Challenging Assumptions: A Genetically Sensitive Assessment of the Criminogenic Effect of Contact With the Criminal Justice System

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Abstract

A key assumption underlying various components of criminological thought is the criminogenic effect of involvement with the criminal justice system. Prior assessments of this effect, however, have been mixed and all are subject to potential genetic confounding. In the current study, we employ twin difference scores using both monozygotic and dizygotic twins to isolate the effect of involvement with the criminal justice system on future criminal behavior. The findings illustrate null associations between a variety of interactions of the criminal justice system and subsequent criminal offending. The study illustrates the continued ineffectiveness of the standard social science methodological approach to assessing criminology's main assumptions.

Keywords

criminogenic effects, contact with CJS, labeling theory, twin difference, genetic confounds

Introduction

A widely held assumption within criminology is the criminogenic effect of official contact with the criminal justice system (hereafter, CJS; e.g., Paternoster & Iovanni, 1989; Wiley, Slocum, & Esbensen, 2013). Scholars point to a variety of mechanisms from social stigma to an accumulation of consistent rejections from conventional society to explain continued and increased criminality after official interaction with the CJS. Layered within this assumption is that contact with the CJS is the causal factor

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that can lead to the unintended consequence of continued antisocial behavior. Indeed, the argument is occasionally made that if not for the interjection of an official apparatus of the CJS an offender may not have engaged in continued criminal behavior.

While the empirical literature assessing the criminogenic effect of interaction with the CJS appears to be mixed (e.g., Nagin & Snodgrass, 2013; Smith & Paternoster, 1990; Wiley et al., 2013), key to the discussion of offending over the life-course is the effect of individual differences. Such factors may provide the initial propensity toward offending, an increased likelihood of contact with the CJS, and an increased likelihood of continued offending. Furthermore, when assessing the potential influence of individual differences, scholars have called for a nuanced recognition of the variety of both genetic and nongenetic factors that affect offending and contact with the CJS (e.g., Barnes, Beaver, & Boutwell, 2011; Beaver, 2011; Nedelec, Park, & Silver, 2016). However, these calls have gone relatively unanswered in the criminological literature assessing labeling theory. Consequently, the current study addressed the potential criminogenic effect utilizing longitudinal analyses of genetically sensitive data. The analyses accounted for the additive influence of genetic factors, shared nongenetic factors, and a variety of nonshared environmental factors by employing the twin-discordance methodology. What follows is a discussion of the potential criminogenic effect of contact within the CJS as situated primarily within labeling theory as well as a discussion of the potential manner in which this perspective is threatened by genetic confounding.

Labeling Theory

As a prominent criminological perspective, labeling theory has guided a substantive quantity of empirical literature on primary and secondary deviance (Lilly, Cullen, & Ball, 2015). Various scholars have adapted the principles of the labeling perspective to understand a variety of criminological issues (Lemert, 1951; Link, 1982; Sampson & Laub, 1995). Consistent across these adaptations is the fundamental proposition that criminal behavior is a function of the internalization of pro-criminal values associated with the formal sanctioning of deviant behavior (Tannenbaum, 1938). Generally, the internalization of pro-criminal values results from the stigmatization associated with the formal physical and psychological punishment of nonnormative behaviors (Tannenbaum, 1938).

An oft-cited adaptation of labeling theory was provided by Lemert (1951) who argued that criminal behavior can be demarcated into two forms of deviance, primary deviance and secondary deviance. Lemert (1951) opined that individuals engaging in primary deviance—the onset of criminal behavior—rarely consider themselves procriminal or internalize the pro-criminal identity associated with the formal punishment of nonnormative behaviors. Criminal continuity, or secondary deviance, Lement argued, results from the internalization of a pro-criminal identity in response to formal punishment associated with primary deviance. Lemert (1951), and others since (e.g., Braithwaite, 1989), claimed that various psychological punishments (e.g., labeling and stereotyping) are substantive when reinforcing the internalization of a pro-criminal

identity. The internalization of the deviant status further imbeds the individual in non-conformity, generating continued involvement in criminal behavior.

While Lemert's (1951) theoretical perspective informed scholars of the potential negative effects of criminal stigmatization, the work of Becker (1963), Erikson (1966), and Kitsuse (1964) positioned labeling theory at the forefront of the criminological paradigm during the second half of the 20th century (Lilly et al., 2015). The combined work of these scholars argued that the stigmatization associated with deviant behavior reinforces group mentality, where individuals who engaged in criminal activity are perceived differently than prosocial individuals (Becker, 1963; Erikson, 1966; Kitsuse, 1964). Generally, prosocial individuals perceive deviant individuals as amoral, deficient in their desire to obey societal norms. Furthermore, the argument continues, the group mentality encourages prosocial individuals to isolate themselves from any potential deviant peers, resulting in various deviant peer networks. In combination, the stigmatization and isolation resulting from formal sanctioning of deviant actions encourages the internalization of a deviant status (Becker, 1963).

Contemporary labeling theorists remain consistent with their sentiments toward society's stigmatization of individuals who engage in criminal activity. Specifically, as suggested by various scholars (e.g., Bernburg, 2009; Link, Cullen, Frank, & Wozniak, 1987; Paternoster & Iovanni, 1989; Sampson & Laub, 1995; Wiley et al., 2013), contact with the CJS generates pro-criminal stigmatizations, diminishes access to prosocial opportunities (e.g., employment, housing, noncriminal peer groups), and encourages contact with pro-criminal peers. However, as noted above, the empirical validity of these assertions remains mixed due, in part, to the various analytical strategies employed in the literature.

The Unintended Consequences of CJS Intervention— Examples of Recent Empirical Assessments

Contemporary scholarship assessing the empirical validity of labeling theory has evolved over time from basic correlation and regression techniques to more sophisticated quasi-experimental and experimental approaches. For example, regression analyses by Smith and Paternoster (1990) exhibited a criminogenic effect of court processing on delinquency. However, after accounting for potential selection effects the association no longer held.

Although pragmatically difficult to conduct, experimental approaches have been employed to assess labeling theory's claims. For example, Nagin and Snodgrass (2013) capitalized on Pennsylvania's practice of county-level randomization of cases to judges and employed a natural experiment approach. The authors compared cross-judge variation in punitiveness as exhibited in the likelihood of incarceration versus noncustodial sentences to variation in reoffending (measured by rearrest). Overall, the findings indicated a lack of a criminogenic effect of incarceration on rearrest. Loeffler (2013) also employed data from the Pennsylvania court system to conduct a similar natural experiment. The results of his analyses echoed Nagin and Snodgrass (2013) as the findings revealed a null association between imprisonment and recidivism as well as

unemployment rates (another negative outcome of CJS intervention purported to be causal in labeling theory). However, as recent scholarship argues, the effects of official intervention such as imprisonment perhaps only manifest when accounting for a variety of labeling processes and mechanisms. To account for such factors, other approaches such as quasi-experimental methods have been employed in recent analyses.

Published in *Criminology*, Wiley and colleagues's (2013) examination of the association between criminal justice contact and secondary deviance presents a notable contribution to the labeling perspective. Using data collected during the Gang Resistance Education and Training (G.R.E.A.T.) study ($N = 2,127$), the authors assessed the differences in deviant outcomes between three groups of adolescents: youth who had experienced an arrest, youth who had experienced a stop by police, and youth who never had contact with the CJS. Furthermore, Wiley and colleagues (2013) assessed these differences using propensity score matching (PSM) and multiple mediator analysis over a 4-year period. The results indicated that participants who had ever been stopped by the police or had experienced an arrest had lower levels of school commitment and prosocial activities, higher levels of poor grades, exclusion from prosocial peers, less anticipated guilt, greater use of neutralizations, higher peer delinquency, more negative peer commitment, and greater delinquency (relative to respondents who had not experienced such contact with the CJS). In addition, the mediator analyses indicated (in general) that the association between contact with the CJS and continued offending was mediated by a number of theoretically relevant variables derived from labeling theory. Overall, the authors concluded that the results provided substantive support for the secondary deviance hypothesis (i.e., the criminogenic effect of contact with the CJS).

Genetic Confounding and Claims of Causation

Although the theoretical reasoning provided by Wiley and colleagues (2013) is compelling, empirical evidence has outlined two critiques that deteriorate the validity of the casual connection between labeling and antisocial behavior (e.g., Hirschi, 1975; Smith & Paternoster, 1990; Tittle, 1980). First, as established by scholarship within the life-course paradigm, individuals often display antisocial tendencies long before the association between criminal justice contact and secondary deviance can be established (Farrington, 2005; Moffitt, 1993; Piquero, Farrington, & Blumstein, 2007; Smith & Paternoster, 1990). Second, labeling theorists often disregard the potential influence of genetic and other biological predispositions on the establishment of antisocial phenotypes that influence primary deviance and criminal continuity (e.g., Jackson & Hay, 2013; Kavish, Mullins, & Soto, 2016; Moore & Tangney, 2017; Ray & Downs, 1986; Wiley et al., 2013). Furthermore, while methods such as PSM are employed to address self-selection, the required reliance on matching participants on only *observed* environmental variables can limit the ability to infer causality. Fortunately, advances in disciplines outside of criminology, such as behavioral and molecular genetics, have provided methods to overcome such limitations. While a variety of genetically sensitive methodologies exist, perhaps the most relevant to the current discussion is the twin-difference score approach.

Twin-Difference Scores (A Genetically Sensitive Methodology)

Scholarship within criminology is generally achieved through the employment of standard social science methodologies (SSSMs; Nedelec & Beaver, 2014). While SSSMs are practical, they rely on various assumptions that cannot be satisfied when discussing antisocial phenotypes (Wright & Boisvert, 2009). As empirical evidence has suggested, the most frequently violated assumption is the blank slate assumption (Pinker, 2003). Specifically, recent scholarship has indicated that a variety of antisocial phenotypes are influenced by biological predispositions as measured by additive genetic factors (e.g., Barnes et al., 2011; Nedelec et al., 2016; Nedelec, Richardson, & Silver, 2017; Polderman et al., 2015). More specifically, various lines of scholarship, including multiple meta-analyses, indicate that about 50% of the variance in antisocial behaviors is due to heritability (the component of phenotypic variance which is attributed to variance in genetic factors), while the majority of the remaining variance is due to nonshared environmental factors (unique experiences that are not shared by twins or siblings and serve to increase within-pair differences; note that measurement error is also captured by this component; Barnes et al., 2014). Phenotypic variance is also affected by a third nongenetic component, called the shared environment (those nongenetic factors that are shared within a sibling pair and serve to decrease within-pair differences), although this component appears to account for relatively little in terms of variance in antisocial behavior (Barnes et al., 2014).

To isolate nongenetic influences on a variery of phenotypes, twin-difference methodologies are often employed and are highly regarded among behavioral geneticists (e.g., Asbury, Dunn, Pike, & Plomin, 2003; Vitaro, Brendgen, & Arseneault, 2009). Specifically, as twins share either 100% of their distinguishing DNA (monozygotic [MZ] twins) or 50% of their distinguishing DNA (dizygotic [DZ] twins) and 100% of the shared environment, only nonshared environmental factors can account for phenotypic discordance (Nedelec et al., 2017; Vitaro et al., 2009).[1] Consequently, twin-difference methodologies address the critiques of labeling theory scholarship because researchers can statistically account for both the unobserved *and* observed factors potentially influencing self-selection (Asbury et al., 2003; Nedelec et al., 2017). Furthermore, twin-difference methodologies are superior to PSM as they allow scholars to account for the unobserved genetic predispositions of the participants whereas PSM must rely on observed variables. While the twin-difference approach is similar to a fixed-effects model in a longitudinal study (where a respondent serves as their own counterfactual), the twin-difference approach employs a co-twin as their twin's counterfactual.[2]

The Current Study

Against this backdrop, the current study assesses the influence of the official labeling process on criminal behavior using a longitudinal, genetically sensitive design. More specifically, employing a twin-difference score approach the current study examines the nonshared environmental influence of varying levels of involvement with the CJS

on offending during a large component of adulthood. Thus, the current study adds to the literature in three distinct ways. First, using the twin-difference methodology provides a control for the confounding effect of genetic factors on the association between involvement with the CJS during adolescence and offending during adulthood. Second, by employing a longitudinal analytical strategy that spans almost 15 years and multiple sections of the life-course, the current study is able to account for limitations associated with cross-sectional designs or longitudinal designs which only range over 3 or 4 years and focus on only one section of the life-course. Third, the current study also includes a wide array of control variables measured during adolescence derived from theory and past research (e.g., Wiley et al., 2013). To conduct these analyses, the current study examines a subset of MZ and same-sex DZ twins drawn from the National Longitudinal Study of Adolescent to Adult Health (Add Health).

Method

Data

The Add Health is a nationally representative sample of over 20,000 American youth who were enrolled in Grades 7 through 12 during the 1994-1995 school year. Detailed information regarding the sampling procedures can be found elsewhere (Harris, Halpren, Smolen, & Haberstick, 2006; Harris et al., 2009). Briefly, the Add Health study includes data from participants at four time points.[3] The first and second waves of data were conducted during adolescence when the majority of respondents were between 14 and 18 years of age. Approximately 5 to 6 years after the second wave of data collection, the third wave took place when most respondents were in their early 20s. The fourth wave of data occurred 6 years later when all respondents were at or beyond the age of 25 years and about 20% were 30 years or older. The data employed in the current study are derived from all four waves of data.

Relevant to the current study, the first wave of data collection also included an oversampling of twins, siblings, and other kinship pairs (Harris et al., 2006). Nested within these oversampled kinship pairs were 307 MZ twin pairs and 452 DZ twin pairs (Harris et al., 2006). After removing different-sex DZ twin pairs and dropping cases with no valid data on the measures of interest, the analytical sample of the current study is comprised of 282 MZ twin pairs (50% male pairs; 564 individuals) and 244 same-sex DZ twin pairs (53% male pairs; 488 individuals).

Measures

Independent variables (IVs)—Involvement with the CJS. At Wave 3, respondents were asked a number of questions regarding their involvement with the CJS. Four of these questions were used to create the involvement with the CJS measures. First, respondents were asked, "How many times have you been stopped or detained by the police for questioning about your activities (do not count minor traffic violations)?". Responses to this item were dichotomized to create the variable *Ever stopped by*

police, where 0 = never stopped by police and 1 = stopped by police one or more times. Second, respondents who indicated that they had been stopped by police were asked, "How old were you the first time this [being stopped by police] happened?". This item was also dichotomized to create the *Stopped by police as a juvenile* variable where 0 = never stopped by police or first stopped by police as an adult (i.e., 18 years or older) and 1 = stopped by police for the first time before the age of 18. Third, respondents were asked if they had "ever been arrested or taken into custody by the police". This variable was coded as 0 = no and 1 = yes to form the *Ever arrested* item. Finally, those respondents who indicated that they had been arrested were asked, "How old were you the first time this [being arrested] happened?". This item was dichotomized to create the *Arrested as a juvenile* item where 0 = never arrested or first arrested as an adult (i.e., 18 years or older) and 1 = first arrested before the age of 18.

Dependent variables—Adult criminal behavior. To assess the long-term criminogenic effect of involvement with the CJS, the outcome measures are derived from Waves 3 and 4. As a reminder, Waves 3 and 4 were conducted while the respondents were, on average, 22 and 29 years of age, respectively. Thus, the outcome measures in the current study represent a substantive segment of early adulthood. During Waves 3 and 4, respondents were asked a number of questions regarding their involvement in criminal behavior over the past year. Thirteen items from Wave 3 and 13 items from Wave 4 addressing both property and violent offending were employed to create two separate index measures of adult criminal behavior: *Wave 3 offending* and *Wave 4 offending* (see Online Appendix A for a complete list of items used to create these items). For each of these measures, an average score was created (where respondents were required to have valid scores on a least 50% of the constituent items).[4] Reliability analyses illustrated moderate to strong internal consistency for both of the indexes (*Wave 3 offending*: α = .73; *Wave 4 offending*: α = .69).[5] Importantly, these items have been employed by past researchers using the Add Health (e.g., Barnes et al., 2011).

To assess a potential differential influence of involvement with the CJS as a juvenile on different types of adult criminal behavior, the measures of offending were categorized as nonserious and serious offending items for both Waves 3 and 4. Following a similar procedure as the overall offending measures, five nonserious offending items at Wave 3 (e.g., damaging property less than $50 (USD), stealing items less than $50 (USD), buying or selling stolen property; see Online Appendix A) were averaged to create the *Wave 3 nonserious offending* measure (α = .54). The same procedure was followed to create the *Wave 4 nonserious offending* variable (five items; α = .35). Similarly, eight items tapping serious offending at Wave 3 (e.g., breaking and entering, using a weapon in a fight, pulling a knife or gun on someone; see Online Appendix A) were average to create the *Wave 3 serious offending* measure (α = .64) and the exact process was followed to create the *Wave 4 serious offending* variable (eight items; α = .67).

Control variables. Following recent analyses of the influence of involvement with the CJS on offending behaviors (Wiley et al., 2013), the current study includes a number

of control variables tapping a variety of aspects of adolescence (see Online Appendix A for a list of all items included in the creation of the control variables). Each of the control variables was derived from both Waves 1 and 2 to include all of the adolescent years. First, to account for involvement in delinquency during adolescence, 34 items from Waves 1 and 2 were summed and averaged to create an average *juvenile delinquency* index ($\alpha = .89$). Second, a measure of the number of *delinquent peers* was created by summing six items asking respondents to indicate the frequency of smoking, drinking, and marijuana use among their three best friends at both Wave 1 and Wave 2 (i.e., three items from each wave). Third, a measure tapping *social support* was generated by summing 13 items from Waves 1 and 2 ($\alpha = .83$). Fourth, five different items tapping parental practices and relationship with parents were derived from both Waves 1 and 2: *maternal attachment* ($\alpha = .67$), *maternal involvement* ($\alpha = .63$), *maternal disengagement* ($\alpha = .88$), and *parental permissiveness* ($\alpha = .75$). A measure tapping the respondents' level of *attachment to school* ($\alpha = .82$) derived from both Waves 1 and 2 was included to control for school commitment. Finally, a measure tapping *low self-control* was constructed by standardizing and then summing 43 different items from Waves 1 and 2 and generating an average score of low self-control for each respondent ($\alpha = .80$).

Analytical Strategy

To isolate involvement with the CJS as a component of the nonshared environmental effect on future offending, the current study employs the twin-difference score methodology. The approach requires two preliminary steps. First, within each twin pair a random designation of Twin 1 was applied to one twin after which the co-twin was designated as Twin 2. Second, to create a difference score on each variable, Twin 2's score on each item was subtracted from Twin 1's score on the corresponding item.

The analyses in the current study followed a series of interrelated steps. First, the difference scores for each variable in the study were constructed following the steps outlined above. Second, descriptive statistics for the original (i.e., untransformed) variables and the difference scores were generated. As part of these descriptive statistics, we calculated the cross-twin (intraclass) correlations for each variable using the original variables. The cross-twin correlation provides an indication of the potential nonshared environmental influence on the variable of interest (Beaver, 2008; Nedelec et al., 2016). In other words, if the cross-twin correlation is $r < 1.00$, it can be assumed that there is a potential nonshared environmental influence on the outcome in question. The third step of the analyses entailed estimating several multivariate ordinary least squares (OLS) regression models for each outcome variable (i.e., the six measures of adult criminal behavior). More specifically, for each outcome variable at both Waves 3 and 4, we estimated two separate OLS regression models: (a) a baseline model wherein each adult criminal behavior measure was regressed on a single involvement with the CJS measure, and (b) a full model wherein each adult criminal behavior measure was regressed on each successive CJS item as well as all of the adolescent control variables. Thus, we estimated 48 different OLS regression models

in total examining the potential long-term effect of involvement in the CJS during adolescence on adult criminal behavior. Importantly, given that these regression models used twin-difference scores and treated the twin pair as the unit of analysis, the results are interpreted accordingly. Finally, all of the multivariate analyses were conducted using a single-entered data set and employed robust standard errors to protect against the potential influence of clustering effects (Beaver, 2008).

Results

The analyses began with the production of summary statistics for all study variables, which are displayed in Table 1. As indicated, the average age of the analytical sample was about 16 years old at Wave 1 ($SD = 1.64$; minimum, maximum: 12, 20) and was comprised of about 52% male ($SD = .50$) and 63% ($SD = .48$) White respondents. Note that these descriptors are provided for information purposes only as the twin-difference methodology controls for the effects of these and all shared characteristics within-twin pairs. A crucial assumption that must be met for the twin-difference methodology to be employed is sufficient within twin pair variability on the measures included in the analyses. Examination of the variation in the difference scores illustrated in Table 1 indicates variability across all measures. In addition, the intraclass (cross-twin) correlations indicate magnitudes of associations that were all $r < 1.00$. As such, the bivariate analyses (intraclass correlations) provided preliminary evidence of a nonshared environmental influence on the variance in these measures. The remaining analyses focus on assessing whether involvement with the CJS represents a component of the nonshared environmental effect on variance in criminal behavior in adulthood.

The next step in the analyses was the estimation of the multivariate models (OLS regression).[6] Two reminders are worth repeating here. First, for each measure of criminal behavior in adulthood, two OLS regression models were estimated: a baseline model wherein the outcome measure (adult criminal behavior) was regressed on a single item tapping contact with the CJS, and a full model wherein the outcome measure was regressed on the contact with the CJS item and all of the covariates (i.e., control variables). Second, given that the twin pair is the unit of analysis in the twin-difference score method, the coefficients resulting from the regression analyses are to be interpreted accordingly (i.e., the coefficient represents the within-twin pair difference on the outcome given the within-twin pair difference on the covariate).

For the sake of brevity and ease of interpretation, the results are displayed in four different figures.[7] Within each figure, there are four panels corresponding to the four measures of contact with the CJS. Within each panel, the results of four models are displayed (baseline and full models for both Waves 3 and 4) and include the point estimates and 95% confidence intervals (CIs) of the unstandardized regression coefficient estimated for the relevant measure of contact with the CJS (i.e., the IV of interest).

Figure 1 presents the results of the first set of multivariate analyses assessing the effect of contact with the CJS on overall offending in Wave 3 and in Wave 4. As

Table 1. Descriptive Statistics and Cross-Twin Correlation for Original and Difference Score Variables for the Analytical Sample of Twins.

Variable	Original			Difference score				
	M	SD	Minimum, Maximum	M	SD	Minimum, Maximum	X-twin r	n (pairs)
Involvement with CJS								
Ever stopped by police[a]	0.18	0.39	0, 1	0.02	0.48	−1, 1	.40*	392 (196)
Stopped as a juvenile[a]	0.10	0.29	0, 1	0.00	0.36	−1, 1	.47*	394 (197)
Ever arrested[a]	0.10	0.30	0, 1	−0.03	0.37	−1, 1	.46*	394 (197)
Arrested as a juvenile[a]	0.04	0.19	0, 1	−0.01	0.26	−1, 1	.44*	396 (198)
Adult criminal behavior								
Wave 3 offending	0.05	0.13	0, 3	0.01	0.17	−1.00, 1.39	.13*	392 (196)
Wave 3 nonserious offending	0.06	0.17	0, 1.8	0.01	0.22	−1.00, 1.80	.16*	392 (196)
Wave 3 serious offending	0.04	0.13	0, 1.38	0.01	0.18	−1.13, 1.38	.11*	392 (196)
Wave 4 offending	0.03	0.10	0, 3	−0.01	0.12	−1.54, 0.62	.20*	400 (200)
Wave 4 nonserious offending	0.02	0.10	0, 1	−0.01	0.13	−1.00, 1.00	.27*	400 (200)
Wave 4 serious offending	0.03	0.12	0, 1.9	0.01	0.16	−1.88, 0.88	.07	400 (200)
Adolescent control variables								
Juvenile delinquency	0.23	0.28	0, 3	−0.01	0.28	−1.23, 1.59	.49*	522 (261)
Delinquent peers	5.43	4.99	0, 18	0.07	4.16	−16, 17	.65*	432 (216)
Social support	53.11	6.63	26, 65	0.26	6.98	−23, 26	.44*	422 (211)
Maternal attachment	18.73	1.81	6, 20	0.14	1.83	−8, 8	.47*	408 (204)
Maternal involvement	7.64	3.14	0, 18	−0.11	3.35	−11, 11	.42*	412 (206)
Maternal disengagement	21.61	6.76	12, 52	−0.01	6.91	−25, 24	.48*	398 (199)
Parental permissiveness	10.48	2.70	1, 14	0.04	2.71	−9, 9	.50*	428 (214)
Attachment to school	29.14	7.36	12, 56	−0.26	7.70	−30, 30	.45*	388 (194)
Low self-control	−0.01	0.34	−1.01, 1.55	−0.01	0.38	−1.23, 1.16	.40*	488 (244)
Demographics (Wave 1)								
Age	16.01	1.64	12, 20	—	—	—	—	—
Sex	0.52	0.50	0, 1	—	—	—	—	—
Race	0.63	0.48	0, 1	—	—	—	—	—

Note. Involvement with CJS items are all from Wave 3; all control variables are averaged from Waves 1 and 2 combined; low self-control was z transformed; "X-twin r": cross-twin (intraclass) correlation; *n* refers to individuals, while "pairs" refers to number of twin pairs; difference scores were created using a single-entered data set; sex: 0 = female, 1 = male; race: 0 = non-White, 1 = White; N_{MZ} = 282 (50% male pairs), N_{DZ} = 244 (53% male pairs). CJS = criminal justice system.

[a]Tetrachoric correlations used for cross-twin (X-twin) correlations.
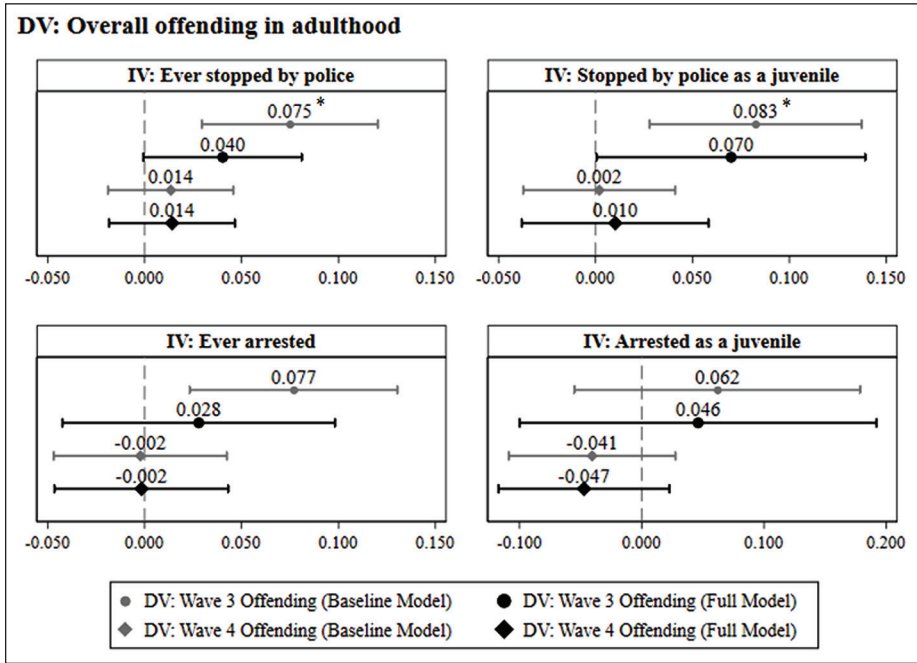
*p < .05, two-tailed test.

Figure 1. OLS regression models assessing the effect of involvement with the criminal justice system in adolescence on overall offending in adulthood using twin-difference scores.
Note. Each point estimate and corresponding 95% CI represent a separate model; the baseline models (n = 334-391) include only the CJS item difference score, whereas the full models (n = 193-224) include the CJS item difference score and all of the control variable difference scores (i.e., adolescent delinquency score, delinquent peers, social support, maternal attachment, maternal involvement, maternal disengagement, parental permissiveness, attachment to school, and low self-control); the CJS items are derived from Wave 3 while the control variables are averaged from Waves 1 and 2; robust standard errors were employed in all analyses; see Online Appendix C for full model information in table form. OLS = ordinary least squares; CI = confidence interval; CJS = criminal justice system; DV = dependent variable; IV = independent variable.
*The association remained statistically significant after Bonferroni correction for multiple comparisons (i.e., $p < .004$).

illustrated, in the baseline models the twin who reported ever being stopped by police ($b = .075$, $SE = .023$, 95% CI = [.296, .120], $p = .001$), being stopped by police as a juvenile ($b = .083$, $SE = .028$, 95% CI = [.028, .137], $p = .003$), or ever being arrested ($b = .077$, $SE = .027$, 95% CI = [.023, .131], $p = .005$) engaged in more antisocial behavior in early adulthood (Wave 3), relative to their co-twin who did not experience such contact with the CJS. However, these effects were all rendered null when the control variables were introduced.[8] In addition, the baseline and full models for the criminal conduct at Wave 4 were all statistically nonsignificant.

Figure 2 illustrates the results of the multivariate analyses assessing the influence of contact with the CJS on nonserious offending in early adulthood (Wave 3) and
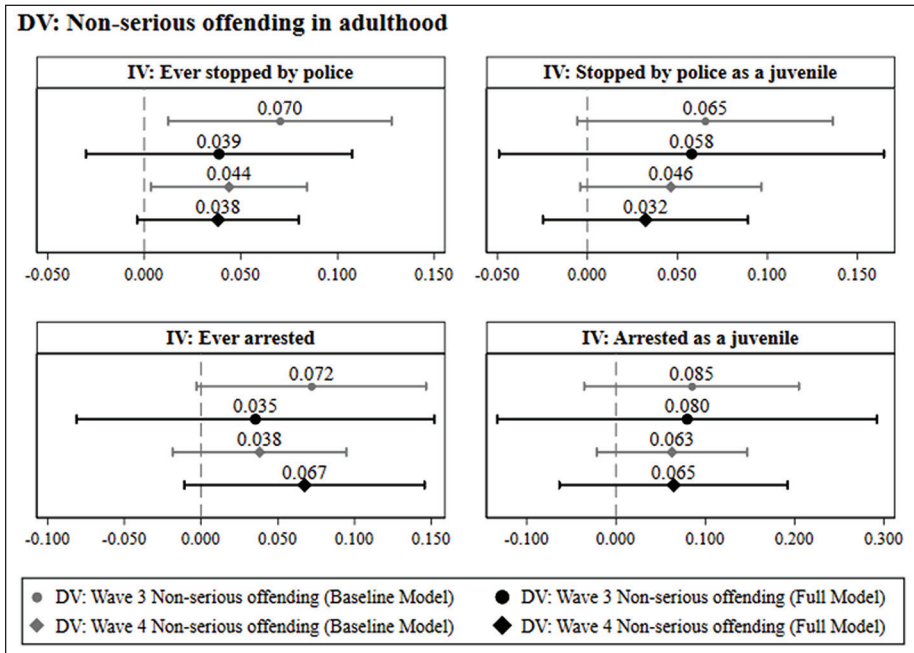
Figure 2. OLS regression models assessing the effect of involvement with the criminal justice system in adolescence on nonserious offending in adulthood using twin-difference scores.

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established adulthood (Wave 4). As indicated, the baseline models for both Wave 3 ($b = .070$, $SE = .029$, 95% CI = [.012, .128], $p = .017$) and Wave 4 ($b = .044$, $SE = .021$, 95% CI = [.003, .084], $p = .033$) reached statistical significance such that the twin who had ever been stopped by police engaged in more nonserious offending at both Waves 3 and 4 relative to their co-twin who had not been stopped by the police. However, these effects did not remain statistically significant once the control variables were introduced into the full models. No other baseline or full models examining nonserious offending in adulthood evinced statistical significance. Thus, the general pattern illustrated in Figure 2 indicates that within-twin pair difference in contact with the CJS did not influence within-twin pair difference in engagement in nonserious offending during adulthood.
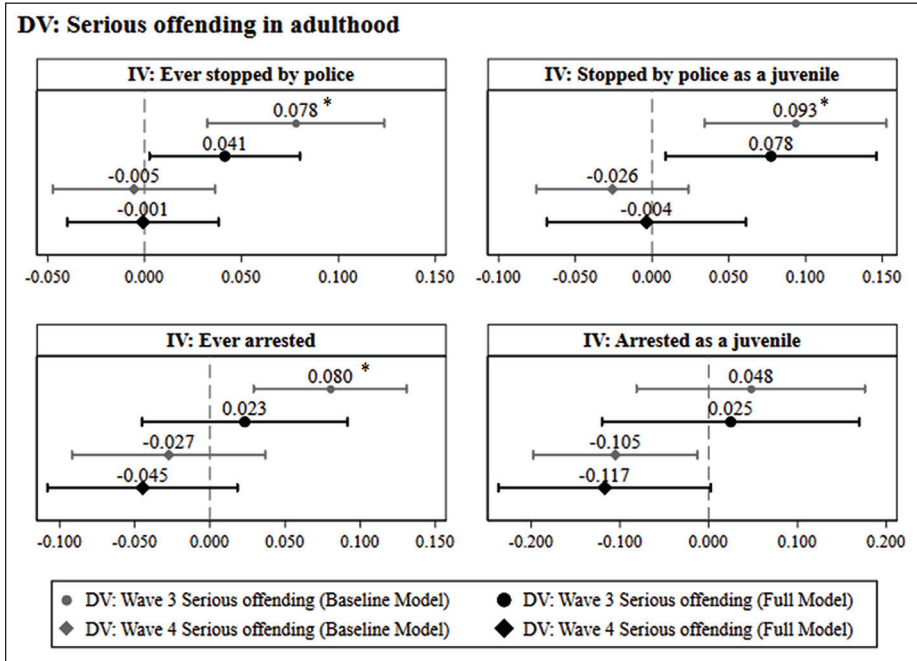
Figure 3. OLS regression models assessing the effect of involvement with the criminal justice system in adolescence on serious offending in adulthood using twin-difference scores.
Note. Each point estimate and corresponding 95% CI represent a separate model; the baseline models (*n* = 334-391) include only the CJS item difference score, whereas the full models (*n* = 193-224) include the CJS item difference scores and all of the control variable difference scores (i.e., adolescent delinquency score, delinquent peers, social support, maternal attachment, maternal involvement, maternal disengagement, parental permissiveness, attachment to school, and low self-control); the CJS items are derived from Wave 3 while the control variables are averaged from Waves 1 and 2; robust standard errors were employed in all analyses; see Online Appendix C for full model information in table form. OLS = ordinary least squares; CI = confidence interval; CJS = criminal justice system; DV = dependent variable; IV = independent variable.
*The association remained statistically significant after Bonferroni correction for multiple comparisons (i.e., *p* < .004).

The final figure, Figure 3, presents the results of the OLS regression analyses examining the effect of contact with the CJS on serious offending at Wave 3 (early adulthood) and Wave 4 (established adulthood). As illustrated, in the baseline models the twin who reported ever being stopped by police (*b* = .078, *SE* = .023, 95% CI = [.032, .124], *p* = .001), being stopped by police as a juvenile (*b* = .093, *SE* = .030, 95% CI = [.034, .153], *p* = .002), or ever being arrested (*b* = .080, *SE* = .026, 95% CI = [.029, .131], *p* = .002) reported increased engagement in serious offending in early adulthood (Wave 3) relative to their co-twin who had not experienced such contact with the CJS. In contrast, the single baseline model that was statistically significant at Wave 4 indicated that the twin who reported being arrested as a juvenile (*b* = –.105, *SE* = .047, 95% CI= [–.198,

–.013], $p = .026$) reported less engagement in serious offending in established adulthood (Wave 4) relative to their co-twin who had not been arrested as a juvenile. All effects observed in the baseline models were rendered nonsignificant in the full models save for ever being stopped by police and being stopped by police as a juvenile. More specifically, the twin who reported ever being stopped by police ($b = .041$, $SE = .020$, $\beta = .142$, 95% CI = [.002, .080], $p = .036$) reported more engagement in serious offending in early adulthood (Wave 3) even after inclusion of the control variables in the full model. The effect was similar for being stopped by police as a juvenile ($b = .078$, $SE = .035$, $\beta = .208$, 95% CI = [.009, .146], $p = .027$).[9] However, as noted, this effect did not continue into established adulthood (Wave 4).[10]

Discussion

While numerous criminological perspectives argue that official intervention on the part of the CJS can have a variety of unintended negative outcomes, labeling theory is perhaps most focused on the criminogenic effect of such contact. Despite a long history of empirical assessment, the claims put forth by labeling theorists have received mixed support. Recent scholarship has improved upon past designs and indicated that the criminogenic effect of contact with the CJS may have a causal influence on criminal continuity (e.g., Wiley et al., 2013). However, despite these improvements, methods such as PSM still suffer from potential genetic confounding. Consequently, the current study employed a genetically sensitive methodology—twin-difference scores—to assess the nonshared environmental effect of contact with the CJS over a time span of approximately 15 years which included multiple segments of the life-course.

Overall, the results indicated a general lack of long-term criminogenic effects of contact with the CJS once shared genetic and shared nongenetic factors were taken into account. While some statistically significant associations did arise, they were primarily limited to baseline (i.e., bivariate) models and the effects were rendered null when theoretically relevant control variables were included in the regression models. In addition, given that our analyses included multiple comparisons there remains the threat of increased likelihood of a Type I error. Thus, to prevent against such bias, we applied a post hoc Bonferroni correction resulting in a corrected alpha threshold of $\alpha = .004$ (original alpha / number of models per CJS item: .05 / 12 = .004). After application of this corrected alpha level, none of the observed associations in the full models remained statistically significant and only a few of the baseline models remained statistically significant (see Figures 1-3). As indicated by behavioral geneticists (e.g., McGue, Osler, & Christensen, 2010), when interpreting the results of twin-difference studies statistically significant associations may or may not be indicative of causality; null results of such models, however, do provide strong evidence that variance in the outcome of interest is not due to within-pair differences in the examined covariates but instead is likely due to shared genetic, shared nongenetic, or some other unmeasured factor of the nonshared environment. Thus, the overall null findings of the current study provide a strong challenge against the hypothesized long-term criminogenic effect of contact with the CJS.

Contextualizing the findings of the current study within the literature not only leads to a challenge of the assumptions underlying the main hypothesis of labeling theory but also highlights the need for genetically sensitive assessments of criminological theories. In addition, when coupled with the ever-accumulating literature indicating a substantial influence of genetic factors on phenotypic variance in antisocial behaviors (e.g., Beaver, 2008; Ferguson, 2010; Nedelec et al., 2016; Polderman et al., 2015), the findings of the current study point toward a likely genetic effect on primary deviance, contact with the CJS, and criminal continuity. Future assessments of the nexus represented by these variables, therefore, ought to account for the influence of genetic factors to avoid the risk of misspecification and confounding.

While the findings of the current study present a critique of the claim of a criminogenic effect of contact with the CJS, the conclusions should be tempered by three primary limitations. First, though the Add Health study was sampled in a way to be nationally representative, the kinship pair subsample was not. Thus, to the extent that the twins included in the analytical sample differ from the wider Add Health study on the variables of interest, generalizations should be made with caution.[11] Second, given that the Add Health is a community-based sample, the relative level of criminality (compared with a forensic or clinical sample, for example) is likely low. Nonetheless, labeling theory provides no indication that the purported criminogenic mechanisms associated with contact with the CJS should be differentiated for various segments of society. Even so, application of similar genetically sensitive models employed in the current study to samples with greater amounts of criminality would help add to the literature in this area. Third, while the twin-difference approach can completely account for shared genetic factors when the sample is limited to MZ twins, employing mixed-twin samples (as was done in the current study) renders observed associations subject to the potential influence of variance in genetic factors (McGue et al., 2010). As a reminder, given that DZ twins only share (on average) 50% of their distinguishing DNA, any observed phenotypic variance could still be due to the differentiated genetic material. Thus, all of the observed statistically significant associations in the current study are still subject to potential confounding due to genetic factors. However, given the general pattern of null findings in the current study, the threat of this limitation is likely minimal. Future research in this area could improve upon the current study by limiting the analytical sample to MZ twins.[12] In the end, the current study highlights the importance of accounting for genetic confounding when assessing criminology's core theories and lays a robust challenge to the primary hypothesis of labeling theory and recent assessments of its empirical validity.

Declaration of Conflicting Interests

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Notes

1. Note, however, that this claim varies whether analyses are conducted on a mixed-twin data set (i.e., MZ and DZ twins) or an MZ-only data set. Claims regarding the influence of nonshared environmental factors on a phenotype of interest are more valid when resulting from MZ twin-difference analyses (as 100% of both shared genetic and shared nongenetic factors are accounted for in the analyses). When DZ twins are included, a portion of the difference between twins could still be due to unobserved genetic factors given the reduced (average) shared genetic material; thus, claims regarding the influence of nonshared environmental factors must be tempered when mixed-twin data sets are employed.
2. We thank the Editors for emphasizing this point in their review of the manuscript.
3. A fifth wave of data collection is currently in progress.
4. This process was employed in creating all of the offending outcomes and averaged control variables in the current study and aligns with past analyses of the Add Health (e.g., Nedelec, Park, & Silver, 2016).
5. The reported alpha values (along with all reported results) are derived from analyses of the analytical twin subsample.
6. See Table B1 in Online Appendix B for the zero-order associations among the study variables.
7. The figures were created using Ben Jann's coefplot command in Stata 15 (Jann, 2014). For full model estimation information, please see Online Appendix C.
8. The coefficient for being stopped by police as a juvenile in the Wave 3 full model (i.e., the panel in the upper right quadrant of Figure 1; $b = .070$, $SE = .035$, 95% CI = [.0006, .1393]) was $p = .048$ and thus was considered nonsignificant.
9. As indicated in the "Discussion" section, these associations did not remain statistically significant after post hoc application of a Bonferroni correction (where, $\alpha = .004$).
10. The results for arrested as a juvenile for the full Wave 4 model (lower right quadrant in Figure 3) were $b = -.117$, $SE = .060$, $\beta = -.126$, 95% CI = [−.237, .002], $p = .055$ and were considered statistically nonsignificant.
11. Note, however, that numerous scholars have indicated that the twin subsample in the Add Health differs little on a wide variety of measures relevant to the current discussion (e.g., Barnes & Boutwell, 2013; Beaver, 2008; Nedelec et al., 2016). In addition, post hoc examination of average differences between the analytical twin sample of the current study and the rest of the Add Health sample (i.e., non-twin kinship pairs and singletons) revealed no substantive differences (see Table D1 in Online Appendix D for results of these analyses).
12. The current study included a mixed-twin analytical sample (i.e., MZ and same-sex DZ twin pairs) to maintain sufficient case count to conduct the multivariate analyses.

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