

The Effect of Police Contact: Does Official Intervention Result in Deviance Amplification?

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Stephanie A. Wiley¹ and Finn-Aage Esbensen¹

Abstract

Current police policies are based on assumptions that proactive policing strategies will not only deter crime but will also improve police–community relations. Deterrence theorists argue that general and specific deterrence can be achieved through such policing strategies. Labeling proponents, however, maintain that juveniles stopped and/or arrested by the police, rather than be deterred, will actually engage in more delinquency as a result of this contact. Research to date has provided mixed evidence. The current study seeks to inform this debate by examining the effect of being stopped or arrested on subsequent delinquent behavior and attitudes. Relying on three waves of data from a multisite sample of youth, we use propensity score matching to control for preexisting differences among youth who have and have not experienced police contact. Our findings reveal that being stopped or arrested not only increases future delinquency but also amplifies deviant attitudes.

Keywords

arrest, police contact, labeling, deterrence

¹University of Missouri–St. Louis, USA

Corresponding Author:

Stephanie A. Wiley, Criminology and Criminal Justice, University of Missouri–St. Louis, 324 Lucas Hall, One University Blvd, St. Louis, MO 63121, USA.

Email: saw6fd@mail.umsl.edu

Guided by the *parens patriae* doctrine, the juvenile justice system, in principle, should ensure the safety and well-being of the child. However, a number of commentators have documented abuses within the system (e.g., Miller, 1998; Pisciotta, 1994; Platt, 1969), primarily postadjudication. Perhaps equally important is the assessment of the effect of justice system contact in the earliest stages of official intervention. What, for instance, is the effect of police contact or arrest on the individual?¹ Proponents of deterrence theory maintain that conventional policing practices and order maintenance strategies reduce offending and that arresting an individual will reduce the probability of further illegal activity. Labeling theorists, on the other hand, argue that arrest, even police contact, results in deviant identity formation and increased offending. These two theories, which represent polar opposites in terms of consequences of police contact, also have distinct implications for policy.

In his controversial book, Schur (1973) makes a theoretically based argument for "radical non-intervention," that is, whenever possible, leave the kids alone. He, along with other labeling theorists, maintains that official responses to deviant behaviors only serve to exacerbate the situation by placing a negative label on the child. This label may result in social exclusion, adoption of a deviant identity, increases in time spent with and commitment to deviant peers, and reinforcement of the deviant behavior (Lemert, 1951; Paternoster & Iovanni, 1989). Policies informed by labeling theory, then, focus on hands-off or socially inclusive approaches aimed at decreasing the negative effects of justice system contact. The historically more dominant perspective in criminology, deterrence theory, posits that legal sanctions and official reaction deter offending by increasing the perceived risks associated with crime (Stafford & Warr, 1993). Modern policing practices are often based on the notion that police presence and contact will monitor and prevent illegal behavior and deterrence-oriented policies rely on swift, certain, and severe punishment to reduce future offending.

Over the years, juvenile justice policies have reflected deterrence and labeling perspectives. Following a wave of labeling research in the 1970s (Ageton & Elliott, 1974; Fisher & Erickson, 1973; Foster, Dinitz, & Reckless, 1972; Jensen, 1972) and Schur's (1973) proposal of radical nonintervention, diversion programs were created to reduce the negative consequences associated with justice system contact. But rather than reducing the flow of juveniles through the justice system, diversion programs appeared to have a net-widening effect. That is, youth workers (from juvenile officers to youth counselors) referred more youth to diversion programs with the belief that such programs resulted in positive outcomes and little to no stigmatizing effects. This increased the number of juveniles who were diverted and

eventually led to the conclusion that diversion programs were ineffective (Austin & Krisberg, 1981; Dunford, 1977; Lemert, 1981).² Thus negative evaluations of diversion programs, coupled with criticisms of labeling theory (e.g., Hirschi, 1980; Tittle, 1980), led to premature dismissal of labeling theory and related policies, which increased deterrence-oriented juvenile justice policies during the 1980s and 1990s.

Aggressive deterrence-oriented policing strategies continue in communities today (see, for example, Jones-Brown, Gill, & Trone, 2010) and schools commonly rely on zero-tolerance policies and increased presence of police officers to prevent crime (Petteruti, 2011). Schools have also begun to collaborate with the justice system to provide early intervention programs (e.g., see Wright, McMahon, Daly, & Haney, 2012), and while they appear to avoid the net-widening problems faced by diversion programs of the past, their long- and short-term effects are unclear (Greenwood, 2012). The continued use of aggressive policing measures is somewhat surprising given legal challenges and the differential impact of such strategies on minorities (for review, see Irwin, Davidson, & Hall-Sanchez, 2013; Jones-Brown et al., 2010; Petteruti, 2011). In addition, research suggests that justice system contact is linked to a host of negative outcomes, ranging from fewer educational and employment opportunities to higher levels of delinquency and deviant attitudes (Ageton & Elliott, 1974; Bernburg & Krohn, 2003; Bernburg, Krohn, & Rivera, 2006; Huizinga, Esbensen, & Weiher, 1996; Kaplan & Johnson, 1991; Lopes et al., 2012). Much of this research focuses on arrest or later justice system processing; thus, little is known about the effects of less serious types of police contact, such as being stopped, that are associated with aggressive policing strategies.

The current study seeks to add to the body of knowledge regarding the consequences of police contact by examining the effects of being stopped, as well as being arrested, on delinquent behavior and attitudes. Although recent studies overcome many of the limitations of early tests of labeling theory (see Paternoster & Iovanni, 1989 for review of early tests), they vary in the extent to which prior criticisms are addressed. For instance, studies may include too few waves of data to test causal ordering; many do not address attitudinal change; research designs do not account for selection effects; and most studies group all types of official contact (from arrest to court processing) together, which masks differences based on the level of contact (Bernburg, 2009; Huizinga & Henry, 2008). Using three waves of data to ensure proper temporal ordering, we use propensity scores to match youth with no police contact, those who were stopped, and youth who were arrested. This allows us to account for possible selection bias while unmasking the effects of varying levels of police contact. To understand how contact affects the

individual's deviant behavior and attitudes, our outcomes include anticipated guilt, use of neutralization techniques, commitment to deviant peers, and delinquency frequency. Our findings support a labeling, rather than deterrent effect of police contact and the implications for juvenile justice programs and policies are discussed.

Prior Research

One of the enduring theoretical debates within criminology is that of labeling versus deterrence. In an effort to identify how official intervention affects future behavior, criminologists subject labeling and deterrence propositions to theoretical tests; however, results of these tests are often inconsistent. To date, the majority of research supports the idea that police contact amplifies deviance; yet, some finds that justice system contact has a deterrent effect and others report no effect on future behavior (see Bernburg, 2009 or [Huizinga & Henry, 2008](#), for reviews). These mixed findings are due, in part, to conceptual and methodological variations, which are reviewed in detail below.

Support for Labeling

Despite some conceptual and methodological differences, a number of studies provide support for labeling theory ([Farrington, 1977](#); [Huizinga et al., 1996](#); [Kaplan & Damphousse, 1997](#); [McAra & McVie, 2007](#)). Not only does this body of research find that police contact results in increased delinquency, but it also shows that official intervention leads to the knifing off of prosocial opportunities and bonds ([Bernburg & Krohn, 2003](#); [Lopes et al., 2012](#)), increased identification with deviant attitudes and self-concepts ([Ageton & Elliott, 1974](#); [Kaplan & Johnson, 1991](#); [Thomas & Bishop, 1984](#)), and greater involvement with deviant peers ([Bernburg et al., 2006](#); [Johnson, Simons, & Conger, 2004](#)). Relying on official police contact records, Lopes and colleagues (2012) showed that arrest and nontrivial police encounters lead to increased offending and fewer educational and employment opportunities. Other research using measures of a self-reported justice system involvement find that police contact is associated with increased delinquency and greater involvement with deviant peer groups ([Bernburg et al., 2006](#); [Johnson et al., 2004](#)). Importantly, these findings indicate that police contact, whether measured using self-report or official data, results in negative social and behavioral consequences.

In addition to variation in data sources, extant research does not systematically define police contact. Broad measures of official contact may not adequately capture the effects of early stages of justice system contact, as labels change with progression through the justice system ([Huizinga & Henry, 2008](#); [Paternoster & Iovanni, 1989](#)). Much of the labeling research focuses on

the effect of any justice system involvement ranging from arrest to incarceration (e.g., [Ageton & Elliott, 1974](#); [Bernburg et al., 2006](#); [Johnson et al., 2004](#)) or a combination of formal and informal sanctions ([Kaplan & Dampousse, 1997](#); [Kaplan & Johnson, 1991](#)). More recently, studies have focused on specific types of police contact, such as being stopped or arrested ([Bernburg & Krohn, 2003](#); [Lopes et al., 2012](#)). For example, Bernburg and Krohn (2003) assessed the impact of police contact (stopped or arrested) as well as justice system intervention (i.e., detention, brought to court, referral, transfer to adult institution, probation) on offending outcomes in young adulthood. Though they found that police contact and justice system interventions increased offending, there was some variation in the mechanisms leading to subsequent crime. Specifically, high school graduation and employment fully mediated the relationship between justice system intervention and drug sales, but the direct effect of police contact on drug sales remained significant even after inclusion of the mediators. Such findings highlight the need to disentangle the effects of varying levels of justice system contact.

Support for Deterrence

Few published studies have found support for a specific deterrent effect associated with official intervention. For the most part, these studies are interested in the impact of sanctions such as fines, probation, or incarceration ([Brennan & Mednick, 1994](#); [Fass & Pi, 2002](#); [Murray & Cox, 1979](#)). However, Smith and [Gartin \(1989\)](#) examined the effect of arrest on offending by comparing adolescent males who were released following police contact with those arrested and referred. They found that although arrest resulted in fewer future police contacts, the specific deterrent effect was different for novice and experienced offenders. That is, arrest resulted in the termination of offending for novice offenders, but for more experienced offenders arrest only decreased criminal involvement. Although these results support deterrence theory, it is important to note that official data were used for the intervention and outcome measures. Reliance on official criminal records likely underestimates levels of offending and could lead to the conclusion that police contact has a deterrent effect. In addition, because the entire sample experienced some form of police contact, the Smith and Gartin (1989) study does not assess a true treatment effect.

Limited or Null Findings

Another body of research suggests that official contact has limited ([Gibbs, 1974](#); [Hepburn, 1977](#); [Thomas & Bishop, 1984](#)) or no effect ([Hemphill,](#)

Toumbourou, Herrenkohl, McMorris, & Catalano, 2006; Lane, Turner, Fain, & Segal, 2005; Smith & Paternoster, 1990) on delinquent identity and behavior. For example, Gibbs (1974) found only a short-term effect of police contact on delinquent identity: Boys processed by the juvenile justice system scored higher on delinquent self-concept immediately following police contact, but they were no different from the control group after 45 days. Thomas and Bishop's (1984) study, on the other hand, uncovered a positive relationship between official sanctions (police contact and/or court involvement) and delinquent self-concepts, but not delinquency. Though these studies suggest that police contact leads to changes in identity, the limited findings may be partly explained by reliance on only two waves of data. Such data limitations lead to the inability to compare delinquent attitudes and behavior before and after intervention and may account for the lack of support for labeling theory among early research.

One concern across labeling and deterrence research is the issue of selection bias; that is, findings supportive of a labeling effect may reflect the fact that youth who come into contact with the justice system are more likely to offend. To illustrate this, Smith and Paternoster (1990) compared delinquent outcomes for court-referred and diverted youth. Findings based on standard multiple regression analysis revealed a labeling effect for youth who experienced court referral, but there was no effect when the authors applied a two-step correction to control for selection bias. More recently, propensity score matching has been used to address selection bias. These results show that court hearings (McAra & McVie, 2007) and incarceration (Nieuwbeerta, Nagin, & Blokland, 2009) are associated with increased delinquency. Interestingly, although McAra and McVie (2007) found no labeling effect for youth who were charged or referred, they found that court hearings were associated with higher levels of delinquency. Although these studies are not generally concerned with the effects of initial police contact, they underscore the need to carefully control for preexisting characteristics associated with delinquency and provide evidence that labels vary with progression through the justice system.

The studies reviewed here rely on self-report and official data sources and assess the impact of various levels of justice system intervention, yet they are generally supportive of labeling theory. Inconsistencies in data sources (i.e., official versus self-report) used to measure offending outcomes and improper temporal ordering likely account for the substantive differences. Overall, the quality of labeling research has improved, and recent research adds to the empirical support of labeling theory by emphasizing mediating and moderating effects as well as variation in types of official intervention (see Bernburg, 2009, for review). Still, relatively few studies have assessed the impact of

initial police contact (i.e., stop, arrest) on adolescent behaviors and attitudes while adequately accounting for selection bias.

The current study fills several gaps in the existing police contact literature. Specifically, we account for possible selection effects by matching youth with varying levels of police contact on Time 1 (T1) characteristics associated with police contact and delinquency. By comparing arrested youth, stopped youth, and youth with no police contact (measured at Time 2), we are able to determine whether contact severity differentially affects the behaviors and attitudes reported at Time 3 (T3). This is important from a policy perspective, as it remains unclear whether minimal justice system involvement such as that resulting from “stop and frisk” or aggressive policing efforts do more harm than good.

Data and Method

The current study relies on data from the second National Evaluation (2006–2013) of the Gang Resistance Education and Training (G.R.E.A.T.) Program. The G.R.E.A.T. Program is taught by police officers in sixth- and seventh-grade classrooms across the United States to help prevent gang involvement, violence, and crime, as well as to improve relationships between youth and police officers. Site selection for the evaluation was based on three main criteria: (a) existence of an established G.R.E.A.T. program, (b) geographic and demographic diversity, and (c) evidence of gang activity. The seven cities chosen for the evaluation were as follows: Albuquerque, NM; Chicago, IL; a Dallas/Ft. Worth area in TX; Greeley, CO; Nashville, TN; Philadelphia, PA; and Portland, OR. After city selection, the research staff worked with the primary local law enforcement agency and the school district in each city to secure their cooperation. On district approval, 31 schools were selected across the seven sites. The 195 appropriate grade-level classrooms within these schools were randomly assigned to treatment (i.e., G.R.E.A.T.) and control conditions (for more information about the evaluation design, see Esbensen, Peterson, [Taylor, & Osgood, 2012](#)).

Of the 4,905 students in the 195 classrooms, active parental consent was provided for 77.9% ($n = 3,820$) of the initial sample (for a review of the active consent process, see Esbensen, Melde, Taylor, & Peterson, 2008).³ The current study relies on the posttest and Waves 3 and 4 from the G.R.E.A.T. Evaluation. There is a 6- to 9-month lag between the posttest and Wave 3, and a 1 year lag between Waves 3 and 4.⁴ For ease of reference, we refer to these waves as times 1 through 3 (T1–T3). Of the 3,820 students in the final evaluation sample, 78% ($n = 2,972$) completed all three waves of data used for the current study. Deletion of cases with missing data results in an analysis

Table 1. Comparison of Study Variables by Level of Police Contact.

	Variable range	Total sample (N = 2,614)	No contact (n = 2,083)	Stopped (n = 361)	Arrested (n = 170)
Percentage/M (SD)					
T1 variables					
Age	9-15	11.80 (.74)	11.73 (.71)	12.01 (.78)	12.24 (.87)
Male		48.66%	44.36%	64.27%	68.24%
Race					
White		29.80%	32.74%	19.39%	15.88%
Black		16.07%	14.74%	20.78%	22.35%
Hispanic		39.71%	37.69%	46.26%	50.59%
Other		14.42%	14.83%	13.57%	11.18%
Impulsivity	1-5	2.75 (.80)	2.68 (.80)	2.98 (.76)	3.08 (.76)
Risk seeking	1-5	2.53 (.97)	2.40 (.93)	2.96 (.95)	3.25 (.97)
Anticipated guilt	1-3	2.64 (.55)	2.71 (.51)	2.47 (.58)	2.17 (.65)
Neutralizations	1-5	2.55 (.82)	2.41 (.77)	3.00 (.77)	3.30 (.80)
School commitment	1-5	3.81 (.73)	3.91 (.68)	3.52 (.79)	3.31 (.82)
Parental monitoring	1-5	4.15 (.75)	4.23 (.71)	3.93 (.77)	3.63 (.83)
Unsupervised time	0-3	.98 (.87)	.86 (.82)	1.37 (.89)	1.56 (.95)
Prosocial involvement	0-4	2.32 (1.19)	2.34 (1.19)	2.35 (1.21)	2.09 (1.23)
Prosocial peers	1-5	3.45 (1.00)	3.60 (.96)	3.01 (.92)	2.61 (.91)
Positive peer commitment	1-5	4.25 (1.09)	4.35 (1.03)	4.03 (1.16)	3.56 (1.37)
Peer delinquency	1-5	1.30 (.51)	1.22 (.39)	1.52 (.64)	1.90 (.84)
Negative peer commitment	1-5	1.73 (.87)	1.62 (.74)	2.01 (1.06)	2.44 (1.33)
Substance use (frequency)	0-16	.36 (1.17)	.21 (.84)	.66 (1.44)	1.52 (2.52)
Delinquency (frequency)	0-154	4.84 (11.89)	2.81 (7.14)	9.67 (17.83)	19.47 (23.74)
T3 variables					
Anticipated guilt	1-3	2.55 (.59)	2.64 (.54)	2.31 (.62)	1.94 (.61)
Neutralizations	1-5	2.75 (.84)	2.58 (.76)	3.32 (.79)	3.61 (.84)
Negative peer commitment	1-5	1.92 (1.01)	1.75 (.86)	2.40 (1.20)	2.87 (1.32)
Delinquency (frequency)	0-154	7.04 (15.26)	3.32 (6.99)	13.88 (16.74)	38.12 (34.01)

Note. Police contact is measured at T2. Column percentages may not equal 100 due to rounding. T1 = Time 1; T3 = Time 3.

sample of 2,614. Analyses reveal differential loss of high-risk youth due to attrition and missing data; however, this should result in an underestimate of the effects of police contact given that excluded youth reported higher levels of delinquency and police contact.⁵

Descriptive statistics for our full-analysis sample, as well as the no contact, stopped, and arrested groups, are presented in Table 1. Most respondents had no contact with the police (79.7%), but nearly 14% had been stopped and more than 6% had been arrested. Based on prior research and known delinquency risk factors (see, for example, Esbensen, Peterson, Taylor, & Freng, 2010), youths who have experienced police contact should, on average, exhibit higher levels of delinquency, more antisocial attitudes, and decreased social

control. As expected, moving from the “no contact” to “arrested” columns, the averages of variables linked to delinquent behavior (e.g., impulsivity, unsupervised time, peer delinquency) increase, while the means of variables associated with lower levels of delinquency (e.g., parental monitoring, school commitment) decrease.

Measures

To ensure proper temporal ordering, the covariates used to calculate propensity scores are measured at T1; the treatment, police contact is measured at T2; and our outcome variables are taken from T3. Summary indices or scales are used to measure key study variables and were computed if at least half the items had nonmissing data.

Dependent variables. This study includes four dependent variables, measured at T3, to capture a range of factors associated with deviance amplification. According to Lemert (1951), secondary deviation occurs when the individual takes on a primarily deviant role “as a means of defense, attack, or adjustment” (p. 76). This new role is accompanied by changes in self-identity, but because the deviant role is not approved by society, the individual must adjust to the incongruence between his or her self-identity and society’s expectations for its members. Therefore, Lemert (1951) suggested that psychological mechanisms such as rationalizations help to minimize this incongruence. Changes in the perceived wrongness of delinquent acts and use of neutralization techniques are believed to be reflective of the role-taking process and similar measures have been included to assess changes in deviant identity in previous tests of labeling theory (Ageton & Elliott, 1974; Kaplan & Johnson, 1991; Klein, 1986).

We include a seven-item scale of *anticipated guilt* to capture beliefs about the wrongness associated with delinquent acts. Respondents were asked how guilty they would feel if they participated in a variety of delinquent activities ranging from “stole something worth less than \$50” to “attacked someone with a weapon.” Responses are coded as “1 = *not very guilty*,” “2 = *somewhat guilty*,” and “3 = *very guilty*” ($\alpha = .93$). To assess the extent to which individuals agree with *neutralization* statements, respondents were asked if they think it is okay, for example, “to lie to someone if it will keep you out of trouble with them,” “to steal something if that’s the only way you could ever get it,” or “to beat someone up if they hit you first.” Responses for the nine items are based on a 5-point Likert-type scale ranging from “1 = *strongly disagree*” to “5 = *strongly agree*” ($\alpha = .84$).

Along with the formation of a deviant identity, secondary deviance is characterized by increased commitment to and involvement with other deviants,

which is thought to occur as labeled individuals are increasingly alienated by prosocial others (Lemert, 1951; Paternoster & Iovanni, 1989). Therefore, we include *negative peer commitment* to capture respondents' attitudes about involvement with deviant peers. Respondents reported the likelihood that they would continue to hang out with their delinquent friends if those friends were getting them into trouble: (a) at school, (b) at home, and (c) with the police. Response categories are scored on a 5-point scale ranging from "1 = *not at all likely*" to "5 = *very likely*" ($\alpha = .81$).

Finally, to capture the "overt demonstrations of the new role they symbolize," (Lemert, 1951, p. 75), we rely on a measure of *delinquency frequency*. Respondents reported their frequency of involvement over the last 6 months in 14 delinquent activities. This includes activities such as, "skipped classes without an excuse," "stolen or tried to steal something worth more than \$50," "attacked someone with a weapon," and "sold marijuana or other illegal drugs." The 11 response categories range from "0" to "more than 10" and responses for each item are summed to create a delinquency frequency measure ranging from 0 to 154. Most respondents reported little to no delinquency at T3, with youths indicating an average of 7.04 delinquent acts.

Treatment. Police contact is measured at T2 and represents the treatment variable. Respondents were asked how many times in the past 6 months they had (a) been stopped by the police for questioning and (b) been arrested. Three dichotomous variables representing arrested versus no contact, stopped versus no contact, and arrested versus stopped are included in this study to allow for comparisons by level of contact. Respondents who reported being stopped and arrested are coded as arrested to ensure that the most severe level of contact is represented.⁶

Covariates. The covariates used to calculate propensity scores are measured at T1 and are based on risk factors associated with police contact and delinquency. Demographic variables include age, sex, and race.⁷ Males are coded as "1" and dichotomous variables for Hispanic, Black, and Other are included, with White serving as the reference group. We also include T1 controls for guilt, neutralizations, negative peer commitment, and delinquency. Other covariates are based on risk factors associated with police contact and delinquency and include impulsivity, risk seeking, school commitment, parental monitoring, unsupervised time with peers, prosocial activities, prosocial peers, positive peer commitment, peer delinquency, and substance use.

Hypotheses. Based on the results of prior research, we expect our findings to support labeling theory. Following police contact, youth should report less

anticipated guilt and greater acceptance of neutralization statements as they take on increasingly deviant identities and attempt to minimize the incongruence between the new identity and society's expectations for its members. Furthermore, negative peer commitment should increase as a result of exclusion from prosocial groups or activities and increased interaction with and support from deviant peers. Finally, we expect delinquency to increase as a result of this labeling process.

Analysis

This study uses propensity score matching to determine whether being stopped or arrested affects subsequent attitudes and behavior. This technique allows for the comparison of individuals who have similar propensities to experience the treatment of interest, but have actually been exposed to different levels of treatment. Due to the three-category nature of police contact, we use multinomial probit regression to calculate the propensity of being stopped or arrested at T2 based on T1 covariates. Because groups are matched two categories at a time, one group is excluded from each propensity score calculation. For example, when matching youth who were stopped to those with no contact, arrested youth are excluded from the analysis and the propensity score represents the predicted probability of being stopped given T1 characteristics.

Individuals are matched using the `psmatch2` module ([Leuven & Sianesi, 2003](#)) for Stata 12.0 (2011). The results presented here are based on Epanechnikov kernel density matching with caliper and bandwidth of .05.⁸ This technique allows individuals in the treatment group to be matched to multiple individuals in the control group, given that the matches fit within the specified propensity score range ([Caliendo & Kopeinig, 2008](#)). This method decreases the likelihood of poor matches, but in doing so, drops cases if they do not fit within the specified parameters.⁹

Results

Covariate Balance

To check that matching eliminated selection bias associated with the covariates used in the analysis, T1 means are compared before and after matching. If there are significant mean differences between the treatment and control groups, covariate balance has not been achieved and conclusions regarding the effect of treatment may be inaccurate. The standardized bias statistic is used to determine covariate balance and represents the mean difference between

Table 2. Covariate Balance for Arrested Versus No Contact.

T1 variables	Unmatched			Matched		
	Arrested (n = 170)	No contact (n = 2,083)	Standardized bias (%)	Arrested (n = 164)	No contact (n = 2,083)	Standardized bias (%)
Age	12.24	11.73	64.4	12.23	12.12	13.6
Male	.68	.44	49.5	.70	.62	15.5
Race						
Black	.22	.15	19.7	.23	.25	-5.2
Hispanic	.51	.38	26.2	.50	.49	1.2
Other	.11	.15	-10.9	.11	.10	3.3
Impulsivity	3.08	2.68	51.2	3.05	2.92	16.2
Risk seeking	3.25	2.40	89.8	3.22	3.10	12.7
Anticipated guilt	2.17	2.71	-92.2	2.19	2.18	.9
Neutralizations	3.30	2.41	113.5	3.26	3.21	5.9
School commitment	3.31	3.91	-79.6	3.33	3.38	-6.3
Parental monitoring	3.63	4.23	-78.4	3.65	3.64	1.5
Unsupervised time	1.56	.86	78.6	1.51	1.39	13.5
Prosocial involvement	2.09	2.34	-20.1	2.12	2.04	6.7
Prosocial peers	2.61	3.60	-105.0	2.63	2.67	-4.4
Positive peer commitment	3.56	4.35	-65.2	3.59	3.62	-2.3
Peer delinquency	1.90	1.22	104.1	1.83	1.80	5.3
Negative peer commitment	2.44	1.62	76.5	2.37	2.24	12.1
Substance use	1.52	.21	69.7	1.27	1.29	-1.0
Delinquency	19.47	2.81	95.1	17.09	14.93	12.3

Note. Significant differences between the treated and control groups may exist when the standardized bias is greater than 20 in absolute value. T1 = Time 1.

treatment and control groups as a percentage of the average standard deviation (Rosenbaum & Rubin, 1985). In general, a bias statistic greater than 20% in absolute value may suggest that covariates are not balanced and that differences between the two groups influence the outcome; however, standards vary and others suggest lower (e.g., 10) or higher (e.g., 25) bias statistics (Harder, Stuart, & Anthony, 2010). The balance results presented in Tables 2, 3, and 4 show that for the unmatched samples, many of the T1 variables are significantly different between groups. For example, Table 2 shows that for the unmatched arrested versus no contact results, the bias statistics for 16 of the 19 covariates are much greater than 20%. After matching, these statistics are under 20, which suggests that bias associated with the T1 variables was successfully eliminated for the arrested versus no contact analysis.

Table 3 (no contact versus stopped) results show that although we achieved balance on nearly all of our T1 variables, the bias statistic for delinquency remains somewhat high after matching (S.B. = 20.2). We were able to achieve balance across all variables, including delinquency, using alternate matching estimators and found substantively similar results. Thus, it is unlikely that our

Table 3. Covariate Balance for Stopped Versus No Contact.

T1 variables	Unmatched			Matched		
	Stopped (n = 361)	No contact (n = 2,083)	Standardized bias (%)	Stopped (n = 361)	No contact (n = 2,082)	Standardized bias (%)
Age	12.01	11.73	38.3	12.01	11.96	7.4
Male	.64	.44	40.8	.64	.63	1.6
Race						
Black	.21	.15	15.8	.21	.21	-1.2
Hispanic	.46	.38	17.4	.46	.46	.7
Other	.14	.15	-3.6	.14	.14	-2.6
Impulsivity	2.98	2.68	38.5	2.98	2.97	1.6
Risk seeking	2.96	2.40	59.9	2.96	2.92	4.8
Anticipated guilt	2.47	2.71	-43.7	2.47	2.52	-8.8
Neutralizations	3.00	2.41	76.0	3.00	2.95	5.4
School commitment	3.52	3.91	-53.3	3.51	3.55	-4.9
Parental monitoring	3.93	4.23	-41.5	3.93	3.95	-2.9
Unsupervised time	1.37	.86	59.5	1.37	1.33	5.2
Prosocial involvement	2.35	2.34	.8	2.35	2.37	-1.6
Prosocial peers	3.01	3.60	-62.4	3.01	3.05	-4.3
Positive peer commitment	4.03	4.35	-28.7	4.03	4.08	-4.3
Peer delinquency	1.53	1.22	58.3	1.52	1.44	16.5
Negative peer commitment	2.01	1.62	43.5	2.01	1.91	10.9
Substance use	.66	.21	38.0	.66	.46	17.3
Delinquency	9.67	2.82	50.5	9.67	6.92	20.2

Note. Significant differences between the treated and control groups may exist when the standardized bias is greater than 20 in absolute value. T1 = Time 1.

results are affected by this slight imbalance. Table 4 presents the results for our arrested versus stopped matching analysis. As expected, there are fewer significant differences between the two groups before matching, but covariate balance was achieved on all variables after matching.

Matching Results

The propensity score-matching results for each of the comparisons are presented in Tables 5 through 7. Outcome variable means for treatment and control groups are displayed for the unmatched and matched samples. The difference represents the mean difference between the treated and control groups, which is referred to as the average treatment effect on the treated (ATT) for the matched samples. The average treatment effect (ATE) is the expected effect of treatment for any given individual randomly assigned to treatment or control conditions, regardless of actual treatment status. Although substantively similar, our analyses reveal some variation by level of contact. All reported significance levels are at the $p < .05$ level.¹⁰

Table 4. Covariate Balance for Arrested Versus Stopped.

T1 variables	Unmatched			Matched		
	Arrested (n = 170)	Stopped (n = 361)	Standardized bias (%)	Arrested (n = 170)	Stopped (n = 360)	Standardized bias (%)
Age	12.24	12.01	27.5	12.24	12.27	-3.4
Male	.68	.64	8.4	.68	.71	-5.8
Race						
Black	.22	.21	3.8	.22	.20	5.3
Hispanic	.51	.46	8.6	.51	.52	-3.0
Other	.11	.14	-7.3	.11	.12	-3.7
Impulsivity	3.08	2.98	13.1	3.08	3.17	-12.8
Risk seeking	3.25	2.96	30.4	3.25	3.36	-11.3
Anticipated guilt	2.17	2.47	-48.5	2.17	2.17	-.5
Neutralizations	3.30	3.00	38.4	3.30	3.37	-9.2
School commitment	3.31	3.52	-25.8	3.31	3.22	10.5
Parental monitoring	3.63	3.93	-37.2	3.63	3.70	-9.6
Unsupervised time	1.56	1.37	20.4	1.56	1.73	-18.3
Prosocial involvement	2.09	2.35	-20.7	2.09	2.13	-2.9
Prosocial peers	2.61	3.01	-43.5	2.61	2.58	3.2
Positive peer commitment	3.56	4.03	-37.5	3.56	3.70	-10.8
Peer delinquency	1.90	1.52	50.3	1.90	1.92	-3.5
Negative peer commitment	2.44	2.01	35.4	2.44	2.44	.1
Substance use	1.52	.66	42.0	1.52	1.37	7.6
Delinquency	19.47	9.67	46.7	19.47	19.37	.5

Note. Significant differences between the treated and control groups may exist when the standardized bias is greater than 20 in absolute value. T1 = Time 1.

Table 5. Matching Results for Arrested Versus No Contact.

	Unmatched			Matched ATT			
	Arrested (n = 170)	No contact (n = 2,083)	Difference	Arrested (n = 164)	No contact (n = 2,083)	Difference	ATE
Anticipated guilt	1.88	2.51	-.62*	1.90	2.15	-0.25*	-0.44
Neutralizations	3.38	2.66	.72*	3.39	3.07	0.32*	0.42
Negative peer commitment	2.74	1.91	.82*	2.72	2.22	0.50*	0.56
Delinquency	27.72	5.00	22.71*	26.83	12.01	14.82*	15.34

Note. Tables represent group means and mean differences before and after matching. Significance levels are based on *t*-tests for the unmatched sample; for the matched sample, significance levels are calculated using bootstrapped standard errors. ATT = average treatment effect on the treated; ATE = average treatment effect.

**p* < .05.

The arrested versus no contact results reported in Table 5 show that before matching, arrested youth reported less anticipated guilt (difference = -.62), more agreement with neutralization techniques (difference = .72), greater

Table 6. Matching Results for Stopped Versus No Contact.

	Unmatched			Matched ATT			
	Stopped (<i>n</i> = 361)	No contact (<i>n</i> = 2,083)	Difference	Stopped (<i>n</i> = 361)	No contact (<i>n</i> = 2082)	Difference	ATE
Anticipated guilt	2.15	2.51	-.36*	2.15	2.32	-0.17*	-0.21
Neutralizations	3.21	2.66	.55*	3.21	2.96	0.25*	0.34
Negative peer commitment	2.36	1.91	.45*	2.36	2.05	0.31*	0.28
Delinquency	13.16	5.00	8.16*	13.16	8.02	5.14*	4.36

Note. Tables represent group means and mean differences before and after matching. Significance levels are based on *t*-tests for the unmatched sample; for the matched sample, significance levels are calculated using bootstrapped standard errors. ATT = average treatment effect on the treated; ATE = average treatment effect. **p* < .05.

commitment to deviant peers (difference = .82), and more delinquency (difference = 22.71). The matched ATT results reveal a similar pattern: Arrest is associated with less anticipated guilt (ATT = -.25), greater acceptance of neutralization techniques (ATT = .32), greater negative peer commitment (ATT = .50), and more delinquency (ATT = 14.82). Although the differences are significant before and after matching, the magnitude of the effect of arrest is smaller after adjusting for factors associated with police contact and delinquency. For example, while the unmatched results show that youth who were arrested reported nearly 23 more delinquent acts than those with no contact, the matched results indicate that this difference is just under 15 after adjusting for selection bias. Thus, had we not matched youth on their propensities to experience police contact, the effect of arrest on delinquency would have been overestimated. The ATE shows that under randomized conditions, as we hypothesized, arrest would decrease guilt (ATE = -.44) and increase agreement with neutralization statements (ATE = .42), negative peer commitment (.56), and delinquency (ATE = 15.34).

Table 6 provides results for the stopped versus no contact comparison. Here, we see a similar pattern of results compared with the arrested versus no contact analysis. In the unmatched sample, being stopped decreases guilt and increases acceptance of neutralizations, negative peer commitment, and delinquency. The ATT results show that after controlling for the propensity to be stopped, these differences, though smaller in magnitude, remain significant. When the stopped versus no contact-matched ATT results are compared with those in the arrested versus no contact analysis (Table 5), the magnitude of the effect of being arrested appears slightly greater than for being stopped. For instance, compared to having no police contact, arrest is associated with a 14.82 increase in delinquency, but being stopped is associated with a smaller increase at 5.14. To determine whether arrest has

Table 7. Matching Results for Arrested Versus Stopped.

	Unmatched			Matched ATT			ATE
	Arrested (n = 170)	Stopped (n = 361)	Difference	Arrested (n = 170)	Stopped (n = 360)	Difference	
Anticipated guilt	1.88	2.15	-.27*	1.88	2.00	-0.12*	-0.15
Neutralizations	3.38	3.21	.17*	3.38	3.30	0.08	0.03
Negative peer commitment	2.74	2.36	.38*	2.74	2.54	0.20	0.21
Delinquency	27.72	13.16	14.56*	27.72	14.77	12.95*	10.49

Note. Tables represent group means and mean differences before and after matching. Significance levels are based on *t*-tests for the unmatched sample; for the matched sample, significance levels are calculated using bootstrapped standard errors. ATT = average treatment effect on the treated; ATE = average treatment effect.

**p* < .05.

negative effects above and beyond what stopped youth experience, we turn to the arrested versus stopped results in Table 7. These findings show that before adjusting for the propensity to experience arrest, mean differences between the stopped and arrested groups are significant for each of the four outcomes. The matched results, however, indicate that being arrested, compared to being stopped, has a significant effect only on guilt (ATT = -.12) and delinquency (ATT = 12.95).

The findings from the three matching analyses presented here are consistent with labeling, rather than deterrence theory. Regardless of the comparison group, our results show that being stopped and being arrested have detrimental effects on attitudes and behavior even after adjusting for characteristics associated with police contact and delinquency. Together, the results from each analysis suggest that the effects of police contact become more pronounced as contact severity increases. These findings have considerable implications for official and unofficial reactions to delinquency, which are discussed in the following section.

Discussion and Conclusion

While extant research shows mixed findings with regard to the effects of justice system contact, findings generally support a labeling perspective. Early labeling research focused, in large part, on the deleterious effects of court involvement or sanctions and findings were often tied to support for diversion programs (Ageton & Elliott, 1974; Fisher & Erickson, 1973; Foster et al., 1972; Klein, 1974; Schur, 1973). Although diversion programs and labeling theory fell out of favor, a recent resurgence in labeling research has revealed short- and long-term consequences of justice system exposure (e.g.,

Bernburg & Krohn, 2003; Bernburg et al., 2006; Lopes et al., 2012). Yet, despite these findings, policies are often based on principles of deterrence. Policing policies in New York City rely on aggressive policing and “stop, question, and frisk” procedures to maintain order and prevent crime (Jones-Brown et al., 2010) and the number of police officers in schools continues to grow (Petteruti, 2011). If the justice system’s involvement in early intervention programs increases, as some suggest it should (e.g., Farrington, 2012; Welsh, 2012; Wright et al., 2012), then youth may come into contact with the juvenile justice system at even earlier ages.

The current study adds to the labeling versus deterrence debate by identifying the negative impact that not only arrest but also simply being stopped by the police has on delinquent behavior and attitudes. The use of propensity score matching reduces the likelihood that our results are being driven by preexisting differences, a problem that may plague much of the existing labeling research (but see, for example, Lopes et al., 2012; McAra & McVie, 2007). It is important to note that the use of propensity score matching adjusts for selection only on the covariates included in the model and although we include a range of covariates associated with delinquency and police contact, it is possible that unmeasured selection bias remains. Nonetheless, our results are consistent with the bulk of labeling research and indicate that youth who have been stopped or arrested report significantly less anticipated guilt, greater agreement with neutralization techniques, greater commitment to delinquent peers, and higher levels of delinquency than youth with no police contact. In addition, our findings show that the negative consequences of police contact are compounded for arrested youth; subsequent to arrest, they report less anticipated guilt and more delinquency compared with stopped youth.

If simply being stopped and questioned has deleterious consequences, policing practices may inadvertently contribute to higher levels of delinquency even before youth have reached the stages of formal processing. This suggests that while diversion programs may have been unfairly or prematurely dismissed, the possible benefits of such programs occur after youth have already experienced the negative effects of initial police contact. To this end, it is important to closely examine aggressive delinquency prevention and intervention strategies. Though policing policies such as those used in New York City may be responsible for some crime decline, they appear to have negative consequences, particularly for racial and ethnic minority residents who experience disproportionate contact (Jones-Brown et al., 2010; Rosenfeld & Fornango, 2012; Rosenfeld, Fornango, & Rengifo, 2007; Zimring, 2012). Similar problems may stem from increased police officers’ presence in schools, which leads to heavier reliance on

officers to deal with problem behaviors and increases in the number of school-based arrests, particularly for minor offenses (Petteruti, 2011). Like the aggressive policing strategies in New York City, increasing the number of police officers in schools disproportionately affects minority youth (Irwin et al., 2013; Petteruti, 2011). Such policies likely decrease perceptions of police legitimacy among citizens who feel that they, or others, have been stopped without justification or treated unfairly. This has implications for deviance amplification, as less perceived legitimacy is associated with defiance or less compliance with the law (Sherman, 1993; Tyler, 1990).

Still, police officers cannot turn away when youth are engaging in disruptive or illegal behavior. As such, strategies must be taken to minimize the negative effects of police contact. Labels are most effective when applied publicly; that is, the labeled individual is most likely to experience social exclusion, changes in identity, and increased involvement with deviant others when the label is known to the public or involves a successful status degradation ceremony (Garfinkel, 1956; Lemert, 1951; Paternoster & Iovanni, 1989). Though we do not address the public nature of the label or the extent to which youth feel labeled or stigmatized in this study, we find that police contact affects procrime attitudes by decreasing anticipated guilt and increasing agreement with neutralization statements, which may suggest that these youth are beginning to take on increasingly deviant identities. Therefore, officers should carefully assess the conditions under which they stop and question youth to reduce the negative effect that contact may have on the development of a deviant identity. It may well be advisable to avoid aggressive questioning of youth in areas where he or she may be publicly labeled if other scenarios are available. Our findings also show that youth who experience police contact report increased commitment to deviant peers, which may be the result of exclusion from prosocial others. It is important that youth are not isolated after experiencing police contact, and family members, criminal justice actors, and the community should take steps to ensure that youths' prosocial bonds are not attenuated following police contact (e.g., Braithwaite & Mugford, 1994).

The current study reveals negative consequences of police contact for our sample of youth, but a better understanding of the conditions or mechanisms surrounding deviance amplification is necessary. As others suggest, the effects of police contact may vary by factors such as age, race, and socioeconomic status (Ageton & Elliott, 1974; Bernburg & Krohn, 2003; Jensen, 1972) or perceived legitimacy and reintegrative efforts (Sherman, 1993). In addition, recent research shows that the short- and long-term effects of police contact are mediated by educational, employment, and relationship opportunities (Bernburg & Krohn, 2003; Bernburg et al., 2006; Lopes et al., 2012).

Future research should explore these potential mediating and moderating mechanisms to further explicate the labeling process and allow more precise policy conclusions to be drawn.

It is particularly disconcerting that simply being stopped by the police has negative consequences. If aggressive policies continue in our schools and communities, there is a greater likelihood that youth will encounter the police. Furthermore, schools working in collaboration with the justice system to provide early intervention for problem youth may lead to increased justice system contact. Research suggests that the harms associated with these aggressive strategies may outweigh the benefits (Jones-Brown et al., 2010; Petteruti, 2011; [Rosenfeld & Fornango, 2012](#)). Yet, not all police–citizen interactions are negative, and research shows that some interactions may improve perceptions of the police ([Esbensen et al., 2012](#); [Mazerolle, Antrobus, Bennett, & Tyler, 2013](#)). For example, the Gang Resistance Education and Training (G.R.E.A.T.) Program is taught by uniformed police officers in the classroom; involvement in the G.R.E.A.T. Program is linked to more prosocial attitudes and behaviors, including more positive attitudes toward the police ([Esbensen et al., 2012](#)). Therefore, future research should determine the effect that positive interactions and attitudes toward the police have on the labeling process, particularly where perceived legitimacy is concerned ([Sherman, 1993](#); [Tyler, 1990](#)).

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Notes

1. We use the term *police contact* throughout the article to refer to encounters with the police, such as being stopped and questioned, that involve the formal control of citizens. Being stopped or arrested implies police exercise of power and/or discretion and it is this sort of interaction that is likely to result in deviance amplification, not simply exposure to the police.
2. Other evidence suggests that diversion programs are effective when implemented with fidelity (e.g., Bynum & Greene, 1984; Esbensen, 1984; Latessa, Travis, & Wilson, 1984).
3. It should be noted that while Esbensen et al. (2008) reported a 79% consent rate, the addition of two schools to the evaluation after the publication of that article resulted in the 78% overall consent rate reported here.
4. We chose to include the posttest for Time 1, rather than the pretest, due to the shorter lag between the posttest and Wave 3. This allows us to capture attitudes and behaviors most temporally relevant to when youth reported police contact, which was first measured at Wave 3.
5. Respondents excluded from the analyses are significantly different on several T1 characteristics. They are more likely to be Black, reported higher levels of impulsivity and risk seeking, less parental monitoring, more unsupervised time with friends, and higher levels of substance use. They also reported less involvement in prosocial activities, fewer prosocial peers, and less commitment to prosocial peers, but greater commitment to deviant peers and more delinquent peers. In addition, excluded youth reported greater use of neutralization techniques, less guilt, and more delinquency at T1 and T3. As expected, excluded youth were also more likely to report contact with the police at T2.
6. Some youth experienced police contact at T3, but not at T2. While this could overestimate the effect of T2 police contact on our outcomes, additional analyses confirm that our results do not change when we collapse T2 and T3 contact into a single measure.
7. For some youth, race and ethnic identity are not static. That is, we found some youth would report that they were Hispanic at one point in time and then at another point in time they would clarify that they were bi-racial (Hispanic and African American). To address such situations, we created a "consensus" race/ethnicity variable that identifies the youth as the race/ethnicity that was reported with the greatest frequency across the study period.
8. To ensure robustness, kernel density, local linear, nearest neighbor 1-to-1 (with replacement), and nearest neighbor 1-to-10 (with replacement), matching estimators were used. All results are substantively similar, but the kernel density results are reported due to higher quality of matches.
9. This resulted in the loss of six treatment cases for the arrested versus no contact analysis, one control case in the stopped versus no contact comparison, and one control case in the arrested versus stopped analysis. As expected, unmatched cases had high propensities to experience the treatment.

10. For unmatched results, significance levels are based on two-tailed independent samples *t*-tests. Matched ATT significance levels are calculated using bootstrapped standard errors with 50 replications, as recommended by Caliendo and Kopeinig (2008).

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Author Biographies

Stephanie A. Wiley is a doctoral candidate in the Department of Criminology and Criminal Justice at the University of Missouri–St. Louis. Her research interests include juvenile delinquency, substance abuse, and biosocial criminology.

Finn-Aage Esbensen is the E. Desmond Lee professor of Youth Crime and Violence and also serves as chair of the Department of Criminology and Criminal Justice at the University of Missouri–St. Louis.