face of official intervention (Sampson and Laub, 1997). Moreover, deviant labeling may be more easily triggered when impoverished youths and African-American youths are processed by the police and the juvenile justice system, since negative stereotypes are already associated with these groups in the mainstream culture (see Gans, 1995).

This study contributes to our understanding of the impact of official intervention on the life course of individuals by addressing some of the weaknesses of past research efforts. Specifically, we examined the impact of labeling among a sample of males drawn from a general urban population. The longitudinal panel design enables us to temporally distinguish the effects of official intervention on adult crime and to examine the intervening processes of educational attainment and employment. We include the most commonly experienced official intervention, police intervention, which has frequently been overlooked (Paternoster and Iovanni, 1989), and we examine whether the effect of official intervention is contingent on social structural factors. One important limitation of the study is that it includes only males, a limitation that must be addressed in future research.

Overall, we have found that hypotheses from the labeling approach concerning the effect that official intervention has on important dimensions of life chances and subsequent criminal behavior are supported. The findings attest to the viability of the labeling approach for explaining secondary deviance and point to the processes that may account for the impact of official intervention on subsequent criminal behavior.

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THE EFFECT OF VICTIM IMPACT PANELS ON DUI REARREST RATES: A FIVE-YEAR FOLLOW-UP*

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Victim Impact Panels (VIPs) were introduced by Mothers Against Drunk Driving (MADD) in 1982 and have since spread throughout the United States in an attempt to reduce drunk driving. The objective of a VIP is to expose DUI offenders to the pain and suffering caused by drunk driving without necessarily condemning the DUI offender. The few scientific evaluations of the effectiveness of VIPs have produced mixed results. The present investigation draws on evidence from a quasi-experimental design and a five-year follow-up to probe further the effects of VIPs on DUI recidivism. Results show that 33.5% of the comparison group, but only 15.8% of the VIP group, were rearrested over the five-year period. Discrete-time event history analyses suggest that VIPs are associated with a 55.7% overall decrease in the hazard of rearrest; the VIP effect is strong in the first two years but then wanes dramatically. Methodological threats stemming from the study's design are considered. The implications of the differing styles of VIP and the resultant outcomes are also discussed.

KEYWORDS: DUI, Victim Impact Panels, recidivism, hazard rates

Drunk driving has remained a serious social problem in American society for decades. Although there has been a 25% reduction in alcohol-related crashes from 1990 to 2000 (National Highway Traffic Safety Administration, 2001), there were still 16,653 alcohol-related fatalities in the United States in 2000. The National Highway Traffic Safety Administration estimates that alcohol was involved in 40% of all fatal crashes, 10% of all injury crashes, and 6% of all property-damage-only crashes in 2000. Statistics suggest that 30% of Americans will be involved in an alcohol-

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related crash at some time in their lives (National Highway Traffic Safety Administration, 2001). The economic cost of these alcohol-related crashes was estimated to be \$45 billion for 1999 alone (Polacsek et al., 2001). Further, nearly 1.5 million persons were arrested in 2000 for driving under the influence, a figure that represents 11% of all arrests made in 2000 (Uniform Crime Reports, 2001).

Various policies and punishments have been employed to reduce the problem of drunk driving. For example, although all states currently have a minimum drinking age of 21, the evidence is weak that this policy has had any impact on drunk driving (Applegate and Cullen, 1995). Nationwide efforts to increase the certainty, severity, or celerity of punishment for drunk driving have had a rather modest deterrent effect (Donovan, 1989; Piquero, 1998; Ross, 1993; Ross and Voas, 1989; Shore and Ferrari, 1998). Some studies suggest that the national standard of 0.08% blood alcohol concentration (BAC) may have prompted a reduction in drunk driving (Wagenaar and Toomey, 2002); however, the National Highway Traffic Safety Administration suggests that changes of that sort need to be examined in the context of a drop in alcohol-related fatalities in the general population (National Highway Traffic Safety Administration, 2001). Several studies find that education programs (DUI or DWI schools) that purportedly increase knowledge and change attitudes toward drunk driving have no effect on drunk driving (Mann et al., 1986; Sheppard and Stoveken, 1993). In addition, the relative effectiveness of various formal sanctions for DUI offenders is far from impressive. Nichols and Ross (1989) report that 75% of DUI offenders continue to drive while having a suspended license. Simpson et al. (1996) reported that during a 7- to 10-year period, more than 30% of DUI offenders were repeat offenders.

In an attempt to address the problem of drunk driving, Mothers Against Drunk Driving (MADD) introduced in 1982 the concept of a Victim Impact Panel (VIP). The objective of a VIP is to provide a forum for DUI victims to share their personal trauma with convicted drunk drivers who are sentenced by a judge to participate in this program. Although there is variation in the content, length, and format of VIPs, a typical pattern is that four or five DUI victims relate how their lives were impacted by a DUI offender. Each presentation is about 10 to 15 minutes long. Although there is no structured or planned interaction between victims and offenders during the VIP presentations, a question and answer period may follow the presentations on some occasions. What is unique about VIPs is that DUI victims describe how a drunk driving incident impacted their life. Rather than condemning DUI offenders, VIPs focus on firsthand testimony from victims about the trauma and tragedy of drunk driving.

This study contributes to the emerging literature on the role that VIPs

might play in reducing DUI recidivism. The balance of the article unfolds in four sections. The next section considers theoretical rationales for VIP programs and what is known from the small number of evaluation studies. The second section describes the quasi-experimental research design and the structure and content of the VIPs that constitute the key treatment effect. A third section contains a description of the statistical methods and a discussion of the results. The fourth and final section places the findings in the context of previous evaluation studies and offers suggestions for additional research.

VICTIM IMPACT PANELS: THEORETICAL RATIONALES AND EVALUATIONS

Theoretical rationales for the VIP approach can be found in the restorative justice movement that emerged in the 1990s (Van Ness and Strong, 1997; Zehr, 1990) and in notions of inclusionary modes of social control and community shaming (Braithwaite, 1989; Cohen, 1985). One of the aims of restorative justice is to replace forms of "state justice" concerned solely with the state and the offender with an approach that includes as stakeholders the victim and the community (Ashworth, 2002). Although there is no common definition to restorative justice, it is critical of the centralization of criminal justice and the exclusion of the victim and the community. Conventional definitions of justice see the state as the offended party, and representatives of the state decide how deviance should be addressed. Restorative justice emphasizes offending in terms of its consequences for victims, offenders, and the community. Further, there is the issue of how to reintegrate victims and offenders in the local community by attempting to heal the harm caused by the offense and to take steps necessary to prevent its recurrence (Morris, 2002).

A related rationale for the VIP approach draws on ideas offered by Cohen (1985) and Braithwaite (1989). The problem of standard sanctions of DUI offenders is related to what Cohen (1985) discussed as exclusionary versus inclusionary modes of social control. The state employs formal judicial machinery that relies on condemnation and punishment for individual acts of deviance but with little or no role for the local community. Inclusionary social control entails what Braithwaite (1989) described as community shaming that is more effective than shaming by an impersonal state. In the case of DUI offenses, shaming induced by DUI victims may well be more effective in maintaining social control because of the personal nature of the message. On the other hand, formal court proceedings in DUI cases can engender alienation, hostility, and anger, with little or no element of moral education. Wilson and Herrnstein (1985:495) argue that "punishment as moral education almost certainly reduces more crime than