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Labeling and Cumulative Disadvantage: The Impact of Formal Police Intervention on Life Chances and Crime During Emerging Adulthood

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Abstract

Research in labeling theory has been revived recently, particularly in relation to the effect of labeling on critical noncriminal outcomes that potentially exacerbate involvement in crime. This study partakes in that revitalization by examining direct and indirect effects of police intervention in the lives of adolescents who were followed into their 30s. The authors find that early police intervention is indirectly related to drug use at the ages of 29 to 31, as well as unemployment and welfare receipt. Given that such effects were found some 15 years after the labeling event, on criminal and noncriminal

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outcomes, and after controlling for intraindividual factors, the authors conclude that the labeling perspective is still relevant within a developmental framework.

Keywords

labeling theory, life course, police intervention, cumulative disadvantage, transition to adulthood

Recent research on the enduring effects of early formal police intervention throughout the life course suggests that it affects not only subsequent criminal behavior but also critical noncriminal outcomes—particularly educational attainment, employment opportunities, and financial well-being (Bernburg & Krohn, 2003; Sampson & Laub, 1993, 1997). Despite the relevance of such findings, few prospective studies have examined noncriminal and criminal outcomes in relation to criminal labeling. Those studies that have done so are typically confined to a restricted temporal range that leaves out precisely the age span at which one should be expected to have completed their education, have established a stable pattern of employment, and have become financially independent. This is particularly important given that the average age at which one is expected to be established has been seen as increasing to the late 20s and early 30s (Arnett, 2004; Furstenberg, Kennedy, McCloyd, Rumbaut, & Settersten, 2003). Furthermore, these few studies largely fail to account for the role played by antisocial proclivities. Here, we examine how recent developments in labeling theory have focused on the importance of these noncriminal outcomes and review the limited research that has addressed these issues. We then present and examine the viability of a theoretically derived model of the impact of police intervention with longitudinal data following participants from early adolescence to the age of 30 or so. Specifically, we examine whether police intervention during adolescence serves as a "turning point" (Sampson & Laub, 1993) and affects educational, employment, and financial concerns at the age by which individuals should have established themselves.

Labeling Theory: Redux and Recast

Labeling theory has experienced its share of popularity, as well as disenchantment, among criminologists and deviance scholars. Although enjoying its intellectual peak in the 1960s, by the 1980s the perspective's demise was

consolidated, with most quantitative assessments of the theory producing little evidence in support of its main tenets (Gove, 1980; Hirschi, 1980; Tittle, 1980). In part, this may have been due to a narrow focus on the self-concept approach to labeling, and in part, it may have resulted from poor research design of most studies conducted at the time (Paternoster & Iovanni, 1989). In recent years, however, interest in the perspective has resurged. This revival can be largely attributed to empirical and theoretical contributions made during the late 1980s and the early 1990s.

The development and refinement of labeling theory has taken different conceptual avenues. Among the emerging trends contributing to this revival were Braithwaite's (1989) theory of reintegrative shaming and Sherman's (1993) defiance theory. These two theoretical approaches and attendant research provided a novel conceptual link between labeling and emotion—shame and pride—thereby offering an extension of the perspective (Bouffard & Piquero, 2010). In addition, since the 1990s, Matsueda and colleagues (e.g., Heimer & Matsueda, 1994; Matsueda, 1992) contributed to the renewed interest primarily by developing a significant research program that assesses adolescents' conceptions of self and their behavior as a reflection of the appraisals by others (e.g., peers, teachers, and parents).

The other avenue that labeling's revitalization has taken coincides with the increasing influence of a developmental perspective on behavior. Such an epistemic shift has challenged crime theories in general to consider the dynamics through which different causal factors operate over the life course. Labeling theory's attention to processes over time, rooted in its symbolic-interactionist underpinnings, highlights the perspective's natural developmental inclination (see Loeber & LeBlanc, 1990; Sampson & Laub, 1997). Along these lines, Sampson and Laub (1997) seized on labeling theory's affinity to a life-course framework and redefined the theory as to incorporate the role of social-structural consequences of early official sanctioning on later adult outcomes.

Sampson and Laub's (1997) developmental reconceptualization addressed the concerns raised by those who, while denouncing the perspective's premature dismissal, had called for a theoretical clarification of the intervening mechanisms taking place between the attachment of a label and subsequent deviance (Paternoster & Iovanni, 1989; see also Wellford, 1975; Wellford & Triplett, 1993). Specifically, these proponents of a "neolabeling" approach had argued for a general synthesis of labeling and social control frameworks based on the notion that a deviant label is one important factor that "weakens one's social bond to conventional society, thereby freeing actors to deviate" (Paternoster & Iovanni, 1989, p. 383).

To be sure, Sampson and Laub's (1997) work extended to criminology many of the vital theoretical clarifications made by Link and other scholars in the mental health field, whose "modified labeling theory" elucidated and systematically tested such intervening causal mechanisms through which former mental patients experience social adjustment (Link, 1982, 1987; Link, Cullen, Frank, & Wozniak, 1987; Link, Cullen, Struening, Shrout, & Dohrenwend, 1989). Link and colleagues' (1989) modified version of the theory addressed the lack of prior emphasis on the effects of labeling on critical arenas of one's life, primarily their education, work, and ability to earn income.

In teasing out a theoretical framework that integrated such issues of social control and stakes in conformity over time, as called for by the "neolabelists" and initiated by "modified labeling" proponents, Sampson and Laub (1997) drew attention to the notion of "cumulative disadvantage" (Merton, 1968, 1988). For the authors, cumulative disadvantage is most explicitly generated by the negative social-structural consequences of official labeling in future conventional opportunities. Hence, a formal deviant label could be a "turning point" in the lives of individuals and be indirectly related to subsequent involvement in delinquency and criminal behavior later in life through its detrimental impact on life chances, especially those molded by education and employment. In this sense, Sampson and Laub recast, in a structural and age-graded framework, one of the labeling's original contentions—that is, official reactions to *primary deviance* (e.g., early formal police intervention) may affect social adjustment (e.g., employment stability) and foster a criminal career through *secondary deviance* (e.g., later arrest; Becker, 1963; Lemert, 1951).

The limits on future opportunities brought about by a criminal label cause deficits and disadvantages that compound to create negative consequences for later development. These compounding effects result from the severance of social bonds, such as attachment to prosocial activities (e.g., school and labor force) and censoring from conventional others, as well as changes in self-concept. Although much of the work on this theory has focused on self-concept (Adams & Evans, 1996; Ageton & Elliott, 1974; Bartusch & Matsueda, 1996; Heimer & Matsueda, 1994; Hepburn, 1977; Horowitz & Wasserman, 1979; Jensen, 1980; Triplett & Jarjoura, 1994), labeling research had largely failed to present an elaborate causal process of effects that specified more clearly the mechanisms intervening between the ascription of a label and subsequent behavioral adjustment. Works by Link et al. (1987, 1989), Sampson and Laub (1993, 1997), and Caspi and Moffitt (1993) are among the few studies that helped shed light on how labeling effects may carry out such detrimental consequences, effectively through the "knifing off" of one's opportunities for a conventional life.

Prior Research

Of particular concern to the current study is the relatively limited scholarly attention given to intervening mechanisms other than measures of self-concept—specifically the detrimental impact of formal labeling through life chance processes such as education, employment, and financial independence and stability. In the following sections, we turn our attention to the few existent contributions regarding the relationship between labeling and these three crucial life arenas.

Education. Labeling theory postulates that specific processes, such as exclusion from conventional opportunities, once triggered by a labeling event, can mediate further entrenchment in deviant behavior. In this sense, knowledge that an adolescent has been in trouble with the law may have a stigmatizing impact that is experienced initially in school, a social environment where youth spend a substantial amount of time. Besides self-concept issues, the "troublemaker" label might shape further reactions of school administrators and teachers. Thus, the perception of youth as such by school officials is an important process that often leads to harsher disciplinary measures in school—such as suspensions, transfers, and involuntary "drops" to "get rid of" such students (Bowditch, 1993, p. 493). Bernburg (2002) has suggested that formal labeling could trigger the ascription of a "troublemaker" label by school officials. Research demonstrates that school notification of formal labeling events such as arrest is significantly associated with the odds of male youth dropping out in a subsequent period (Bernburg, 2002). Consistent with these observations, labeling decreases the likelihood of success in the educational arena (Bernburg, 2002; Bernburg & Krohn, 2003; Hjalmarsson, 2008; Menard & Morse, 1984; Sweeten, 2006). Few studies, however, have focused on whether the impact of police intervention in the educational arena mediates the relationship between police intervention and delinquent or criminal behavior (for exceptions, see Bernburg & Krohn, 2003; Sampson & Laub, 1993).

Employment. Formal police intervention also may affect the likelihood of finding and/or maintaining stable employment. For example, employers can be resistant to hiring those with a preexistent criminal label (Irwin, 2005; Kurlychek, Brame, & Bushway, 2007; Schwartz & Skolnick, 1962). In an early study on the impact of incarceration on employment opportunities, Schwartz and Skolnick (1962) found that potential employers were less likely to hire applicants who were described as having been convicted or incarcerated. These findings have been replicated in similar studies (Boshier & Johnson, 1974; Buikhisen & Dijksterhuis, 1971; Pager, 2003). Research

inquiring about the actual job histories of individuals who have experienced official intervention also finds that they are more likely to have had difficulty securing stable employment (Bernburg & Krohn, 2003; Davies & Tanner, 2003; Freeman 1991; Western & Beckett, 1999; see also Lanctôt, Cernkovich, & Giordano, 2007). Finally, in a recent study about the relationship between employers' attitudes toward hiring ex-offenders and their actual hiring behavior, Pager and Quillian (2005) found that even though employers indicate a high level of willingness to hire ex-drug offenders, they are less than half as likely to even call back such applicants compared with applicants without criminal records (see also, Uggen, Vuolo, Ruhland, Whitham, & Lageson, 2012).

Financial stability/independence. Delinquent behavior and formal ascription of a deviant label may also affect (directly and indirectly) economic self-sustainability in the long run. The "knifing off" of educational and employment opportunities that would lead one to attain financial stability and independence is especially problematic as this domain is a key criterion in the transition to adulthood (Settersten, Furstenberg, & Rumbaut, 2005) and an effective "turning point" especially for offenders in their late 20s (Uggen, 2000). Sampson and Laub (1993) found that delinquency in childhood is related to economic dependency, namely, welfare reliance, for young adults (17- through 25-year-olds) and in later adulthood (25- through 32-year-olds). Enduring reliance on state welfare is also increased by previous police intervention (Bernburg, 2002) as well as by chronic lack of job security (Paugam, 1996).

The research on the impact of labeling on education, employment, and economic status certainly is suggestive that problematic outcomes in areas that are central to one's life chances are adversely affected by that experience. With the exception of Sampson and Laub's (1993) retrospective study, examinations of these processes have focused on a limited age range. Much of the research has been limited to what happens during the teenage or very early adult years to youth who have contact with the juvenile justice system.

Bernburg and Krohn (2003) presented one of the few prospective examinations of the impact of police intervention on the social-structural intervening variables and behavioral outcomes. They examined the impact of early labeling (ages 14-16) on outcomes when respondents were in their very early 20s. They found that early police intervention was related to educational attainment, unemployment, and crime and drug use. Although the Bernburg and Krohn study suggested that the impact of formal labeling on educational and employment outcomes may be important in assessing the life chances of those who are labeled, the implications of these findings are limited in two ways.

First, Bernburg and Krohn (2003) failed to incorporate measures of criminal proclivities that may have rendered their results regarding the impact of official labeling spurious. From a developmental perspective, however, these early antisocial traits persist only to the extent to which their attendant behavior attenuate the social and institutional bonds that link these individuals to society. As Sampson and Laub (1993) argued, "informal social bonds in adulthood to . . . employment explain changes in criminality over the life span, despite early childhood propensities" (p. 7). According to latent trait theorists (Gottfredson & Hirschi, 1990), variations in self-control (that emerge early and remain fairly stable throughout the life course) and other antisocial propensities offer the primary causal explanation for individual differences in involvement in crime and delinquency throughout the life course. Although Bernburg and Krohn incorporated a measure of early participation in delinquent behavior, their examination of these alternative explanations was somewhat restricted.

Second, and more importantly, Bernburg and Krohn (2003) assessed the impact of official labeling only through ages 20 to 22. People in their early 20s (20-22) typically are not expected to have completed their education and obtained financial independence (via stable career-oriented employment and economic self-reliance). Even though Bernburg and Krohn's finding that early intervention is related to criminal involvement indirectly through education and employment is certainly suggestive of an enduring, longer term problem, such an effect could not be demonstrated in that study. To assess these potential effects appropriately, a time span that includes ages at which educational attainment is likely to have been completed and career paths begun needs to be covered. Furthermore, this longer time span offers us a more stringent test that allows us to assess whether the label effects are transitory or indeed pervasive.

Transitions to Adulthood and Criminal Labeling

Sampson and Laub (1997) suggested that it is important to focus on "turning points" in the lives of individuals as these transitions can alter their trajectories in a number of arenas. Although the focus of their research is largely on those turning points that deflect one toward conformity (e.g. establishing a good marriage, acquiring a career-oriented job, or joining the military), Sampson and Laub also suggest that being labeled can in fact deflect a person toward furthering their criminal career. As suggested previously, this may be due to the knifing off of conventional social networks and conventional opportunities, thus limiting the social capital that they can call upon to

successfully traverse the difficulties of obtaining an education, acquiring a job (career), and succeeding financially. Although studies like Bernburg and Krohn (2003) and Bernburg, Krohn, and Rivera (2006) are instructive of the potential effect of labeling on social networks and life chances, they cannot determine whether official labeling at young ages has the type of enduring impact on life chances predicted by labeling theorists because they follow individuals only through the age 20 to 22. Work by Furstenberg et al. (2003) and especially Arnett (2001, 2004) has suggested that in today's society, these ages constitute a stage in the life cycle when people are no longer adolescents, yet they have not established themselves in the arenas such as education and employment that define what it means to be an adult.

Historically, the age at which we complete our formal education and establish career paths that will have a higher probability of defining what our ultimate life course will look like has increased (Furstenberg et al., 2003). In the very early 20s, individuals have either just completed their formal education (or not), or are still pursuing further educational goals. Jobs at these ages are likely to be part-time or a temporary necessity to make ends meet; these jobs tend not to be those that ultimately define a career path.

Furstenberg et al. (2003) and Arnett (2004) suggested that adulthood is probably not achieved until at least the age of 25 and quite possibly not until about the age of 30 in American society. By then, not only will most people have completed whatever education they will acquire but many will also have started along whatever their career path will be. Arnett (2001) coined the phrase "emerging adulthood" to distinguish this phase from other periods within the much broader concept of adulthood. In part, the concept is intended to convey the notion that it is not until these ages that the path that one's life chances will take effectively emerges. Arnett calls attention to the age when options tend to close off and lifelong commitments must be made—"the age 30 deadline" (Henig, 2010). Therefore, to assess adequately the impact of police intervention on life chances, it is necessary to focus on how labeling has affected educational attainment and employment until the end of such "emerging adulthood."

Current Study

The breadth of the data utilized in the current study, coupled with its richness and relatively long and interspersed time span, allows us to address a number of the above-mentioned limitations. For one, our sample was followed from age 14 to 31. The inclusion of these later adulthood years is crucial because by then most individuals have completed their education and are on track to

establishing stable, career-oriented employment. Thus, we are able to assess whether the effect of *police intervention* is still apparent at this critical period in the life course, when trajectories are established in these two important arenas.[1] In addition, the data used to examine the impact of labeling come from a longitudinal panel study of adolescents who were enrolled in a public high school. Because the sample is not limited to those who have had contact with law, the absolute effects, and not just the relative effects, of formal police intervention are examined. Finally, thorough reviews of labeling research have indicated that there is limited research on mediational processes and the long-term effects of labeling (Paternoster & Iovanni, 1989; Sampson & Laub, 1997). The longitudinal nature of the data set and its wide temporal measurement allow for a more rigorous analysis of the processes that potentially mediate the relationship between formal police intervention and problematic behavior. The hypothesized effects are shown in Figure 1.

As a longitudinal test of structural labeling theory, our study assumes that the stigmatizing effect of a deviant label furthers social marginalization and thereby amplifies and solidifies delinquent and criminal behavior over time. Overall, our models and data enable us to ascertain the direct and indirect cumulative effects of structural factors related to delinquency and criminal behavior at different points in the life course. Specifically, we hypothesize the following:

Hypothesis 1: Police intervention in adolescence decreases the likelihood that youth will graduate from high school and secure stable employment in early adulthood (early 20s), whereas it increases the likelihood of arrest during this time period. It is important to note that we anticipate that police intervention will have this effect even after controlling for not only prior delinquency and drug use but also other measures of antisocial proclivities, such as academic aptitude, aggressiveness, and low self-control.

Hypothesis 2: The effects of police intervention in adolescence on the likelihood of employment and welfare receipt in late adulthood (age 29) are predicted to be direct and indirect, that is, through education, unemployment, arrest, and crime and drug use in early adulthood (approximately ages 21-23).

Hypothesis 3: Finally, we do hypothesize a relationship between economic unsustainability and criminogenic behavior. Thus, employment and welfare status in late adulthood (age 29) is expected to increase the probability of continued crime and drug use (at ages 29-31). Unfortunately, our measures of unemployment, welfare

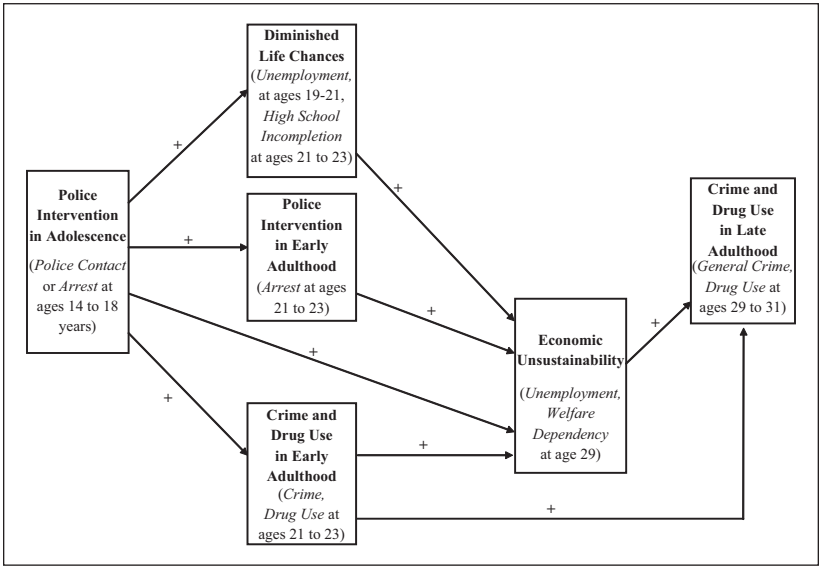

Figure 1. Hypothesized effects of police intervention from adolescence (ages 14-18) to emerging adulthood (ages 29-31)

status, and criminogenic behavior in later adulthood are contemporaneous and as such they do not allow us to optimally ascertain a causal effect. For this reason, we consider the analysis of these effects to be, at best, suggestive.

We estimate a system of nine equations that trace possible direct and indirect effects of labeling on early and later adult crime and noncrime outcomes. This is examined using prospective longitudinal panel data, which followed participants from age 14 (on average) to their early 30s.

Method

Data and Sample

The analysis presented here uses data from the Rochester Youth Development Study (RYDS), a multiwave panel study of the development of delinquency and drug use among youth and adults that has followed a sample of 1,000 adolescents over 14 intervals, or waves, of data collection from ages 14 to

31. Phase 1 of RYDS covered the adolescent years of participants (called G2), who were of ages 14 to 18 on average. In Phase 1, we interviewed G2 for 9 waves (1-9) and their parents (called G1) for 8 waves (1-8) at 6-month intervals. In Phase 2, after a 2½-year gap in data collection, we interviewed G1 and G2 for 3 annual waves (10-12), when G2 participants were of ages 21 to 23 on average. In Phase 3, we interviewed G2 for 2 additional waves (13-14), when they were of ages 29 and 31, on average. We also collected official data from the police, schools, and the Department of Social Services. The current analysis uses all 14 waves of data collection, covering roughly 16 years in the lives of these individuals.

The initial panel sample was selected in 1988 from the population of seventh and eighth graders in the Rochester, New York, public schools. To obtain a sufficient number of serious, chronic delinquents, a stratified sample was selected so that the entire school population was represented in the sample, but high-risk youth were overrepresented. Overrepresentation was accomplished by stratifying on two dimensions. First, males were oversampled (75% vs. 25%) as they are more likely to engage in chronic delinquency (Blumstein, Cohen, Roth, & Visher, 1986). Second, students were sampled proportionately to the rate of criminal offenders residing in their neighborhoods, while the highest one third of resident arrest rate areas was sampled with certainty. This assumes that adolescents living in areas of the city with high residential arrest rates are at greater risk for delinquency than those living in low resident arrest rate areas. The panel is 68% African American, 17% Hispanic, and 15% White. These proportions are quite close to what was expected, given the population characteristics of the Rochester schools and the decision to oversample high-risk youth.

Of the base-panel participants, 917 had sufficient data to contribute to the multiple imputation model that was estimated to handle missing data; this is a retention rate of 92% of the original sample. These 917 cases have complete data on demographic variables and police records of arrests and contacts across Waves 1 through 12 (ages 14-23), and the overall rate of missing data for variables in the analysis is approximately 8%. Although retention and item response rates are quite high, case deletion that is commonly used for handling missing data results in a loss of many cases from the multivariate analyses and leads to biased and inefficient estimates (Schafer, 1997). Multiple imputation is a Bayesian method that generates pseudorandomly drawn data sets from a probability distribution (Schafer, 1997). This process is done multiple times and then estimates are averaged across the imputed data sets to obtain valid inference (Rubin, 1987). For more detailed discussions of the multiple imputation model, see Rubin (1987) and Schafer (1997).

The multiple imputation model was built using SAS Proc MI following a procedure similar to that outlined in Gullion, Chen, and Meltesen (2008). The validity of the multiple imputation model was assessed by examining time-plots and autocorrelation plots (see Allison, 2001, and Yuan, 2000), assessing the posterior parameter estimates degrees of freedom (see Gullion et al., 2008), and examining pre- and postimputation descriptive statistics for extreme deviations (see University of California–Los Angeles Academic Technology Services, 2010). With respect to the assumptions underlying the imputation model (normality, data missing at random [MAR], and distinctness of parameters; see Schafer, 1997, for an in-depth discussion), there is no evidence that these assumptions are not upheld. SAS Proc MI is robust to departures of normality, and although it is not possible to test whether the data are MAR (Allison, 2001), even if they are not MAR, multiple imputation is still a better method of addressing missing data than other methods that introduce more bias (Schafer, 1997). In summary, we have confidence that the multiple imputation model performs well and will provide valid inference. Once we confirmed that the imputation model was sound, we estimated 20 imputations. All analyses presented are averaged effects across these 20 imputed data sets.

Measures

The primary question that this research addresses concerns the impact of official labeling on a variety of subsequent crime-related and noncrime outcomes. Table 1 lists each measure, how they are coded, and descriptive statistics. Two measures of official labeling are used in the analysis. From police data, we create a dummy variable labeled *Arrest or Contact 1-9* that indicates whether the youth has a record of arrest or official police contact in the adolescent years spanning Waves 1 through 9 (ages 14-18 on average). In Rochester, New York, only nontrivial police contacts with juveniles are officially recorded. This measure has been shown to be predictive of problematic outcomes in previous studies (Bernburg & Krohn, 2003; Bernburg et al., 2006). Forty-two percent of adolescents have had an arrest, police contact, or both. *Arrest 10-12* indicates whether they have been arrested during the time period spanning Waves 10 through 12, when they were approximately 21 to 23 years of age. Thirty-four percent have been arrested as young adults.

Our model predicts that the formal police intervention will affect long-term outcomes through its effect on education, employment, and welfare status. *Education* is a dummy variable indicating whether respondents obtained a high school diploma (0) or did not (1) by the age of 20 (New York State

Table 1. Coding and Descriptive Statistics ($N = 917$)

Variable	Coding	*M*	*SE*
Arrest or Contact 1-9 (ages 14-18)	Arrest or police contact in Waves 1 through 9, 0 = *no*, 1 = *yes*	0.42	0.02
Arrest 10-12 (ages 21-23)	Arrest in Waves 10 through 12, 0 = *no*, 1 = *yes*	0.34	0.02
Education	Less than high school level of education, 0 = *no*, 1 = *yes*	0.43	0.02
Unemployment 10-12 (ages 19-21[a])	Percentage of the time unemployed from age 19 through 21	0.26	0.01
Unemployment 13 (age 29)	Unemployed at Wave 13, 0 = *no*, 1 = *yes*	0.12	0.01
Welfare 13 (age 29)	Welfare receipt in the year prior to Wave 13, 0 = *no*, 1 = *yes*	0.35	0.02
General Crime Variety Score 13-14 (ages 29-31)	Number of different types of general crime respondent committed in Waves 13 through 14	1.54	0.09
Drug Use 13-14 (ages 29-31)	Incidence of drug use in Waves 13 through 14	72.68	21.84
General Delinquency 2-9 (ages 14.5-18)	Incidence of general delinquency in Waves 2 through 9	79.41	6.80
General Crime 10-12	Incidence of general crime in Waves 10 through 12	171.64	50.48
Drug Use 2-9	Incidence of drug use in Waves 2 through 9	28.54	3.05
Drug Use 10-12	Incidence of drug use in Waves 10 through 12	118.53	15.83
Male	Male sex, 0 = *no*, 1 = *yes*	0.73	0.01
Female	Female sex, 0 = *no*, 1 = *yes*	0.27	0.01
African American	African American race, 0 = *no*, 1 = *yes*	0.68	0.02
Hispanic	Hispanic ethnicity, 0 = *no*, 1 = *yes*	0.17	0.01
White	White race, 0 = *No*, 1 = *Yes*	0.15	0.01
Poverty	Poverty level income at Wave 2, 0 = *no*, 1 = *yes*	0.32	0.02
Academic Aptitude	Math percentile score on CAT math exam	49.53	0.82
Aggression	Twelve-item Achenbach Scale ranging from 0 to 2, higher scores equal more aggression; measured at Wave 3	0.46	0.01
Low Self-Control	Twelve-item scale ranging from 1 to 4, higher scores indicate lower self-control; measured at Wave 10	2.22	0.02

[a]Although the unemployment measure at young adulthood was collected during interviews conducted at Waves 10 to 12 when participants were on average 21 to 23 years old, this measure corresponds, retroactively, to employment reports by participants for ages 19 to 21.

does not allow a person to attend high school after the age of 20). Forty-three percent of the sample has no high school diploma. We include two measures of employment. For Waves 10 through 12 (when participants were approximately 21-23), we collected life history data allowing us to compute *Unemployment 10-12*, a measure of the proportion of months during which the participants reported being unemployed in the previous 2 years. On average, participants were unemployed 26% of the time. Life history data were not available for Wave 13 (age 29). However, we did ask respondents whether they were currently employed or not. From this information, we created a dummy variable, *Unemployment 13*, with "1" indicating that they were not employed at the time of data collection and "0" indicating that they were employed. Twelve percent of the sample was unemployed at Wave 13 (age 29). *Welfare Status* is measured only at Wave 13 (age 29), when the sample should have established their financial independence. The variable is coded "1" if they received any form of welfare and "0" if they did not. Thirty-five percent of the sample received welfare in the year prior to Wave 13 interview.

The outcome variables of interest are general crime and drug use. For these crime-related measures, we combined Waves 13 and 14 (ages 29-31) to provide sufficient variation. The *General Crime Variety Score 13-14* measure is generated from self-report responses to questions asking whether respondents had committed any of the 26 delinquent or criminal acts at Waves 13 and 14. The variety score of these criminal acts simply counts the number of different offenses the respondent reported. The mean for this variable is 1.5. *Drug Use 13-14* is an incidence measure of the number of times respondents had used any of 11 drugs at Waves 13 and 14 (on average 73 times). We used the log of these measures in the analyses because of the skewed nature of the data.[2]

We control for earlier delinquency (*General Delinquency 2-9*, ages 14-18, and *General Crime 10-12*, ages 21-23) and drug use (*Drug Use 2-9*, ages 14-18, and *Drug Use 10-12*, ages 21-23) in adolescence and early adulthood to determine whether the intervention had an impact on the change in the incidence of those behaviors. When general crime and drug use incidence at Waves 10 through 12 (ages 21-23) are used as outcomes, we log these measures because of their skewness. We also control for sex, race/ethnicity, and poverty. Seventy-three percent of the sample is male. Two dummy variables, *African American* (68% of sample) and *Hispanic* (17% of sample), are created, with Whites (15% of sample) being the reference category. *Poverty* is measured by a dummy variable with "1" being equal to the household having an income below the poverty line and "0" equal to an income above the

poverty line. This information was provided by the parent or guardian of the youth, in the early stages of data collection (at Wave 2). Thirty-two percent of the sample reported a poverty level income.

In addition, we include three measures of antisocial proclivities that might render any relationship between police intervention and subsequent outcomes spurious. *Academic Aptitude* is the math percentile score received on the CAT Math Exam at Wave 1 (age 14 on average). The sample received an average percentile score of 49.53. *Aggression* is measured using the Achenbach Aggression subscale that was administered to the parent or guardian at Wave 3 (age 15). It is an average of 12 items indicating aggressive tendencies. Scores ranged from 0 to 2, with an average of 0.46. We also include a 12-item scale measuring *self-control*. Unfortunately, this scale was not included in the survey instrument until Wave 10, when respondents were approximately 20 years of age. We would have preferred having a measure when respondents were younger, but if Gottfredson and Hirschi (1990) were correct in assuming that self-control is stable as one ages, then the wave at which we measure the concept should not make a difference. If they are wrong and the measure is affected by life events such as official labeling, then we have a more stringent test of the labeling hypothesis. The measure is a 12-item scale incorporating items from the larger scale originally constructed by Grasmick, Tittle, Bursik, and Arneklev (1993). It ranges from 1 to 4 with higher scores indicating lower self-control. The average self-control score was 2.22.[3]

Results

The zero-order correlations among the independent and control variables and the outcomes at both Waves 10 through 12 (approximately ages 21-23) and 13 through 14 (ages 29-31) are shown in the appendix. It is important to note that early police intervention is significantly related in the expected directions to criminal and noncriminal outcomes at early and later adulthood. Arrests at Waves 10 to 12 (ages 21-23) are significantly related to the outcomes of the Waves 13 to 14 (ages 29-31). We conducted analyses on five outcomes at Waves 10 through 12 (ages 21-23) that may be caused by labeling. Three are crime-related (arrest, general crime, and drug use) and two are noncrime (education and unemployment) outcomes. Then we conducted further analyses on four outcomes at Waves 13 and 14 (ages 29-31). These examined two crime-related (general crime and drug use) and two noncrime (welfare and unemployment) outcomes.

Tables 2 to 4 show ordinary least squares OLS and logistic regression equations predicting these outcomes. All the equations fit the data quite well with statistically significant *F* tests and model improvement chi-square tests. Because the analyses are longitudinal, focusing on three broad time periods ranging from adolescence (Waves 1-9, ages 14-18) to young adulthood (Waves 10-12, ages 21-23), and to later adulthood (Waves 13 and 14, ages 29-31), direct and indirect effects must be considered. Table 2 shows the equations for young adult crime and noncrime outcomes, whereas Tables 3 and 4 show equations for noncrime and crime outcomes, respectively. We show the equations in reduced form.

Our findings show that some of the latent trait variables introduced as controls in our models were significantly related to noncriminal and crime outcomes, as were a few other control variables (gender, African American race, academic aptitude, and poverty). For example, Table 2 shows the equations for young adults. Aggression and low self-control directly predict all three forms of subsequent criminogenic behavior (arrest, general crime, and drug use) and predict both noncrime outcomes (education and unemployment) for early adults. Tables 3 and 4 show that for later adults aggression becomes insignificant, but low self-control still predicts both noncriminal (unemployment and welfare status) and one criminal outcome (drug use). In addition, there is a somewhat inconsistent link between early general delinquency and drug use and general crime and drug use for young but not for later adulthood.

Our analysis also demonstrates how the impact of early labeling on later outcomes has the character of cascading, indirect effects ranging over the life course—from adolescence well into adulthood. Figure 2 shows the direct and indirect labeling effects as a causal diagram. We include the results for the hypothesized paths and omit other effects for clarity. As the figure shows, although arrest/police contact in adolescence has no direct impact on any of the outcomes for later adults (29- to 31-year-olds), it does have indirect effects on one crime outcome well into adulthood. Experiencing police intervention during adolescence (Waves 1-9, ages 14-18) more than triples the odds ($e^{1.22} = 3.38$, $p < .001$) of being arrested in young adulthood (Waves 10-12, ages 21-23), and this in turn has a positive and statistically significant effect on drug use at about age 30.[4]

What is particularly interesting here is that police intervention has short-term and long-term effects, contemporaneously. And these effects, some of which unfold over nearly two decades of the life course, hold while controlling for all other variables measured in adolescence and in young adulthood—including antisocial proclivities. So, whereas the Bernburg and Krohn (2003)

Table 2. Outcomes at Waves 10 to 12 (Ages 21-23) Predicted by Variables at Waves 1 to 9 (Ages 14-18; $N = 917$)

	Crime outcomes							Noncrime outcomes				
	Arrest			General Crime		Drug Use		Education[a]			Unemployment	
	B	*SE*	*p*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*p*	*b*	*SE*
Independent variables												
Male	0.92***	.20	.72	0.61***	.18	0.68***	.17	−0.10	.18	.47	−0.13***	.02
African American	0.44	.24	.61	−0.06	.23	0.04	.23	−0.69**	.24	.33	0.08**	.03
Hispanic	0.34	.30	.58	−0.06	.29	0.16	.28	0.07	.29	.52	0.06	.04
Poverty	0.00	.18	.50	−0.10	.17	−0.11	.17	0.35	.18	.59	0.05	.02
Academic Aptitude	−0.00	.00	.50	0.01*	.00	0.00	.00	−0.02***	.00	.49	−0.00***[a]	.00
Aggression	0.58*	.23	.64	0.57*	.24	0.56*	.22	0.80**	.24	.69	0.09**	.03
Low Self-Control	0.46*	.18	.61	1.36***	.18	1.01***	.17	0.44*	.19	.61	0.05*	.02
Arrest or Contact 1-9	1.22***	.16	.77	0.61***	.17	0.64***	.17	1.29***	.17	.78	0.14***	.02
General Delinquency 2-9	0.00	.00	.50	0.00**[a]	.00	0.00*[a]	.00	0.00	.00	.50	−0.00	.00
Drug Use 2-9	0.00**[a]	.00	.50	0.00	.00	0.01***	.00	0.00**[a]	.00	.50	0.00**[a]	.00
Average likelihood ratio	189.24***			—		—		239.95***			—	
Average *F* test	—			26.89***		27.33***		—			21.97***	
Average adjusted R^2	—			.22		.22		—			.19	

[a]A fractional quantity less than .01 in absolute value.
*$p < .05$. **$p < .01$. ***$p < .001$.

Table 3. Noncrime Outcomes at Waves 13 to 14 (Ages 29-31) Predicted by Variables at Waves 1 to 9 and 10 to 12 (Ages 21-23; $N = 917$)

	Welfare			Welfare			Unemployment			Unemployment		
	b	*SE*	*P*	*b*	*SE*	*p*	*B*	*SE*	*p*	*b*	*SE*	*p*
Independent variables												
Male	−1.52***	.19	.18	−1.57***	.20	.17	−0.46	.26	.39	−0.14	.30	.47
African American	0.44	.28	.61	0.40	.30	.60	0.67	.44	.66	0.44	.46	.61
Hispanic	0.65*	.33	.66	0.56	.34	.64	0.56	.49	.64	0.29	.50	.57
Poverty	0.17	.19	.54	0.11	.20	.53	−0.35	.26	.41	−0.50	.29	.38
Education	—	—	—	0.54**	.20	.63	—	—	—	0.40	.31	.60
Academic Aptitude	−0.01**	.00	.50	−0.01	.00	.50	−0.01*	.00	.50	−0.01	.01	.50
Aggression	0.21	.25	.55	−0.02	.26	.50	0.50	.32	.62	0.25	.35	.56
Low Self-Control	0.51**	.19	.62	0.38	.20	.59	0.60*	.27	.65	0.57*	.28	.64
Unemployment 10-12	—	—	—	0.87**	.31	.70	—	—	—	2.25***	.44	.90
Arrest or Contact 1-9	0.54**	.18	.63	0.14	.20	.54	0.67*	.27	.66	0.17	.30	.54
Arrest 10-12	—	—	—	0.46*	.21	.61	—	—	—	0.38	.30	.59
General Delinquency 2-9	−0.00	.00	.50	−0.00	.00	.50	0.00	.00	.50	0.00	.00	.50
General Crime 10-12	—	—	—	0.00	.00	.50	—	—	—	0.00	.00	.50
Drug Use 2-9	0.00	.00	.50	0.00	.00	.50	0.00	.00	.50	0.00	.00	.50
Drug Use 10-12	—	—	—	0.00	.00	.50	—	—	—	−0.00	.00	.50
Average likelihood ratio	134.04***			178.30***			47.89***			107.81***		

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

results detected what is now a relatively short-term effect, this analysis shows that labeling has a longer term, indeed lasting effect on drug use.[5]

More importantly, the consequences of early police intervention do not exclusively concern criminal outcomes in adulthood. Our results show statistically significant effects for unemployment and high school graduation for young adults that cascade into unemployment and welfare receipt for older adults. The impact of early labeling also operates through arrests in young adulthood to welfare receipt later. In other words, adolescents experiencing police contact or arrest have higher odds of arrest in young adulthood. Participants arrested in young adulthood (i.e., early 20s) then have 57% higher odds ($e^{0.46} = 1.57$, $p < .05$) of depending on welfare at around the age of 30.

Table 4. Crime Outcomes at Waves 13 to 14 (Ages 29-31) Predicted by Variables at Waves 1 to 9 (Ages 14-18) and 10 to 12 (Ages 21-23; *N* =917)

	General Crime		General Crime		General Crime		Drug Use		Drug Use		Drug Use	
	b	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*	*b*	*SE*
Independent variables												
Male	.38***	.05	.33***	.06	.35***	.06	.69***	.17	.47**	.17	.47*	.18
African American	–.01	.07	–.03	.07	–.02	.07	.36	.23	.32	.23	.31	.23
Hispanic	.02	.09	.00	.09	–.00	.09	.39	.30	.33	.30	.32	.30
Poverty	–.08	.05	–.08	.05	–.09	.05	–.15	.18	–.17	.17	–.17	.17
Education	—	—	–.03	.06	–.03	.06	—	—	.02	.17	.02	.17
Academic aptitude	.00***[a]	.00	.00***[a]	.00	.00***[a]	.00	.01*	.00	.01*	.00	.01*	.00
Aggression	.16*	.07	.13	.07	.14*	.07	.54*	.25	.35	.24	.35	.24
Low Self-Control	.26***	.06	.21***	.06	.22***	.06	.47**	.17	.23	.17	.23	.17
Unemployment 10-12	—	—	–.03	.09	.00	.10	—	—	–.00	.28	–.01	.29
Unemployment 13	—	—	—	—	–.16	.08	—	—	—	—	.03	.24
Arrest or contact 1-9	.13*	.05	.09	.06	.09	.06	.40*	.16	.15	.17	.15	.17
Arrest 10-12	—	—	.11	.06	.11	.06	—	—	.47**	.18	.47**	.18
Welfare 13	—	—	—	—	.06	.06	—	—	—	—	.02	.17
General Delinquency 2-9	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00
General Crime 10-12	—	—	.00	.00	.00	.00	—	—	.00	.00	.00	.00
Drug Use 2-9	.00	.00	.00	.00	.00	.00	.00***[a]	.00	.00*[,a]	.00	.00*[,a]	.00
Drug Use 10-12	—	—	.00	.00	.00	.00	—	—	.00	.00	.00	.00
Average *F* test	19.94***		17.25***		15.65***		15.19***		20.56***		18.13***	
Average adjusted R^2	.17		.21		.21		.13		.24		.24	

[a]A fractional quantity less than .01 in absolute value.
*$p < .05$. **$p < .01$. ***$p < .001$.

We see also that those with police intervention early in life report more unemployment as young adults (Waves 10-12). Those who were unemployed during young adulthood (Waves 10-12, ages 21-23) subsequently have more than 9 times the odds ($e^{2.25} = 9.49$, $p < .001$) of being unemployed as later adults. Finally, those who experience police intervention in adolescence have more than 3 times the odds ($e^{1.29} = 3.63$, $p < .001$) of not obtaining a high school degree, and these participants then have almost double the odds

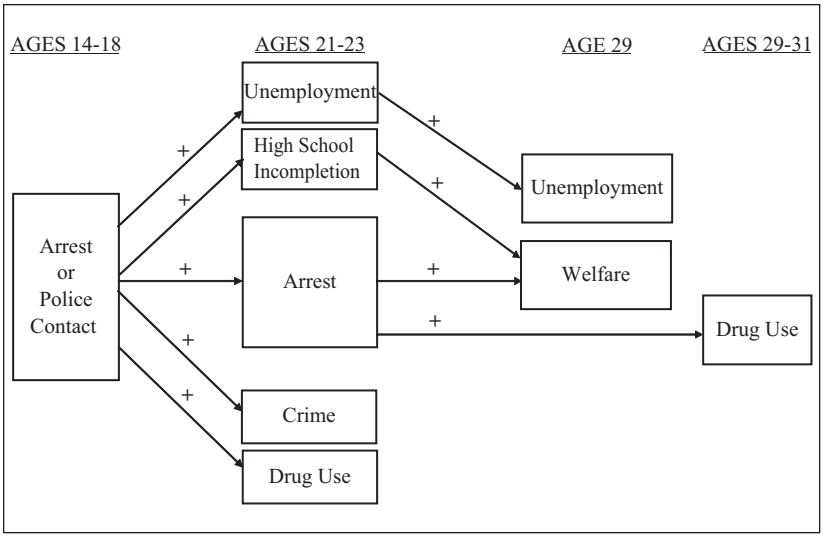

Figure 2. Selected direct and indirect effects of early official intervention on early and later adult outcomes
Note: Results shown are statistically significant.

($e^{0.54} = 1.72$, $p < .01$) of being on welfare at Wave 13 (age 29). So, again, the effects of early labeling cascade through young adult arrest, unemployment, educational deficits, and drug use reducing the chances for employment and fostering welfare dependence later in life.[6]

As one would suspect from indirect effects, the reduced form of equations in Table 3 show that unemployment, graduating from high school, and arrest in early adulthood totally mediate the impact of adolescent arrest on early adult unemployment and welfare receipt. Similarly, early adult arrest mediates the impact of adolescent arrest on adult drug use. There are neither direct nor indirect effects of adolescent arrest on general delinquency for later adult general crime. This is curious because there is a moderate and statistically significant correlation between adolescent arrest and early adult general crime ($r = .23$) and between adolescent arrest and later adult general crime ($r = .16$). We discuss this in the following section.

Discussion

The recent revitalization of labeling research coincides with the advance of the life-course approach to examining trajectories of crime and drug use.

Contemporary studies assessing the impact of official labels on subsequent behavior highlight the detrimental effects of intervention on criminal outcomes, as well as underscore the importance of noncriminal outcomes that are critical in shaping a person's life chances. Specifically, the effects of police intervention on a person's educational opportunities, employment prospects, and economic well-being not only have been conceptualized as intervening variables between the label and subsequent criminal and drug using behavior, but they are also now treated as important outcomes in their own right in determining the life course of labeled individuals.

This study addressed the primary limitation of prior prospective analyses of the impact of labeling by examining the effect of having been arrested in adolescence on subsequent life chances and continued participation in crime and drug use at the age of 30 or so. Importantly, by expanding the time range of analysis, we were able to take a longer view of the potential effects of labeling over one's life course and examine outcomes at two discrete time points—when respondents were in their early 20s and at about age 30. In this sense, the time span from when initial contact with the juvenile justice system occurred to the observed outcomes is between 13 and 17 years, well past the time when these young men had formal contacts with police or were arrested. More significantly, extending the time period to the late 20s and early 30s allows for an examination of the impact of the label at an age when most young men will have completed their education and are beginning to establish a career path, thereby attaining financial independence—a traditional marker of transition to adulthood. Problematic noncriminal and criminal outcomes at these ages may be particularly indicative of a life beset by continuing economic hardship and illegal behavior.

Our findings indicate that police intervention during adolescence has a significant, indirect effect on criminal and noncriminal outcomes when respondents were in their late 20s and early 30s. For example, we find that early labeling's effects continue to affect one's financial stability well beyond their early 20s. In this sense, police intervention in adolescence is indirectly related to the probability of receiving welfare when participants are roughly 30 years old, through the increased probability of being arrested in their early 20s and their failure to receive a high school diploma. In addition, adolescent intervention has an effect on unemployment in young adulthood, which in turn is significantly related to later adult unemployment (age 29). Official police intervention in adolescence also increases the likelihood that these young men will continue to use drugs in later adulthood. Adolescent arrests/police contacts are indirectly related to later drug use through arrests at ages 21 to 23.

Our results show two related and equally important stories about how police intervention affects the later life course. Clearly, early intervention increases the probability that those who are labeled will continue to be involved in illegal behaviors some 15 or more years later. The impact of the label is not just a short-term phenomenon; rather, police intervention can lead indirectly to an increased likelihood of continued crime for many years thereafter.

Though these criminogenic effects of early labeling are expected to carry serious implications for the life course of these young men, our findings regarding the impact of police intervention on indicators of economic well-being may be even more significant. Because those who have been labeled have an increased probability of continued participation in drug use, we would expect most of them to decrease their involvement in crime and drug use as they age. Contrastingly, the economic disadvantage that is, in part, due to the label can be expected to continue to affect the life chances of our respondents.

These findings corroborate our hypothesized indirect effects of early labeling on criminal behavior and on economic unsustainability at age 30 or so. By age 29, those who experienced formal police intervention were more likely to be unemployed and on welfare. Because later adulthood unemployment and welfare status were measured contemporaneously with criminal behavior and drug use outcomes when respondents were in their early 30s, we need to treat the results as tentative. Given the finding of an indirect effect of arrest on drug use at ages 29 to 31, it was surprising that a similar effect was not found for the general crime measure. A possible explanation for this may be the stability of our general crime measure from ages 21-23 to 29-31. When we eliminate the early adulthood measure (but retain the adolescent measure), there is a direct effect of police intervention at early adulthood on general crime at ages 29 to 31. Future research will need to delve into this issue to explain the lack of an indirect effect on general crime in the full model.

In addition, our results point to the salient role played by latent trait measures in predicting subsequent crime involvement (as well as deficits in education and welfare receipt), thereby suggesting that pathways to deviance involve intraindividual factors in addition to the labeling effects aforementioned. This is consistent with a developmental approach, which "acknowledges the importance of early childhood behaviors and individual differences in self-control" (Sampson & Laub, 1993, p. 7), while rejecting the implication that later adult social factors are irrelevant.

Overall, our results help shed some light on previous research by exploring how opportunities for a conventional life are "knifed off" (see Caspi & Moffitt, 1993; Sampson & Laub, 1993, 1997) in terms of official reactions to criminogenic behavior. For example, Moffitt (1993) has argued that continuity in offending behavior throughout the life course is due partly to the ensnaring consequences of such behavior. The author highlights how certain events, such as school dropout and drug use, are in effect "*snares* that diminish the probability of later success" (Moffitt, 1993, p. 684, italics in original). Our results demonstrate how salient labeling's role is in leading to these detrimental events that prune away prosocial options for change in behavior. The "bewildering forest of unsavory outcomes" (Caspi & Moffitt, 1995, p. 497) brought about by persistency in drug use and unsatisfactory financial independence are shown here to be affected by police intervention at two points in time even after we control for underlying personality traits that contribute to an innate propensity to antisocial behavior—poor self-control, aggressiveness, and low cognitive ability.

Other events, such as marriage (see, for example, Sampson & Laub, 1993; Sampson, Laub, & Wimer, 2006), have been highlighted as salient episodes in one's life course, which play a part or a "turning point" in one's future criminogenic behavior. Our study did not examine the role of marriage as a potential deterrent. As argued by Sampson et al. (2006), "marriage has the potential to 'knife off' the past from the present in the lives of disadvantaged men" and as such it may lead to increased prosocial opportunities (p. 498). Incorporating marriage and other variables whose impact may be expected to vary by gender (such as the potential role of teenage pregnancy as a "turning point") would allow for a more detailed examination of gender-based contingent effects of labeling (e.g., Feld, 2009). In addition, further analyses on the relationship between race, socioeconomic status, criminal history, and employment stability are needed. Though in this study we did not find interaction effects of race and poverty status with official labeling on later adult outcomes, recent research by Devah Pager and colleagues (Pager, Bonikowski, & Western, 2009; see also Bernburg & Krohn, 2003) is suggestive of the importance of testing these relationships in other longitudinal panel data to ascertain the prevalence of discrimination in shaping employment opportunities for those who have experienced intervention.

Moreover, our measure of the "label" police contact or arrest does not assess the immediate impact on the boy and those with whom he had regular contact (family members, school officials). Nor does this measure assess the

relative impact of different degrees of labeling. That is, do more invasive interventions such as incarceration lead to more problematic outcomes? These are certainly important issues to examine in future research. However, it is particularly significant that we have demonstrated that even with a relatively less invasive intervention and no assessment of the impact on the individual and his associations, we have found that police intervention does have important effects that result in the continuation of problematic life chances at an age when life trajectories are becoming more stable.

The life-course perspective has underscored the importance of events and behaviors throughout one's life. Being labeled is a traumatic event that has been shown not only to lead to continued criminal involvement but also to other deleterious outcomes, such as economic hardship. Our research, along with other studies that have examined the impact of official labeling on the life course, demonstrates that not only is the perspective not dead, but it may also be even more relevant than it was before.

Noninterventionist criminal justice policies defended by labeling proponents in the 1970s (see, for example, Schur, 1973), and later supplanted by "get tough" approaches to delinquency, have experienced a recent revival in popularity. Predominantly, the decriminalization of narcotics use (Polsby, 1997), as well as diversion alternatives, has enjoyed current scholarly discussion. The results presented here show how proposals articulated by researchers like Schur are still relevant—not only due to the criminogenic effects of an affixed label, as he and others anticipated, but also due to its curtailment of key prosocial opportunities. Our study hopes to inform policy research about how the above-mentioned structural mechanisms underlying the impact of police intervention actually work to amplify detrimental outcomes in crucial arenas of one's life.

Appendix

Table A1. *Correlations Between Independent Variables and Wave 10 to 12 (Ages 21-23) Outcomes (N = 917)*

	Arrest	General crime	Drug use	Education	Unemployment
Male	.20***	.17***	.19***	.06	−.15***
White	−.07*	.04	.02	−.01	−.18***
African American	.05	−.03	−.04	−.08*	.14***
Hispanic	.00	−.01	.02	.11***	−.00
Poverty	.03	−.03	−.03	.11**	.14***
Academic Aptitude	−.10**	.01	−.04	−.28***	−.24***
Aggression	.16***	.18***	.18***	.20***	.15***
Low Self-Control	.19***	.36***	.30***	.18***	.12***
Arrest or Contact 1-9	.36***	.23***	.25***	.36***	.29***
General Delinquency 2-9	.19***	.32***	.28***	.18***	.10*
Drug Use 2-9	.23***	.21***	.35***	.21***	.14***

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

Table A2. Correlations Between Independent Variables and Wave 13 to 14 (Ages 29-31) Outcomes ($N = 917$)

	General crime	Drug use	Welfare	Unemployment
Male	.29***	.18***	−.29***	−.03
White	.09*	−.01	−.14***	−.08*
African American	−.07	−.00	.07*	.06
Hispanic	−.00	.01	.05	.00
Poverty	−.09*	−.04	.09**	−.02
Education	.09*	.15***	.22***	.20***
Academic Aptitude	.11**	.04	−.15***	−.12***
Aggression	.14***	.14***	.06	.09*
Low Self-Control	.25***	.19***	.09*	.12***
Unemployment 10-12	−.00	.07*	.28***	.33***
Unemployment 13	−.03	.05	—	—
Arrest or Contact 1-9	.16***	.18***	.11**	.15***
Arrest 10-12	.21***	.25***	.13***	.16***
Welfare 13	−.03	.02	—	—
General Delinquency 2-9	.17***	.20***	.04	.07
General Crime 10-12	.23**	.25**	.04	.03
Drug Use 2-9	.16***	.27***	.05	.08*
Drug Use 10-12	.26***	.40***	.05	.02

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

Declaration of Conflicting Interests

The authors declared no potential conflicts of interests with respect to the research, authorship, and/or publication of this article.

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Notes

1. In using police intervention (contact with or arrest by the police), we are testing the assumption that the experience has a significant impact on youth. We do not have a measure of how youth perceived the experience, which may have better assessed the labeling effect. We would anticipate that using such a measure would increase the probability of finding support for our hypothesis. If we indeed find that police intervention has the hypothesized effects without assessing the subjective impact, there will be strong support for the theoretical argument.
2. We also used serious delinquency and drug sales as outcome measures. The results were similar, but too few respondents reported involvement in those types of behavior, and we do not have confidence in the reliability of those results.
3. As indicated, general crime comprises 26 items, such as weapon-carrying, theft, assault, car theft, and drug sales, but not drug use. Drug use comprises 11 items, including marijuana, inhalants, hallucinogens, cocaine, crack, heroin, PCP, tranquilizers, downers, uppers, and other drugs, such as steroids or ecstasy. Aggression comprises 12 items, such as being restless/hyperactive, cruelty to animals, fighting, and threatening people. Low self-control also comprises 12 items, such as acting on the spur of the moment, not devoting thought/effort to preparing for the future, and doing risky things.
4. To examine further whether the effect of labeling (i.e., arrests in adolescence) on drug use is due to some unmeasured variable or selection effect, we used a

difference-in-differences test (see Ashenfelter & Card, 1985, for an example of this method). Essentially, this test compares change scores between individuals who were labeled during Waves 4 to 9 and those who were not labeled, making this test inherently free of bias due to unobserved heterogeneity, or differences between individuals that are stable over time. We took a prelabeling measure of drug use across Waves 1 to 3 (this was to ensure enough variability on the pre-measure) and a postlabeling measure of drug use across Waves 10 to 12, then compared the change in means for each group. The results show a significant difference between groups in the prelabeling mean drug use and the postlabeling mean drug use, suggesting a selection effect into labeling. However, the difference-in-differences test also is significant, which means that the labeled group experienced a larger increase in drug use than the nonlabeled group. This supports our finding that it is the experience of being labeled that causes increased drug use, not differences between individuals that are stable over time. As a second sensitivity analysis, we ran a propensity score matching (PSM) analysis to account for any selection bias present in our sample. After matching on early covariates, we then used labeling to predict all of the outcomes. The average treatment effect on the treated (i.e., labeled individuals) remained statistically significant for predicting all outcomes at Waves 10 to 12 and Waves 13 to 14. This bolsters our conclusion that early labeling has long-standing negative effects for later arrest, financial well-being, and crime.

5. In their study, Bernburg and Krohn (2003) also examined whether the impact of official intervention on educational achievement, employment opportunities, and young adults' criminality is contingent on sociostructural factors, namely, poverty and minority statuses. They found, for instance, that the effect of juvenile intervention on drug selling in early adulthood is stronger among those from impoverished families. Likewise, we examined potential interaction effects of race and poverty status with official labeling on later adult outcomes. We did not find any such contingent effects (results not shown).
6. Because our measures of welfare receipt and unemployment are contemporaneous with measures of criminal behavior and drug use, we do not include the findings concerning the indirect effect through welfare and unemployment of the antecedent variables on problem behavior outcomes in the formal part of the analysis. We did, however, estimate the relevant equations and found that there was not an indirect effect of official labeling on problem behavior outcomes through unemployment and welfare receipt.

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